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The Gift of the
Association,
Prof Joseph Lovering,
Secretary,
2 April, 1858,
21 March, 1859.

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PROCEEDINGS

OF

THE AMERICAN ASSOCIATION

FOR THE

ADVANCEMENT OF SCIENCE.

ELEVENTH MEETING,

HELD AT MONTREAL, CANADA EAST,

AUGUST, 1857.

CAMBRIDGE:
PUBLISHED BY JOSEPH LOVERING.
1858.

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Gift of the Association.

Profr Lovering Secy

EDITED BY

JOSEPH LOVERING,

Permanent Secretary.

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ALLEN AND FARNHAM, PRINTERS.

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OFFICERS OF THE ASSOCIATION

AT THE

MONTREAL MEETING.

Prof. J. W. Bailey,* *President.*
Prof. Alexis Caswell, *Vice-President.*
Prof. Joseph Lovering, *Permanent Secretary.*
Prof. John LeConte, *General Secretary.*
Dr. A. L. Elwyn, *Treasurer.*

Standing Committee.

EX OFFICIO.

Prof. J. W. Bailey,*
Prof. Alexis Caswell,
Prof. Joseph Lovering,
Prof. John LeConte,

Dr. A. L. Elwyn,
Prof. James Hall,
Dr. B. A. Gould.

AS CHAIRMEN OF THE SECTIONAL COMMITTEES.

Prof. A. D. Bache,

Prof. J. D. Dana.

FROM THE ASSOCIATION AT LARGE.

Prof. J. W. Dawson,
Prof. Daniel Wilson,
Prof. Joseph Henry,

Prof. Benjamin Peirce,
Prof. Denison Olmsted,
Dr. P. N. Lynch.

---

* Died before the Meeting.

Local Committee.

Sir William E. Logan, *Chairman.*
A. N. Rennie, *Secretary.*

The Mayor of the City,
The President of the Board of Trade,
The President of the Natural History Society,
Sir Louis La Fontaine,
Hon. P. J. O. Chauveau,
Lieut. Col. Munro,
L. A. H. Latour,
H. H. Whitney,
Hon. Judge Day,
L. H. Halton, Esq.,
H. Lyman, Esq.,
A. A. Donan, Esq.,
Hon. P. D. Beaujeu,
Hon. James Ferrier,
Hon. George Moffat,
T. Sterry Hunt,
W. H. Hingston, M. D.,
D. Kinnear, Esq.

SPECIAL COMMITTEES.

A. Committees continued from Former Meetings.

1. *Committee to Memorialize the Legislature of Ohio on the Subject of a Geological Exploration of that State.*

Dr. J. P. Kirtland,
Gov. S. P. Chase,
Hon. Thomas Ewing,
Judge George Hoadley, Jr.,
Gen. C. B. Goddard,
Pres. J. W. Andrews,
Prof. Joseph Henry,
Prof. J. D. Dana,
Prof. Louis Agassiz.

2. *Committee to Report in Relation to Uniform Standards in Weights, Measures, and Coinage.*

Prof. A. D. Bache,
Prof. Joseph Henry,
Prof. J. H. Alexander,
Prof. John F. Frazer,

Prof. Wolcott Gibbs,
Prof. Benjamin Peirce,
Prof. John LeConte,
Prof. W. B. Rogers,
Dr. J. H. Gibbon,
Dr. B. A. Gould, Jr.,
Prof. J. Lawrence Smith,
Prof. R. S. McCulloch.

3. *Committee to Memorialize the Legislature of New York in Reference to Fish-Breeding.*

Prof. Louis Agassiz,
Prof. J. D. Dana.

B. New Committees.

1. *On Dr. Hare's Storm-Curves.*

Prof. A. D. Bache,
Prof. Joseph Henry,
Prof. Benjamin Peirce,
Prof. Arnold Guyot,
Prof. Joseph LeConte,
Prof. Denison Olmsted.

2. *On the Registration of Births, Deaths, and Marriages.*

Dr. James Wynne,
E. B. Elliott,
Franklin B. Hough.

3. *Committee on the Coast Survey.*

Judge J. K. Kane,
Gen. Joseph G. Totten,
Prof. Benjamin Peirce,
Prof. John Torrey,
Prof. Joseph Henry,
Prof. J. F. Frazer,
Prof. William Chauvenet,
Pres. F. A. P. Barnard,
Prof. John LeConte,
Prof. W. M. Gillespie,
Prof. Francis M. Smith,
Prof. W. H. C. Bartlett,
Prof. Wolcott Gibbs,
Prof. Stephen Alexander,
Prof. Lewis R. Gibbes,
Prof. Joseph Winlock,
Prof. James Phillips,
Prof. William Ferrel,
Prof. Edward Hitchcock,
Prof. James D. Dana.

4. *Committee to Audit the Accounts of the Permanent Secretary and the Treasurer.*

Prof. DENISON OLMSTED,	Dr. B. A. GOULD.

5. *Committee on Rules and Regulations.*

Dr. B. A. GOULD,	Prof. JOSEPH LOVERING.
Prof. A. D. BACHE,	

6. *Committee to act with the Standing Committee in Nomination of Officers for next Meeting.*

Section A.	Section B.
Prof. E. N. HORSFORD,	Prof. B. SILLIMAN, Jr.,
Professor SMITH,	Sir W. E. LOGAN,
Prof. JAMES ROBB,	Prof. EDWARD HITCHCOCK.
Dr. L. H. STEINER,	

OFFICERS OF THE BALTIMORE MEETING.

Prof. Jeffries Wyman, *President.*
Prof. John E. Holbrook, *Vice-President.*
Prof. Joseph Lovering, *Permanent Secretary.*
Prof. William Chauvenet, *General Secretary.*
Dr. A. L. Elwyn, *Treasurer.*

Standing Committee.

Prof. Jeffries Wyman,
Prof. John E. Holbrook,
Prof. Joseph Lovering,
Prof. William Chauvenet,
Dr. A. L. Elwyn,
Prof. Alexis Caswell,
Prof. John Leconte.

Local Committee.

Hon. Thomas Swann, *Chairman.*
Dr. Lewis H. Steiner, *Secretary.*

Gen. John Spear Smith,
Hon. Joshua Vansant,
J. F. Meredith,*
Dr. J. Hanson Thomas,
William H. Young, Esq.,
Dr. Christopher Johnston,
Augustus J. Albert, Esq.,
William McKim, Esq.,
Geo. W. Brown, Esq.,
Rev. Geo. W. Burnap, D. D.,
S. F. Streeter, Esq.,
Hon. S. O. Hoffman,
Dr. R. S. Stuart,
Rev. John G. Morris, D. D.,
Jonathan Meredith, Esq.,
Dr. Lewis H. Steiner,
William P. Smith.†

---

* In place of Hon. John P. Kennedy, who declined.
† In place of Dr. Campbell Morfit, who declined.

MEETINGS OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

Meeting.	Date.	Place.	President.	Vice-President.	General Secretary.	Permanent Secret'y.	Treasurer.
1st,	Sept. 20, 1848,	Philadelphia, Pa.,	W. C. Redfield, Esq.,		Prof. Walter R. Johnson,		Prof. J. Wyman.
2d,	Aug. 14, 1849,	Cambridge, Mass.,	Prof. Joseph Henry,		Prof. E. N. Horsford,		Dr. A. L. Elwyn.
3d,	March 12, 1850,	Charleston, S. C.,	Prof. A. D. Bache,*		Prof. L. R. Gibbes,*		Dr. St. J. Ravenel.*
4th,	Aug. 19, 1850,	New Haven, Ct.,	Prof. A. D. Bache,		E. C. Herrick, Esq.,		Dr. A. L. Elwyn.
5th,	May 5, 1851,	Cincinnati, Ohio,	Prof. A. D. Bache,		Prof. W. B. Rogers,	Prof. S. F. Baird,	Dr. A. L. Elwyn.
6th,	Aug. 19, 1851,	Albany, N. Y.,	Prof. L. Agassiz,		Prof. W. B. Rogers,	Prof. S. F. Baird,	Dr. A. L. Elwyn.
7th,	July 28, 1853,	Cleveland, Ohio,	Prof. Benj. Peirce,		Prof. J. D. Dana,	Prof. S. F. Baird,	Dr. A. L. Elwyn.
8th,	April 26, 1854,	Washington, D. C.,	Prof. J. D. Dana,		Prof. J. Lawrence Smith,	Prof. J. Lovering,	Dr. J. L. Leconte.*
9th,	Aug. 15, 1855,	Providence, R. I.,	Prof. John Torrey,		Prof. Wolcott Gibbs,	Prof. J. Lovering,	Dr. A. L. Elwyn.
10th,	Aug. 20, 1856,	Albany, N. Y.,	Prof. James Hall,		Dr. B. A. Gould, Jr.,	Prof. J. Lovering,	Dr. A. L. Elwyn.
11th,	Aug. 12, 1857,	Montreal, C. E.	Prof. J. W. Bailey,	Prof. Alexis Caswell.	Prof. John Leconte,	Prof. J. Lovering,	Dr. A. L. Elwyn.

* In the absence of the regular officer.

CONSTITUTION OF THE ASSOCIATION.*

OBJECTS.

THE Association shall be called "The American Association for the Advancement of Science." The objects of the Association are, by periodical and migratory meetings, to promote intercourse between those who are cultivating science in different parts of the United States; to give a stronger and more general impulse, and a more systematic direction, to scientific research in our country; and to procure for the labors of scientific men increased facilities and a wider usefulness.

MEMBERS.

RULE 1. Members of scientific societies or learned bodies having in view any of the objects of this Association, and publishing transactions, shall be considered members on subscribing these rules.

RULE 2. Collegiate professors, also civil engineers and architects who have been employed in the construction or superintendence of public works, may become members on subscribing these rules.

RULE 3. Persons not embraced in the above provisions may become members of the Association upon recommendation in writing by two members, nomination by the Standing Committee, and election by a majority of the members present.

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* Adopted August 25, 1856, and ordered to go into effect at the opening of the Montreal Meeting.

OFFICERS.

Rule 4. The officers of the Association shall be a President, Vice-President, General Secretary, Permanent Secretary, and Treasurer. The President, Vice-President, General Secretary, and Treasurer, shall be elected at each meeting for the following one; — the three first-named officers not to be reëligible for the next two meetings, and the Treasurer to be reëligible as long as the Association may desire. The Permanent Secretary shall be elected at each second meeting, and also be reëligible as long as the Association may desire.

MEETINGS.

Rule 5. The Association shall meet, at such intervals as it may determine, for one week or longer — the time and place of each meeting being determined by a vote of the Association at the previous meeting; and the arrangements for it shall be intrusted to the officers and the Local Committee.

STANDING COMMITTEE.

Rule 6. There shall be a Standing Committee, to consist of the President, Vice-President, Secretaries, and Treasurer of the Association, the officers of the preceding year, the permanent chairmen of the Sectional Committees, after these shall have been organized, and six members present from the Association at large who shall have attended any of the previous meetings, to be elected upon open nomination by ballot on the first assembling of the Association. A majority of the whole number of votes cast to elect. The General Secretary shall be Secretary of the Standing Committee.

The duties of the Standing Committee shall be, —

1. To assign papers to the respective sections.

2. To arrange the scientific business of the general meetings, to suggest topics and arrange the programmes for the evening meetings.

3. To suggest to the Association the place and time of the next meeting.

4. To examine, and, if necessary, to exclude papers.

5. To suggest to the Association subjects for scientific reports and researches.

6. To appoint the Local Committee.

7. To have the general direction of publications.

8. To manage any other general business of the Association during the session, and during the interval between it and the next meeting.

9. In conjunction with four from each Section, to be elected by the sections for the purpose, to make nominations of officers of the Association for the following meeting.

10. To nominate persons for admission to membership.

11. Before adjourning, to decide which papers, discussions, or other proceedings shall be published.

SECTIONS.

RULE 7. The Association shall be divided into two Sections, and as many sub-Sections as may be necessary for the scientific business, the manner of division to be determined by the Standing Committee of the Association. The two Sections may meet as one.

SECTIONAL OFFICERS AND COMMITTEES.

RULE 8. On the first assembling of the Section, the members shall elect upon open nomination a permanent chairman and secretary, also three other members, to constitute, with these officers, a Sectional Committee.

The Section shall appoint, from day to day, a chairman to preside over its meetings.

RULE 9. It shall be the duty of the Sectional Committee of each Section to arrange and direct the proceedings in their Section; to ascertain what communications are offered; to assign the order in which these communications shall appear, and the amount of time which each shall occupy.

The Sectional Committees may likewise recommend subjects for systematic investigation by members willing to undertake the researches, and to present their results at the next meeting.

The Sectional Committees may likewise recommend reports on particular topics and departments of science, to be drawn up as occasion permits, by competent persons, and presented at subsequent meetings.

REPORTS OF PROCEEDINGS.

RULE 10. Whenever practicable, the proceedings shall be reported by professional reporters or stenographers, whose reports are to be revised by the secretaries before they appear in print.

PAPERS AND COMMUNICATIONS.

RULE 11. No paper shall be placed in the programme, unless admitted by the Sectional Committee; nor shall any be read, unless an abstract of it has been previously presented to the Secretary of the Section, who shall furnish to the chairman the titles of papers of which abstracts have been received.

RULE 12. The author of any paper or communication shall be at liberty to retain his right of property therein, provided he declare such to be his wish before presenting it to the Association.

RULE 13. Copies of all communications, made either to the General Association or to the Sections, must be furnished by the authors; otherwise only the titles or abstracts shall appear in the published proceedings.

RULE 14. All papers, either at the general or in the sectional meetings, shall be read, as far as practicable, in the order in which they are entered upon the books of the Association; except that those which may be entered by a member of the Standing Committee of the Association shall be liable to postponement by the proper Sectional Committee.

Rule 15. If any communication be not ready at the assigned time, it shall be dropped to the bottom of the list, and shall not be entitled to take precedence of any subsequent communication.

Rule 16. No exchanges shall be made between members without authority of the respective Sectional Committees.

GENERAL AND EVENING MEETINGS.

Rule 17. The Standing Committee shall appoint any general meeting which the objects and interests of the Association may call for, and the evenings shall, as a rule, be reserved for general meetings of the Association.

These general meetings may, when convened for that purpose, give their attention to any topics of science which would otherwise come before the Sections.

It shall be a part of the business of these general meetings to receive the Address of the President of the last meeting; to hear such reports on scientific subjects as, from their general importance and interests, the Standing Committee shall select; also to receive from the chairmen of the Sections abstracts of the proceedings of their respective Sections; and to listen to communications and lectures explanatory of new and important discoveries and researches in science, and new inventions and processes in the arts.

ORDER OF PROCEEDINGS IN ORGANIZING A MEETING.

Rule 18. The Association shall be called to order by the President of the preceding meeting, and this officer having resigned the chair to the president elect, the General Secretary shall then report the number of papers relating to each department which have been registered, and the Association consider the most eligible distribution into Sections, when it shall proceed to the election of the additional members of the Standing Committee in the manner before described; the meeting shall then adjourn, and the Standing Committee, having divided the Association into Sections as directed, shall allot to each its place of meeting for the Session. The Sections shall then organize by electing their officers and their representatives in the Nominating Committee, and shall proceed to business.

PERMANENT SECRETARY.

RULE 19. It shall be the duty of the Permanent Secretary to notify members who are in arrears, to provide the necessary stationery and suitable books for the list of members and titles of papers, minutes of the general and sectional meetings, and for other purposes indicated in the rules, and to execute such other duties as may be directed by the Standing Committee or by the Association.

The Permanent Secretary shall make a report annually to the Standing Committee, at its first meeting, to be laid before the Association, of the business of which he has had charge since its last meeting.

All members are particularly desired to forward to the Permanent Secretary, so as to be received before the day appointed for the Association to convene, complete titles of all the papers which they expect to present during its meeting, with an estimate of the time required for reading each, and such abstracts of their contents as may give a general idea of their nature.

Whenever the Permanent Secretary notices any error of fact or unnecessary repetition, or any other important defect in the papers communicated for publication in the proceedings of the Association, he is authorized to commit the same to the author, or to the proper sub-committee of the Standing Committee for correction.

LOCAL COMMITTEE.

RULE 20. The Local Committee shall be appointed from among members residing at or near the place of meeting for the ensuing year; and it shall be the duty of the Local Committee, assisted by the officers, to make arrangements and the necessary announcements for the meeting.

The Secretary of the Local Committee shall issue a circular in regard to the time and place of meetings, and other particulars, at least one month before each meeting.

SUBSCRIPTIONS.

RULE 21. The amount of the subscription, at each meeting, of each member of the Association shall be two dollars, and one dollar in addition shall entitle him to a copy of the proceedings of the annual meeting. These subscriptions shall be received by the Permanent Secretary, who shall pay them over, after the meeting, to the Treasurer.

No person shall be considered a member of the Association until the subscription for the meeting at which he is elected has been paid.

RULE 22. The names of all persons two years in arrears for annual dues shall be erased from the list of members; provided that two notices of indebtedness, at an interval of at least three months, shall have been previously given.

ACCOUNTS.

RULE 23. The accounts of the Association shall be audited annually, by auditors appointed at each meeting.

ALTERATIONS OF THE CONSTITUTION.

RULE 24. No article of this constitution shall be altered, or amended, or set aside, without the concurrence of three fourths of the members present, and unless notice of the proposed change shall have been given at the preceding annual meeting.

RESOLUTIONS

OF A

PERMANENT AND PROSPECTIVE CHARACTER,

ADOPTED AUGUST 19, 1857.

1. No appointment may be made in behalf of the Association, and no invitation given or accepted, except by vote of the Association or its Standing Committee.

2. The General Secretary shall transmit to the Permanent Secretary for the files, within two weeks after the adjournment of every meeting, a record of the proceedings of the Association and the votes of the Standing Committee. He shall also daily, during the meetings, provide the chairmen of the two sectional committees with lists of the papers assigned to their Sections by the Standing Committee.

3. All printing for the Association shall be superintended by the Permanent Secretary, who is authorized to employ a clerk for that especial purpose.

4. The Permanent Secretary is authorized to put the proceedings of the meetings to press one month after the adjournment of the Association. Papers which have not been received at that time may be published only by title. No notice of articles not approved shall be taken in the published proceedings.

5. The Permanent Chairmen of the Sections are to be considered their organs of communication with the Standing Committee.

6. It shall be the duty of the Secretaries of the two Sections to receive copies of the papers read in their Sections, all subsections

included, and to furnish them to the Permanent Secretary at the close of the meeting.

7. The Sectional Committees shall meet not later than 9 A. M. daily during the meetings of the Association, to arrange the programmes of their respective Sections, including all subsections, for the following day. No paper shall be placed upon these programmes which shall not have been assigned to the Section by the Standing Committee. The programmes are to be furnished to the Permanent Secretary not later than 11 A. M.

8. During the meetings of the Association the Standing Committee shall meet daily, Sundays excepted, at 9 A. M., and the Sections be called to order at 10 A. M., unless otherwise ordered. The Standing Committee shall also meet on the evening preceding the first assembling of the Association at each annual meeting, to arrange for the business of the first day, and on this occasion three shall form a quorum.

9. Associate members may be admitted for one, two, or three years, as they shall choose at the time of admission, — to be elected in the same way as permanent members, and to pay the same dues. They shall have all the social and scientific privileges of members, without taking part in the business.

10. No member may take part in the organization and business arrangements of both the sections.

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OF THE

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FOR THE

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NOTE. — Names of deceased members are marked with an asterisk (*); and those of members who, in 1840, formed the original "Association of American Geologists," are in small capitals. The figure at the end of each name refers to the meeting at which the election took place.

A.

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Andrews, Israel W., Marietta, Ohio [11].
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Anthony, Henry, Providence [9].
Anthony, J. G., Esq., Cincinnati, Ohio [1].
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Arden, Thomas B., Garrison's P. O., Putnam Co., New York [7].
Armour, A. H., Toronto, Canada [10].
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Astrop, R. F., Crichton's Store, Burns Co., Virginia [7].
Atterbury, Rev. John G., New Albany, Indiana [11].
Austin, Samuel, Providence [9].

B.

Baby, G., Montreal [11].
Bache, Prof. Alexander D., Washington, D. C. [1].
Bache, Dr. Franklin, Philadelphia [1].
Bacon, Dr. John, Jr., Boston [1].
Bacon, William, Richmond, Berkshire Co., Massachusetts [7].
Bagg, Moses M., Utica, New York [4].
*Bailey, Prof. J. W., West Point, New York [1].
Baird, Prof. S. F., Washington, D. C. [1].
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Barber, Edgar A., Albany, New York [10].
Barlow, Thomas, Canastota, New York [7].
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Barnston, Dr. James, Montreal, Canada [10].
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Dwinelle, John H., Rochester, New York [11].
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E.

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Gale, Hon. Samuel, Montreal [11].
Gale, L. D., Washington, D. C. [8].
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Glynn, Com. James, U. S. N., New Haven, Connecticut [1].
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Grant, John M., Montreal [11].
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*Gray, Dr. James H., Springfield, Massachusetts [6].
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*Griffith, Dr. Robert E., Philadelphia [1].
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H.

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Haldeman, Prof. S. S., Columbia, Pennsylvania [1].
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*Hale, Dr. Enoch, Boston [1].
Hall, Archibald, Montreal, Canada [10].
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Hall, Joel, Athens, Illinois [7].
Hall, N. K., Buffalo, New York [7].
Hallowell, Benjamin, Alexandria, Virginia [7].
Hamlin, A. C., Bangor, Maine [10].
Hammond, Ogden, Charleston, South Carolina [3].
Hance, Ebenezer, Morrisville, Bucks Co., Pennsylvania [7].
Handy, Isaac W. K., Portsmouth, Virginia [10].
Hardcastle, Edmund L. F., U. S. A., Washington, D. C. [7].
Hare, Dr. Robert, Philadelphia [11].
*Harlan, Prof. Joseph G., Haverford, Pennsylvania [8].
*Harlan, Dr. Richard, Philadelphia [1].
Harrequi, Prof. José Salazar, Minerra, Mexico [10].
Harris, Ira, Albany [11].
*Harris, Dr. Thaddeus W., Cambridge, Massachusetts [1].
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Headlam, William, Jr., Albany [11].
Hedrick, B. S., New York [8].
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Wood, William, Portland, Maine [10].
Woodall, John W., Scarborough, England [11].
Woodbridge, George A., Nashville, Tennessee [10].
*Woodbury, Hon. L., Portsmouth, New Hampshire [1].
Woodman, John S., Hanover, New Hampshire [11].
Woolworth, Hon. S. B., Albany [10].
Worcester, Dr. Joseph E., Cambridge, Massachusetts [2].
Worthen, A. H., Warsaw, Illinois [5].
Wright, Charles, Wethersfield, Connecticut [10].
Wright, Chauncey, Cambridge, Massachusetts [9].
*Wright, Dr. John, Troy, New York [1].
Wurtele, Louis C., Lennoxville, Canada East [11].
Wurtz, Henry, Trenton, New Jersey [10].
Wyman, Prof. Jeffries, Cambridge, Massachusetts [10].
Wyman, Morrill, M. D., Cambridge, Massachusetts [11].
Wynne, Dr. James, New York [8].
Wynne, Thomas H., Richmond, Virginia [8].

Y.

Youmans, E. L., Esq., Saratoga Springs, New York [6].
Young, Prof. Ira, Hanover, New Hampshire [7].
Young, Hon. John, Montreal [11].

The above list contains ten hundred and fourteen names, of which sixty-eight are of deceased members.

MEMBERS ELECTED

AT

THE MONTREAL MEETING.*

Anderson, Rev. William, Montreal.
*Andrews, Israel W., Marietta, Ohio.
Atterbury, Rev. John G., New Albany, Ind.

*Baby, G., Montreal.
Baldon, A. S., M. D., Jacksonville, Florida.
Bancroft, Rev. Canon, Montreal.
Baudry, J. H., Montreal.
Baylis, James, Montreal.
Beale, James, M. D., Richmond, Va.
Belle, Charles E., Montreal.
Benedict, Thomas B., M. D., Trinity, La.
Benton, Charles, Oxbow, N. Y.
Berczy, William, Daillehaut, C. E.
*Bernard, A., M. D., Montreal.
Bertron, S. R., Port Gibson, Miss.
Bibaud, J. G., M. D., Montreal.
Bidwell, Rev. Walter H., New York.
*Billings, E., Montreal.
Blackmarr, Rev. Henry, Rochester, N. Y.
Blake, Rev. C. M., Chili.
Bonar, Rev. James B., Montreal.
Bradford, James C., M. D., Elyria, Ohio.
Braithwaite, Rev. Joseph, Chambly, C. W.
*Brewer, Fisk P., New Haven.
Briggs, George W., Pittsfield, Mass.
*Bristow, William, Montreal.
Brown, Robert, Jr., Cincinnati.
*Brush, George I., New Haven.
Brush, George S., Montreal.

Cady, Rev. Calvin B., Alburgh, Vt.
*Campbell, A. D., Montreal.
*Campbell, Prof. George W., Montreal.
*Campbell, W. D., Quebec.
Cameron, Hector, Toronto.
*Cartier, Hon. G. E., Montreal.
*Chamberlin, B. C., Montreal.
*Chapin, L. C., New Haven.
*Chapman, Prof. C. B., Madison, Wis.
*Chapman, Prof. E., Jr., Toronto.
Clark, John S., M. D., New Orleans.
Cleghorn, Rev. E. B., New Orleans.
*Cochran, J. W., New York.
*Collier, Prof. G. H., Wheaton, Ill.
*Collins, C., Carlisle, Penn.
*Comstock, J. C., Hartford, Conn.
Conckling, Fred. A., New York.
Copes, Joseph S., M. D., New Orleans.
Cordner, Rev. John, Montreal.
Cowan, J. P., M. D., Montreal.
*Cramp, J. M., Acadia College, N. S.
*Craik, Robert, M. D., Montreal.
*Croft, Prof. Henry, Toronto.
Cummings, William M., New York.
Cunynghame, Thurlow, Montreal.
Curry, Rev. W. F., Geneva, N. Y.
*Curtis, Jasper, St. Albans, Vt.
Cyr, Rev. Narcisse, Montreal.

Dalrymple, Rev. E. A., Baltimore, Md.
*Dalton, Prof. John C., Jr., New York.
Danforth, Edward, Clarence, N. Y.
*David, A. H., M. D., Montreal.

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* Those marked with an asterisk paid the assessment, and signed the Constitution without being formally elected.

*Davies, W. H. A., Montreal.
*Davies, W. H., Toronto.
*Day, Charles D., Montreal.
Day, John J., Montreal.
Dexter, G. M., Boston.
Dickson, Andrew, Kingston, Can.
*Disturnell, John, New York.
Doolittle, Rev. L., Lennoxville, C. E.
Dorr, Robert L., Danville, N. Y.
Dowie, J. Muir, Liverpool, Eng.
*Doyle, John P., Montreal.
*Dunkin, Christopher, Montreal.
Dwinelle, John H., Rochester, N. Y.

Eaton, A. H., New York.
*Edmondstone, William, Montreal.
Elin, Frederick, London.

Fairchild, Charles, Madison, Wis.
Fairhaven, James, Montreal.
*Fairly, Dr. David, Glasgow, Scot.
Fair, Samuel, M. D., Columbia, S. C.
Fay, Rev. Charles, St. Albans, Vt.
*Ferland, J. B. A., Quebec.
Ferrel, William, Nashville, Tenn.
*Feuchtwanger, Louis, M. D., N. Y.
Field, Roswell, Gill, Mass.
*Fisher, Prof. George P., New Haven.
*Fitch, Edward H., Ashtabula, Ohio.
Folsom, George, New York.
*Franklin, Danforth, Buffalo, N. Y.
Fraser, Hugh, Montreal.
*Fraser, Prof. W., M. D., Montreal.
French, J. H., Syracuse, N. Y.
*French, Rev. J. W., West Point.
*Fristoe, Edward T., Washington.
Frothingham, George H., Montreal.
Frothingham, Rev. Fredric, Portland, Me.

Gale, Hon. Samuel, Montreal.
*Garth, Charles, Montreal.
Geddes, Charles, Montreal.
Gibb, J. Campbell, Montreal.
Ginder, Henry, New Orleans.
*Gould, A. A., M. D., Boston.
*Grant, John M., Montreal.
*Grant, S. Hastings, New York.
Greene, F. C., M. D., East Hampton, Mass.
Greenshields, J B., Montreal.
*Guerin, Thomas, Albany.

Hager, Albert D., Proctorsville, Vt.
*Harris, Ira, Albany.
Harrison, B. F., Wallingsford, Conn.
*Harrison, Edwin, St. Louis, Miss.
Hart, Theodore, Montreal.
Headlam, William, Jr., Albany.
Henderson, Rev. Isaac J., New Orleans.
Heneker, William R. Sherbrooke, C. E.
Hickok, Rev. M. J., Scranton, Penn.
*Hilgard, Eugene W., Washington.
*Hill, Benjamin, Montreal.
*Hincks, Rev. William, Toronto, C. W.
Hitchcock, Charles H., Amherst, Mass.
*Hodgins, George, Toronto, C. W.
*Holmes, Prof. A. F., Montreal.
*Holmes, Benjamin, Montreal.
*Holton, L. H., Montreal.
*Holwell, W. A., Quebec.
*Homes, Henry A., Albany.
Hopkins, Rev. J. H., New York.
Houghton, George F., St. Albans, Vt.
*Howard, R. P., M. D., Montreal.
*Howe, Prof. Henry A., Montreal.
Hulburd, Hon. Calvin T., Brasher Falls, N. Y.
Humphrey, Frederick, Iowa City, Io.
*Humphreys, Capt. A. A., U. S. A., Washington.
Hunter, Andrew W., New Orleans.
Hunter, George W., New Orleans.
Hunt, Freeman, New York.
Hurlburt, Dr. J., Hamilton, C. W.
Huson, Calvin, Jr., Rochester, N. Y.

Inglis, Rev. David, Hamilton, C. W.

*Jack, Prof. W. B., Fredericton, N. B.
Janes, D. P., Montreal.
*Joseph, J. H., Montreal.

Keefer, Samuel, Brockville, C. E.
*Keefer, Thomas C., Hamilton, C. W.
*Kemp, Rev. Alexander, Montreal.
Kerr, Robert C., New Orleans.
*Kiershowski, A. E., Montreal.
*Kingston, G. T., Toronto.
*Kinnear, David, Montreal.
Kittredge, Dr. Josiah, South Hadley Mass.
Knox, Rev. John, Newtown, L. I.

*Lachlan, Major R., Cincinnati, Ohio.
Laflamme, G., Montreal.
*Laflamme, Prof. Rodolphe, Montreal.
*Latour, L. A. H., Montreal.
*Lawford, Frederick, Montreal.
Leckie, Robert, Montreal.
Lindsay, William B., New Orleans.
Little, Weare C., Albany.
Loranger, Hon. T. W., Montreal.
Louson, John, Montreal.
Lovelace, P. E. H., M. D., New Orleans.
*Lunn, William, Montreal.
*Lyman, Henry, Montreal.
*Lyman, Theodore, Montreal.

Lyon, Merrick, Providence.

*Mac Callum, Prof. D. C., Montreal.
Mackay, J. P., Montreal.
*Mason, Charles, Burlington, Io.
*Mathieson, Alex., D. D., Montreal.
McChesney, J. H., Springfield, Ill.
McClory, Rev. Henry, Warehouse Point, Conn.
*McCord, Hon. J. L., Montreal.
McCormick, Richard C., Jr., New York.
McElroy, John E., Albany.
McIlvaine, Rev. J. H., Rochester, N. Y.
Mackay, Joseph, Montreal.
Meilleur, J. B., M. D., Montreal.
Meredith, Edmund P., Toronto.
Merrill, Stephen, Charlestown, Mass.
Millar, G. M., Montreal.
Mills, Charles C., M. D., Richmond, Va.
*Miles, Prof. Henry H., Lennoxville, C. E.
Moffatt, George, Montreal.
Moffatt, J. O., Montreal.
*Mondelet, Judge Charles, Montreal.
Montgomerie, Hugh E., London.
Morris, J. R., Houston, Texas.
Morrow, E. G., Cambridge, Mass.
Morse, Charles M., Waterville, Me.
*Mowat, O., Toronto.
*Munro, Col. William, Montreal.
*Murphy, Edward, Montreal.
*Murray, Alexander, Woodstock, C. W.
Murray, Prof. David, Albany.
Myers, Gustavus A., Richmond, Va.

*Nason, Rev. Elias, Natick, Mass.
*Nault, Dr. J. Y., Quebec.
*Nelles, Pres. S. S., Victoria College.
*Newell, W. A., Trenton, N. J.
*North, Prof. Edward, Clinton, N. Y.

Paradis, H. C. A., M. D., Montreal.
Parker, Rev. Henry E., Concord, N. H.
*Paton, George, Galt, C. W.
Paton, John, Toronto.
*Patterson, Prof. J. W., Hanover, N. H.
Perkins, Louis, New London, Conn.
*Perkins, J. A., Montreal.
Phelps, Prof. W. F., Trenton, N. J.
Powell, Robert I., Illinois.
*Pynchon, Prof. Thomas R., Hartford, Conn.

Quincy, Edmund, Jr., Dedham.

*Ramsay, Andrew C., London.
Ramsay, J. K., Montreal.
Rauch, John H., M. D., Burlington, Io.
*Read, D. B., Toronto.
Redfield, Charles B., Albany.
Richards, Newton, New Orleans.
Robertson, Rev. W. H. C., Stamford, Conn.
*Robinson, W. B., Toronto.
Rockwell, John, Chicago, Ill.
Rogers, Prof. Fairman, Philadelphia.
*Roosevelt, Clinton, New York.
*Rose, Henry, Montreal.
Roy, Euclide, Montreal.
Russell, Andrew, Toronto.
Russell, Archibald, New York.
Ruttan, Allan, M. D., Newburgh, C. W.
*Ryan, Thomas, Montreal.
Ryerson, Rev. E., Montreal.

Sanderson, Rev. J. E., Montreal.
Scott, Rev. Joseph, Durham, C. E.
*Scott, Rev. William, Peterborough, C. W.
Scripps, John L., Chicago.
*Seemann, Berthold, Ph. D., London.
Seymour, M. H., Montreal.
Shelton, E. E., Montreal.
*Sheppard, William, Drummondville, Can.
*Sherwin, Thomas, Dedham, Mass.
Shotwell, Samuel L., Macedon, N. Y.
Skipwith, P. H., New Orleans.
Smith, Gen. John Spear, Baltimore.
Smith, John, Montreal.
*Smith, Spencer, St. Louis, Missouri.
Snodgrass, Rev. William, Montreal.
*Sola, Rev. Dr. A., Montreal.
Spink, William, Toronto.
Sprague, Daniel J., South Orange, N. J.
Sprague, Rev. Daniel G., South Orange, N. J.
*Streeter, S. F., Baltimore.
Stuart, Prof. A. P. S., Acadia, N. S.
Swann, Hon. Thomas, Baltimore.

*Taylor, J. W., Montreal.
*Taylor, Rev. William, Montreal.
*Taylor, Thomas M., Montreal.
Thickstun, I. F., Meadville, Pa.
*Thomas, Richard, Montreal.
Thomson, Rev. John, New York.
Tolderoy, James B., M. D., Fredericton, N. B.
*Treadwell, C. P., L'Original, C. W.
*Trudeau, Alexis, Montreal.
Tyler, Robert S., Montreal.

Ulffers, A. H., Springfield, Ill.
Ulrici, Richard W., St. Louis.

Van Cortlandt, Dr., Ottawa, C. W.

Wadsworth, Charles F., Geneseo, N. Y.
*Wallbridge, L. C., Toronto.
Warner, H. G., Rochester, N. Y.
Watertown, Charles, Wakefield, Eng.
Wetherill, Samuel, Bethlehem, Pa.
*Wheeler, T. B., M. D., Montreal.
White, Rev. Robert J. P., Chambly, C. E.
Whitney, H. H., Montreal.
Whitney, H. N., Montreal.
Whitney, John R., Philadelphia.
*Whittlesey, Charles C., St. Louis, Mo.
*Wilgress, George, Montreal.
*Wilkes, Henry, D. D., Montreal.
Williams, Henry W., M. D., Boston.
*Williamson, Prof. James, Kingston, Can.
*Woodall, John W., Scarborough, Eng.
*Woodman, John S., Hanover, N. H.
Woodruff, H. W. B., New York.
Wright, Joseph C., Oswego, N. Y.
Wurtele, Lewis C., Lennoxville, C. E.
*Wyman, Morrill, M. D., Cambridge, Mass.

*Young, Hon. John, Montreal.

The following paid the assessment, but were not formally elected, and did not sign the Constitution.

Beaubien, Dr.
Brydges, Sir Harford J.
Brinsmade, Dr. T. C.

Court, James.

Davis, W. H.
Dougall, J.

Fenwick, George E., M. D.

Gibb, J. C.

Hilton, W.

Jackson, Stephen.

Lench, Dr.

M'Gill, Hon. Peter.
Mackay, R.

Peel, T. B.
Perrault, O.

Walden, Dr. A. S.
Weaver, G. W.
Wykoffe, J. L.

The following signed the Constitution, but were not elected, and did not pay the assessment.

Abbott, J. H.

Bacon, J. R., Bahama.
Bancroft, Charles, St. Johns, C. E.

Dairs, William M., Cincinnati.
Douglas, Dr. George M., Quebec.
Douglas, James, Quebec.
D'Urban, William S. M., Montreal.

Harley, Lieut., 3d W. I. Regiment.

Lyman, S. J.

Poe, David A., Montreal.

Redpath, Peter, Montreal.
Robertson, Duncan, Montreal.
Rodden, William, Montreal.

Sandborn, Edwin D., Hanover, N. H.

Tompson, J. H., Lennoxville, C. E.

Whitney, N. S., Montreal.

The following have voluntarily withdrawn from the Association, or declined their election.

J. B. Angell.
J. H. Alexander.
William Goddard.
Roswell Field.
Owen Mason.
Dr. T. R. Ingalls.
Josiah W. Gibbs.

AN ADDRESS

IN COMMEMORATION OF

PROFESSOR J. W. BAILEY,

PRESIDENT OF THE ASSOCIATION,

DELIVERED BEFORE THE ASSOCIATION, AUGUST 19, 1857,

BY DR. A. A. GOULD.

MR. PRESIDENT, AND GENTLEMEN OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE, —

We are called upon, at this time, to advert to an event such as has not before occurred in the history of this Association during the seventeen years since its inception. He, who was elected at our last session to preside at this meeting, has in the mean time been taken from us. He, whom we all delighted to honor, though he sought not public honors from men, and who required no higher stimulus to his ambition nor reward of his toil than the satisfaction derived from the discovery and elucidation of the works and laws of Nature's God, has left this arena of trial and doubt to meet that God; and in His presence, where faith is turned to sight, we may well believe that he is unfolding with delight, unmingled with doubt, those wonderful works and perfectly harmonious laws which so engaged and delighted him on earth.

It is becoming that we should bestow a few moments in commemoration of his life and labors. There are others, who knew him much better, and were more conversant with his special studies than myself, to whom this office properly belonged; but as in another connection I had gathered some of the items of his history, and had noted the results of his scientific investigations, I have been requested to present them on this occasion.

Jacob Whitman Bailey was born April 29, 1811, in the old town of Ward, Mass. (now Auburn), at the residence of his grandfather, Rev. Isaac Bailey, the first minister of that town. In infancy he was removed to Providence, and there received his early education in the ordinary schools of that day. The limited resources of his family rendered it necessary that he should early engage in some employment; and, at the age of twelve years he was placed at a Circulating Library, where he attracted the attention of visitors by his studious devotion to books. At this time also he began a collection of shells and insects. During a visit of the West Point cadets at Providence, he became acquainted with some of the officers, and then decided to seek admittance to the Military Academy. He received an appointment as cadet in July, 1828, and graduated July, 1832. He was appointed second lieutenant in the artillery, April, 1833, and was promoted to first lieutenant, February, 1837. During this time he was stationed at Old Point Comfort, Bellona Arsenal, and Fort Moultrie. But with the development of his scientific tastes, military life had few attractions for him; and in 1839 he received the more congenial appointment of Professor of Chemistry, Mineralogy, and Geology in the United States Military Academy, which appointment he held, first as assistant, and soon afterwards as principal, until his death. He was married in 1835, and, with his wife and only daughter, then seventeen years of age, was on board the steamer Henry Clay, which took fire on the Hudson river, July 28, 1852. After having lowered them successfully to the water, and received from them the assurance of their safety, he proceeded to follow, when suddenly a volume of smoke and flame veiled them from his view, and they were lost. He had previously been subject to a bronchial affection and occasional spitting of blood, for which he had resorted to Florida the previous winter, with decided benefit. But the exertion and exposure on this occasion, together with the intensity of his bereavement, gave him a shock, from which he never rallied. With the exception of an occasional resort to the sea-shore during vacations, he was afterwards obliged to exclude himself almost entirely from society. His health steadily declined; and, feeling the certainty of the issue, he employed his leisure in arranging his papers, his microscopical collections, and his Algæ, so that they might be practically available to his successors. He died on the 27th of February last, at the age of forty-six.

As a man, he was remarkably unobtrusive and modest, gentle in his manners, truthful in his character, cordially beloved by all who had the good fortune to enjoy his acquaintance.

But it is more with his scientific position that we are concerned. His taste for science was very early developed. Beginning with botany and mineralogy, and passing from those to geology, chemistry, and microscopy, he traversed a large portion of the field of natural science. In the departments more especially relating to his position at West Point he held a high rank, and his publications show that he introduced many improvements in chemical manipulation. His correspondence, too, shows that he was extensively consulted by men of science on some of the most difficult points of analysis and general physics. His observations were always of the most careful and accurate character; and he early began the important practice of making notes of them, accompanied by delineations, leaving nothing to recollection or mere indefinite statements; thus having always at hand, permanent data for his subsequent papers. The volume containing these, which he denominated "Microscopic Sketches," is, of itself, a surprising evidence of his industry and skill. There are four hundred and fifty sheets, containing about three thousand sketches. By his great skill with the pencil he rendered himself independent of artists, an accomplishment, for the lack of which many of the best observers lose their labors. These drawings date back as far as 1838, twenty years ago, and enable us to trace out the course of his studies, as well as his wanderings; for wherever he went, his microscope or his collecting bottles went with him. At first, we have mostly sketches of vegetable and animal tissues, and occasionally an entire animal or plant. In January, 1839, while examining some aquatic plants, he perceived a curious object, a Gomphonema as it subsequently proved, which he did not understand. This excited his attention in that direction, and soon we find many others of the more common Diatoms delineated. In March, 1839, he sketched a new one, to which Ehrenberg gave the complimentary name *Stauronema Baileyi;* and finally he devoted himself with great zeal to the varied objects included under the general term Infusoria, and also to a department almost equally demanding his skill as a microscopist, namely, the Algæ. So far as the Infusoria were concerned, he stated, in 1843, that no one else in this country had studied them; and that it was almost impossible to procure any works relating to them. Ehrenberg's work he had not seen, though he modestly utters the thought that Ehrenberg might sometime see and correct his paper. He, however, gradually possessed himself of all the important works on those subjects, and became the active correspondent of Ehrenberg, Kützing, Agardh, Queckett, Ralfs, Harvey, Greville, DeBrébisson, Montagne, and very many others. Fossil deposits, mud, and guano, were collected from every quarter for investigation. The various exploring expeditions were laid under contribution; and more recently, the objects brought up on the sounding lead in the coast survey, and by Lieut. Berryman's line of soundings across the Atlantic, made in reference to the laying of the telegraphic cable, occupied his attention. In pursuing these examinations he found the relics from the bottom so well characterized in certain localities and at certain depths, that he suggested the possibility of being able, in some instances at least, to determine the safety or otherwise of a vessel, by an examination of the organisms brought up on the sounding lead, when prevented by darkness, snows, or fogs, from deciding by ordinary observations.

Not a little of the obligations of microscopists to Prof. Bailey is due for his labors to improve the microscope. At any rate, few among us have ventured upon the purchase of a valuable instrument, without first consulting him in reference to it, and perhaps taxing him with unwelcome negotiations; and his letters show that numerous applications of this kind must have been a most serious tax upon his time. It is said that his own early observations were made with globules of glass blown by himself. After he became possessed of a proper instrument, many modifications in the construction of the stage and its movements, and in other appendages, were made by him; and it is to his experience and scientific deductions, coupled with the genius and incomparable mechanical skill of Spencer, that we are indebted for the most powerful microscopes that have yet been made. His masterly and triumphant defence of them against the detractions of transatlantic pens, also exhibits his complete mastery of the subject. One of his last essays was to construct an Indicator, by means of which the place of any object on a slide might readily and certainly be found. No one, in looking at the card, would credit the labor and thought which he, in conjunction with his friends, Judge Johnson and Mr. Gavitt, bestowed upon it. Many futile efforts were made, and many quires were used in correspondence, before the accuracy of its measurements, and a method for the unerring application of it, were satisfactorily accomplished.

At a very early date, Prof. Bailey began to publish the results of his observations, — a duty too often neglected by scientific men. His published papers are very numerous, — more than fifty, — extending as far back as 1837, and up to his very last hours. They were, for the most part, very brief, free from ostentation, aiming to communicate facts in the simplest and most direct manner. In the words of his friend, Prof. Gray, "they are all clear, explicit, and unpretending as they are thorough; and every one of them embodies some direct and positive contribution to science." Most of them were terminated by a condensed statement of the general facts elicited, so as to show, at a glance, the subject, and result arrived at. They are mostly to be found in Silliman's Journal of Science, or in the Smithsonian Contributions to Knowledge, except one in the first volume of the Transactions of the Association of Geologists and Naturalists, which embodied his previous papers on the Infusoria of the United States, with additions, and which gave him at once a high position as a scientific naturalist.

His MICROSCOPICAL COLLECTION will constitute his most splendid monument. The slides, of which there are five hundred and fifty, are arranged in boxes in the form of octavos, of which there are twenty-four volumes. More than three thousand objects, fixed upon slides, are catalogued and noted with reference to Bailey's Indicator, thus enabling any one readily to find with certainty the identical specimens described by him. There are also very many other slides not included in the regular collection. Being objects either described by himself or given to him by other describers, this collection must always possess the highest authority, and must be our ultimate reference in all cases of doubt.

The COLLECTION OF ALGÆ is equally complete and authentic. It consists of thirty-two portfolios, containing about 4,500 specimens; and it may safely be said that few collections in the world are superior to it.

It is probably well known that Prof. Bailey bequeathed his

Microscopical Collection, his Collection of Algæ, his books on Botany and Microscopy, his Memoranda and his Scientific Correspondence, to the Boston Society of Natural History. While the Society intends to keep this bequest sacredly, it means also to make it as extensively useful as possible. I hesitate not, in behalf of the Society, to invite all who are pursuing similar researches to consult these collections, whenever convenient,—and to give assurance also, that any questions which may be solved by it may be freely addressed to the Society. A large collection of rough material for microscopic research, from many of the most interesting localities, is also in the possession of the Society, and will be distributed to microscopists and societies.

Such are some of the principal events in the history of our distinguished associate and President, and such are some of the accumulated fruits of his scientific labors,—labors which were performed in addition to the full duties of a professorship, executed with military precision and punctuality. He may well be styled the Ehrenberg of America, and has won for himself a place by the side of the most eminent microscopists and algologists of the old world. He will always stand as the father, in this country, of those branches of Natural History which relate to the *world of atoms*, and must for ever remain the standard reference here in relation to them. Let no man think lightly of them because they relate to little things, too small to be discerned by the unassisted eye. Are they not equally the handiwork of Him who made and sped the spheres, and formed man in his own image? And if he, by the microscope, demonstrated the vegetable structure of coal, illustrated the lowest habitable depths of the ocean, settled the nature of some of the important geological strata, and of the vast deserts otherwise deficient in geological indications,—questions of practical importance in our investigations of the crust of the earth, let him receive a corresponding rank with him who points the telescope to the mighty orbs above, determines their magnitudes and movements by scientific induction, and thereby enables us to determine our place upon that crust.

I cannot refrain from quoting, in conclusion, the words of an intimate friend in a letter to him, on learning of his appointment as President for this meeting. He says, "I am sure every one acquainted with what you have done for the advancement of science, American science, and American scientific character, will say, that no appointment, at the present time, could be more appropriate or just. I hope the great Disposer of events, whose minute works you have done so much to place before our eyes in all their exquisite beauty of form, of workmanship, and of adaptation, will give you yet many years to enjoy the honors you have so honestly acquired, and to add many more discoveries to those you have already secured." And may I not respond for you all, Would that this desire had been granted!

AN ADDRESS

IN COMMEMORATION OF

WILLIAM C. REDFIELD,

FIRST PRESIDENT OF THE ASSOCIATION.

DELIVERED BEFORE THE ASSOCIATION, AUGUST 14, 1857,
BY PROFESSOR DENISON OLMSTED, LL.D.

MR. PRESIDENT, AND GENTLEMEN OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE, — Since last we met, the Destroyer has been very busy in our ranks. Besides other beloved and respected associates, our earliest and our latest Presidents have suddenly vanished from our midst; — Redfield, who was the first to suggest the idea of the American Association on its present comprehensive plan, and the first to preside over its deliberations, and Bailey, who, we fondly hoped, would occupy the same distinguished position on the present occasion. From the vision of both, as we humbly trust, the veil which permits us here to see only through a glass darkly is removed; and the grand laws of Nature, and the infinitesimal, no less than the infinite in God's works, are revealed to them in the clear light of heaven.

With Mr. Redfield my acquaintance has been long and intimate. I was conversant with his earliest researches on the subject which is so closely associated with his name, and I have been constantly a witness of his untiring, self-sacrificing labors in the cause of science, through all the subsequent years of his life. I respected him as a man; I admired him as a philosopher; I loved him as a friend. We miss him here, always the earliest to come and the latest to depart. We miss his gentle tones, his kindly greetings. We miss still more the radiance which his clear mind cast upon our pathway up the hill of science. I am thankful for the opportunity of presenting before this learned assembly a synopsis of his scientific labors. Some brief notice also of his personal history may be acceptable, not only to satisfy the wishes of his friends, but for the benefit of his example, which, I trust, will especially commend itself to the self-taught votary of science, and to all who are engaged in the pursuit of knowledge under difficulties, both as an incentive and a model. A life passed in the ordinary walks of business or in the quiet of philosophical research, affords little of that romantic incident which lends a charm to biography; still we think the life of Mr. Redfield affords an interesting and instructive theme for contemplation in a threefold point of view,—as affording a marked example of the successful pursuit of knowledge under difficulties,—as happily illustrating the union in the same individual, of the man of science with the man of business,—and as exhibiting a philosopher, whose researches have extended the boundaries of knowledge, and greatly augmented the sum of human happiness.

WILLIAM C. REDFIELD was born at Middletown, Connecticut, on the 26th* of March, 1789. He was of pure English descent, both by the father's and mother's side. His father, from a natural love of adventure, chose in early youth a seafaring life, and afterwards followed the seas as a profession to the time of his death, which happened when this, his eldest son, was only thirteen years old. His early training, therefore, devolved chiefly on his mother, who was a woman of superior mental endowments and of exalted Christian character.

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* Printed erroneously in Silliman's Journal, "25th of March."

The slender pecuniary resources of the family would not allow young Redfield any opportunities of school education beyond those of the common schools of Connecticut, which, at that time, taught little more than the simplest rudiments — reading, spelling, writing, and a little arithmetic; and all access to the richer treasures of knowledge seemed to be forever denied him, when, at the early age of fourteen, he was removed to Upper Middletown, now called Cromwell, and apprenticed to a mechanic, whose tasks engrossed every moment of his time except a part of his evenings. These brief opportunities, however, he most diligently spent in the acquisition of knowledge, eagerly devouring every scientific work within his reach. He was denied even a lamp for reading by night much of the time during his apprenticeship, and could command no better light than that of a common wood fire in the chimney corner. Under all these disadvantages, it is evident that, before he was twenty-one years of age, he had acquired no ordinary amount and variety of useful knowledge. During the latter part of his apprenticeship, he united with other young men of the village in forming a debating society, under the name of the "Friendly Association," with which was connected a small but growing library. To this humble literary club Mr. Redfield always ascribed no small agency in inspiring him with a love of knowledge, and a high appreciation of its advantages; and during his future years, he nursed and liberally aided by his contributions this benefactor of his youth.

Fortunately for young Redfield, a distinguished and learned physician, Dr. William Tully, fixed his residence in the same village, and generously opened to him his extensive and well-selected library; and, what must have been equally inspiring to youthful genius, Dr. Tully furnished him with a model of an enthusiastic devotee to knowledge, and of a mind richly stored with intellectual wealth. The modest youth who first presented himself as a suppliant for the loan of a book from the Doctor's library was soon recognized as a congenial spirit, and was admitted to an intimate friendship, which lasted to the day of his death. Dr. Tully has favored us with the particulars of his first acquaintance with our friend. On his application for a book to occupy such moments as he could redeem from his daily tasks, the Doctor, being then ignorant of his acquirements or his taste, opened different cases of his library, submitting the contents of each to his selection. Among a great variety of authors, that which determined his choice was Sir Humphrey Davy's Elements of Chemistry. As this was one of the earliest systematic works that contained the doctrine of Chemical Equivalents, a subject then considered as peculiarly difficult, and one understood by few readers of the work, the Doctor had little expectation that this young inquirer after knowledge would either understand or relish it. In a short time he returned the book, and surprised the Doctor by evincing a thorough acquaintance with its contents, and expressing a high satisfaction, in particular with the doctrine of chemical equivalents, which, he said, he had then met with for the first time.

Some time before young Redfield reached the end of his apprenticeship, his widowed mother had married, and removed to the State of Ohio. He was no sooner master of his time than he set out on foot to pay her a visit in her new home, distant more than seven hundred miles. It was a formidable undertaking, at that early period before the age of steamboats and railways, and when a large part of the way was covered with dense forests, with hardly an open path even for the pedestrian. Stage-coaches, indeed, ran on the nearer portions of the route, but these were too expensive for the slender finances of our young adventurer. Accompanied, therefore, by two other young men, he shouldered his knapsack, and commenced the arduous journey. Every evening he noted down the incidents and observations of the day. This journal is now in my possession, and I have perused it with deep interest for the graphic sketches it contains of the countries he passed through, then mostly new settlements, and for the indications it affords of those powers of observation which afterwards led to the development of the laws of storms. The style of composition is far superior to what might reasonably have been expected from one who had enjoyed so few literary advantages, evincing two qualities for which Mr. Redfield was always distinguished — good sense and good taste. The sketches of Western New York, and of Northern Ohio, taken while the sites of Rochester and Cleveland were dark and gloomy forests, and Buffalo was a mere hamlet, possess no ordinary degree of historical interest. Instead of a "Lake Shore" road, traversed by the iron horse, as at present, our young pedestrians could find no better paths in which to travel over the southern side of Lake Erie than to course along the beach. Yet in twenty-seven days they made good their journey, having rested four days on the way, making an average of about thirty-two miles per day. After passing the winter with his friends in Ohio, he resumed his way homeward, on foot and alone, returning by a more southern route, through parts of the States of Virginia, Maryland, and Pennsylvania. We shall soon see to what valuable account he afterwards turned the observations made on these early pedestrian tours, in tracing the course, as well as originating the project, of a great railway connecting the Hudson and the Mississippi rivers.

Returning to his former home in 1811, Mr Redfield commenced the regular business of life. No circumstances could seem more unpropitious to his eminence as a philosopher, than those in which he was placed for nearly twenty years after his first settlement in business. A small mechanic in a country village, eking out a scanty income by uniting with the products of his trade the sale of a small assortment of merchandise, Mr. Redfield met with obstacles which, in ordinary minds, would have quenched the desire of intellectual progress. Yet every year added largely to his scientific acquisitions, and developed more fully his intellectual and moral energies. Meanwhile his active mind left its impress on the quiet community where he lived, in devising and carrying out various plans for advancing their social comfort and respectability, in the improvement and embellishment of their streets, school-houses, and churches, and in promoting the interests of the literary club, from which he himself, in early youth, had derived such signal advantages. From deep domestic trials which afflicted him about the year 1820, he had recourse for solace both to the word and the works of God. It was soon after one of the severest of these trials that his attention was first directed to the subject of Atlantic Gales.

On the 3d of September, 1821, there occurred, in the eastern part of Connecticut, one of the most violent storms ever known there, and long remembered as the "great September gale." Shortly after this, Mr. Redfield, being on a journey to the western part of Massachusetts, happened to travel over a region covered by marks of the ravages of the recent storm. He was accompanied by his eldest son, then a young lad, who well remembers these early observations of his father, and the inferences he drew from them. At Middletown, the place of Mr. Redfield's residence, the gale commenced from the south-east, prostrating the trees towards the north-west; but on reaching the north-western part of Connecticut, and the neighboring parts of Massachusetts, he was surprised to find that there the trees lay with their heads in the opposite direction, or towards the south-east. He was still more surprised to find, that, at the very time when the wind was blowing with such violence from the *south-east* at Middletown, a *north-west* wind was blowing with equal violence at a point less than seventy miles distant from that place. On tracing further the course and direction of prostrated objects, and comparing the times when the storm reached different places, the idea flashed upon his mind that the storm was a *progressive whirlwind.* A conviction thus forced upon his mind, after a full survey of the facts, was not likely to lose its grasp. Amid all his cares, it clung to him, and was cherished with the enthusiasm usual to the student of nature, who is conscious of having become the honored medium of a new revelation of her mysteries. Nothing, however, could have been further from his mind than the thought, that the full development of that idea would one day place him among the distinguished philosophers of his time. So little, indeed, did he dream of fame, that, for eight or nine years after the first conception of his theory, he gave little attention to the study of the phenomena of storms, but was deeply engrossed in other enterprises, which, although foreign to this subject, were alike evincive of his original and inventive turn of mind. Of these we may take a passing notice.

Before the scientific world, Mr. Redfield has appeared so exclusively in the character of a philosopher, especially of a meteorologist, that they have been hardly aware of the important services he has rendered the public in the character of *naval engineer*, particularly in the department of steamboat navigation. His attention was turned professionally towards this subject as early as the year 1820, when he became much interested in an experiment with a small boat propelled by an engine of new and peculiar construction, the invention of Franklin Kelsey, Esq., a townsman of his. Although the enterprise was not successful to the company, yet to himself it was not destitute of valuable results, as it was the occasion of his acquiring a more intimate knowledge of the properties of steam, of steam navigation, and of ship-building. On the ruins of that enterprise was erected another, which, after some vicissitudes, acquired a permanent success, and opened to him a sphere of professional labor, which constituted ever afterwards the leading object of his life, as a man of business. Several disastrous steamboat explosions had spread alarm through the community, and created a general terror of steamboats. Redfield was the first to devise and carry into execution the plan of a line of *safety barges* to ply on the Hudson between New York and Albany. The scheme was to construct a passenger boat to be towed by a steamboat at such a distance from it as to avoid all apprehension of danger to the passengers. Large and commodious barges were built, fitted up with greater taste and luxury than had at that time been exhibited by steamboats. With these were connected two large and substantial steamers; and in the excited state of the public mind, these safety barges became great favorites with travellers, especially with parties of pleasure. But our countrymen never hold their fears long; a short interval of exemption from steamboat accidents ended the excitement, while the greater speed attained by the ordinary boats, and the lower fare, gradually drew off passengers from the safety barges, until they could be no longer run with profit to the company, and were abandoned. But the idea was not without profit, for it suggested to him the system of *tow-boats* for conveying freight, which was established in the spring of 1826, and still continues under its original organization. The fleets of barges and canal boats, sometimes numbering forty or fifty, which make so conspicuous a figure on the Hudson river, were thus set in movement by Mr. Redfield, and for thirty years the superintendence of the line first established constituted the appropriate business of our friend. In its management he employed unwearied industry, superior mechanical genius for contriving expedients, and a knowledge of both the science and art of steam navigation, possessed by few men of business. Seldom have we seen the inductive philosopher so happily united with the practical engineer, each character borrowing aid from the other. I know not that any other man connected with the management of a steam-navigation concern, as his profession, ever carried into his business more of the spirit of true science; and it is chiefly on this account that I have thought it fitting to attend our associate into the familiar walks of business, for the purpose of seeing how compatible, and how productive of useful results, is the happy union, in the same person, of the philosopher and the man of business. No one else would have so thoroughly collected the statistics of the profession in this country, embracing all the facts relating to the explosion of steamboat boilers, as they successively occurred, the number of lives lost, the number of deaths by steam compared with those by lightning, and the number compared with those lost by other modes of travel. Moreover, while Mr. Redfield was diligently pursuing his daily business, and conducting with success the affairs of the "Steam Navigation Company," he was also collecting facts for the improvement of the art itself, or for securing the safety of passengers. He divised simpler, cheaper, and safer forms of apparatus than those in general use. He investigated the influence of legal enactments for regulating steam navigation, and pointed out to legislatures and governments the inefficacy or inexpediency of such enactments, and suggested the true measures to be taken to promote the convenience and secure the safety of the public. He addressed a series of letters through the public prints to one of our prominent naval commanders, setting forth the adaptedness of steam as an agent of national defence. He responded to the call of the Secretary of the United States Treasury to point out the causes of steamboat explosions, and to suggest the means of safety. Happy would it be, if in all the great operations of the mechanical arts, the true spirit of the philosopher were so fully conjoined with the practical knowledge and skill of the engineer. How rapid would be the improvement of the arts! How science and art would walk hand in hand, and mutually aid and illustrate each other!

We turn now to another subject which engaged the attention of Mr. Redfield, and brought into exercise his remarkable sagacity and forecast. He was the first to place before the

American people the plan of a system of railroads connecting the waters of the Hudson with those of the Mississippi. His pamphlet containing this project, issued in 1829, is a proud monument of his enlarged views, of his accurate knowledge of the topography of the vast country lying between these great rivers, of his extraordinary forecast, anticipating, as he did, the rapid settlement of the Western States, the magic development of their agricultural and mineral wealth, and the consequent rapid growth of our great commercial metropolis. The route proposed is substantially that of the New York and Erie Railroad, as far as this goes; but his views extended still further, and he marked out, with prophetic accuracy, the course of the railroads which would connect with the Atlantic States the then infant states of Michigan, Indiana, and Illinois. These, he foresaw, would advance with incredible rapidity the settlement of those regions of unbounded fertility, and would divert no small portion of the trade from the Mississippi to the great metropolis of the east.

It must be borne in mind that railroads, for general transportation, were unknown in this country until 1826, when the project of constructing the Albany and Schenectady Railroad was first entertained. As yet the advantages of railroads had not, with us, been practically demonstrated, and especially their advantages over canals were not generally understood or appreciated. At the moment when the Erie Canal, having just been completed, was at the summit of its popularity, Mr. Redfield set forth in his pamphlet, under nineteen distinct heads, the great superiority of railroads to canals, advantages which, although then contemplated only in theory, have been fully established by subsequent experience. He had even anticipated that after the construction of the proposed great trunk railway connecting the Hudson and the Mississippi, many lateral railways and canals would be built, which would bind in one vast network the whole great West to the Atlantic States. "This great plateau (says he) will indeed one day be intersected by thousands of miles of railroad communications; and so rapid will be the increase of its population and resources, that many persons now living will probably see most or all of this accomplished." How well has this remarkable prediction, uttered in 1829, when there was not a foot of railroad in all the country under review, been fulfilled, and how truly has it happened that many of the elder members of this association still live to witness its accomplishment!

The motives which impelled Mr. Redfield to spread this subject before the American people at that early day, when railroads were scarcely known in this country, were purely patriotic. He had no private interests to subserve in the proposed enterprise, and the whole expense of preparing and publishing two editions of the pamphlet embodying these enlarged and prophetic views was defrayed from his own limited resources.

In 1832, Mr. Redfield, in company with Mr. Morgan, civil engineer, reconnoitred the series of interior valleys through which the Harlem Railroad now runs, with a view to the establishment of the New York and Albany Railroad. He was instrumental in obtaining the charter of that road, and published a pamphlet entitled "Facts and Suggestions relating to the New York and Albany Railroad." About the same period, in connection with James Brewster, Esq., of New Haven, he explored the route of a railroad leading from New Haven to Hartford, which afterwards resulted in the construction of the Hartford and New Haven Railroad. As early as 1829, he addressed a memorial to the Common Council of the city of New York, asking permission to lay an experimental railroad in Canal street. The project of a railroad through one of the public streets of New York was, at that time, considered as chimerical, but time has developed the wisdom of the plan, and illustrated the sagacity and forecast that first devised it.

When the project of the Hudson River Railroad was started, he entered into it with his characteristic enthusiasm, and was a member of the board of directors which brought that road to its final completion. In the progress of the work he was deeply interested, frequently visiting all parts of the line, and at different periods examining on foot the entire road between New York and Albany. His associates of the board acknowledged themselves indebted to him for many valuable suggestions relating to its construction.*

But we turn from these noble enterprises, in which the philosopher and the engineer were happily united in the same individual, to the consideration of the great subject which, from this time, formed the leading object of his life, namely, to perfect his *theory of storms*. Nor do we turn away from great practical subjects to such as are merely speculative. The lives and property which Redfield's disinterested labors in behalf of steam navigation contributed to save, would, we believe, be of small amount compared with the sailors and ships which the rules founded on his theory of storms, when fully applied to practice, will save from shipwreck.

We have already seen that the attention of Mr. Redfield was first drawn to the subject of storms in the year 1821, by examining the position of trees prostrated by the great September gale, which passed over Connecticut and the western part of Massachusetts that year. Although he had never lost sight of the theory of storms, yet the multifarious business concerns which engrossed the greater part of his time for a number of years afterwards, prevented his bringing it distinctly before the public until the year 1831. I chanced, at that period, to meet him for the first time on board a steamboat on the way from New York to New Haven. A stranger accosted me, and modestly asked leave to make a few inquiries respecting some observations I had recently published in the American Journal of Science on the subject of hailstorms. I was soon sensible that the humble inquirer was himself a proficient in meteorology. In the course of the conversation, he incidentally brought out his theory of the laws of our Atlantic gales, at the same time stating the leading facts on which his conclusions were founded. This doctrine was quite new to me; but it impressed me so favorably that I urged him to communicate it to the world through the medium of the American Journal of Science. He manifested much diffidence at appearing as an author before the scientific world, professing only to be a practical man little versed in scientific discussions, and unaccustomed to write for the press. At length, however, he said he would commit his thoughts to paper, and send them to me, on condition that I would revise the manuscript and superintend the press. Accordingly, I soon received the first of a long series of articles on the laws of storms, and hastened to procure its insertion in the Journal of Science. Some few of the statements made in this earliest development of his theory he afterwards found reasons for modifying; but the great features of that theory appear there in bold relief. Three years afterwards he published, in the 25th volume of the same journal, an elaborate article on the hurricanes of the West Indies, in the course of which he gives a full synopsis of the leading points of his doctrine, as matured by a more extended analysis of the phenomena of storms than he had made when he published his first essay.

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* From the outset Mr. Redfield maintained that the low rate of fares at first adopted would prove inadequate to sustain the road, and published in the papers of that day a series of articles to show that the road could not be supported at a less rate than two cents per mile. These views met with much opposition at the time, not only from residents on the line of the road, but from members of the board of directors. But the result has proved the soundness of his judgment on that point.

Possibly some of those whom I have the pleasure to address may not have fully acquainted themselves with Redfield's theory of storms, and would desire to be informed of its leading principles. I understand this theory to be, substantially, as follows: —

That all violent gales or hurricanes are great *whirlwinds*, in which the wind blows in circuits around an axis either vertical or inclined; that the winds do not move in horizontal circles, as the usual form of his diagrams would seem to indicate, but rather in spirals towards the axis, a descending spiral movement externally, and ascending internally.

That the *direction of revolution* is always uniform, being from right to left, or against the sun, on the north side of the equator, and from left to right, or with the sun, on the south side.

That the *velocity of rotation* increases from the margin towards the centre of the storm.

That the whole body of air subjected to this spiral rotation is, at the same time, *moving forward* in a path, at a variable rate, but always with a velocity much less than its velocity of rotation, being at the minimum, hitherto observed, as low as four miles, and at the maximum forty-three miles, but more commonly about thirty miles per hour, while the motion of rotation may be not less than from one hundred to three hundred miles per hour.

That in storms of a particular region, as the gales of the Atlantic, or the typhoons of the China seas, *great uniformity exists in regard to the path pursued*, those of the Atlantic, for example, usually issuing from the equatorial regions eastward of the West India islands, pursuing, at first, a course towards the north-west, as far as the latitude of 30°, and then gradually wheeling to the north-east, and following a path nearly parallel to the American coast, to the east of Newfoundland, until they are lost in mid-ocean; the entire path when delineated, resembling a parabolic curve, whose apex is near the latitude of 30°.

That their *dimensions* are sometimes very great, being not less than one thousand miles in diameter, while their path over the ocean can sometimes be traced for three thousand miles.

That the *barometer*, at any given place, falls with increasing rapidity as the centre of the whirlwind approaches, but rises at a corresponding rate after the centre has passed by; and finally,

That the phenomena are more uniform in large than in small storms, and more uniform on the ocean than on the land.

These laws Mr. Redfield claims as so many *facts* independently of all hypothesis; as facts deduced from the most rigorous induction, which will ever hold true, whatever views may be entertained respecting the origin or causes of storms.

The *method* adopted by the author of this theory, in all his inquiries, — the method which first led him to the discovery of the whirlwind character of storms, and afterwards fully confirmed the doctrine, — was first to collect and then to collate as many records as possible of vessels that had been caught in the storm, in various parts of the ocean. The most laborious and profound investigation of this nature of which he has left us an example is in the case of the Cuba hurricane of October, 1844. First, he examined all accessible marine reports of vessels that had arrived in port after encountering the storm; secondly, he inspected the log-books of all such vessels, as far as was practicable, and carefully transcribed their records; and, thirdly, by an extended correspondence, he obtained a great number of written statements from shipmasters, who of all men would be the most accurate and vigilant observers. The different independent accounts obtained from these various sources amounted to no less than one hundred and sixty-four, all of which were reduced to the form of tables, containing the latitude and longitude of each vessel or place at the time of observation; the exact date and duration of the gale; the successive directions of the storm-wind; the state of the barometer; and, finally, every additional particular, that was deemed of the least importance in determining the peculiar characteristics of the storm. With these data before him, he spread out a marine chart, and having noted on it the position of each vessel and place with the direction and force of the wind, the plot itself proclaimed to the eye the whirlwind character of the storm; and the comparison of dates, and corresponding courses of the winds, and respective states of the barometer, showed the dimensions of the storm, its rotary and progressive velocities, its duration at any given place, and its various degrees of violence at different distances from the centre. In the character of the researches before us, conducted as they were, not in the shades of philosophic retirement and learned leisure, but in hours redeemed from the pressing avocations of an onerous and responsible business, or borrowed from the season allotted to sleep, we trace qualities of mind that belong only to the true philosopher.

The benevolent and practical mind of Redfield had no sooner established the laws of storms than it commenced the inquiry, what rules may be derived from it, to promote the safety of the immense amount of human life and of property that are afloat on the ocean, and exposed continually to the dangers of shipwreck; in this imitating our Franklin, who, as soon as he had discovered the identity of lightning with the electricity of our machines, hastened to the inquiry, How may we so apply our knowledge of the laws of electricity as to disarm the thunderbolt of its terrors? We might pursue the comparison and say, that, as every building saved from the ravages of lightning by the conducting rod is a token both of the sagacity and the benevolence of Franklin, so every vessel saved from the horrors of shipwreck by rules derived from these laws of storms, is a witness to the sagacity and benevolence of Redfield. Other writers on the laws of storms, especially Reid and Piddington, have lent important aid in establishing rules for navigators, until it is now easy for the mariner, by the direction in which the gale strikes the ship, to determine his position in the storm, and the course he must steer in order to escape from its fury. Nor are testimonies wanting of the successful application of these rules. The most accomplished navigators (we might instance, particularly, Commodores Rodgers and Perry, and Commander Glynn, of the U. S. Navy) have testified, that, within their knowledge and in some cases within their own observation, many ships have owed their deliverance from the perils of shipwreck to a faithful observance of the rules derived from Redfield's theory of storms. In no department, perhaps, of the studies of nature, have mankind been more surprised to find things governed by fixed laws, than in the case of the winds. It is now rendered in the highest degree probable, that every breeze that blows is a part of some great system of aerial circulation, and helps to fulfil some grand design. "Inconstant as the winds" has long been a favorite expression to denote the absence of all uniformity or approach to fixed rules; but the researches of the meteorologists of our times force on us the conclusion, that the winds, even in the violent forms of hurricanes and tornadoes, are governed by laws hardly less determinate than those which control the movements of the planets.

It has been often noticed in the history of science and the arts, that great discoveries and inventions spring forth simultaneously from different independent sources. Thus the discovery of oxygen gas, the greatest single discovery in chemistry, was made almost at the same moment by Priestley in England and Scheele in Sweden; and the method of fluxions, or the infinitesimal calculus, was invented at nearly the same time by Newton and Leibnitz. Such discoveries and inventions are the true resultant of innumerable forces, which, at that moment, and never until then, since the origin of time, all conspired. It is remarkable that the idea that great storms are progressive whirlwinds was, for the first time, embraced nearly at the same instant by Redfield and Dové, although the conclusion was arrived at by totally different methods of investigation. Mr. Redfield says, in a note to his paper on the Cuba hurricane, published in 1846, that it was not until seven years after the publication of his theory of the rotary and progressive character of storms, that he became acquainted with the suggestions and opinions of Col. Capper; and with the particular views and elucidations published by Professor Dové in his paper on Barometric Minima, found in Poggendorff's Annalen for 1828. To all who were personally acquainted with Redfield, it would be quite unnecessary to adduce any other evidence than his simple declaration, of the perfectly original and independent character of his theory of the laws of storms. But we might refer to the circumstances under which it was conceived, when he was far removed from all libraries, and all intercourse with the scientific world; and as respects Dové in particular, whose essay was communicated to the public in 1828, it may be said, that at that period there was scarcely a copy of Poggendorff's Annalen (in which Dove's essay appeared) in the United States; and being in the German language, nothing could be more improbable than that its contents were then known to Redfield. In 1838, our friend found, to his great joy, a most able ally in Col. Reid of the Royal English Engineers, then stationed in the island of Barbadoes. The earliest inquiries of Col. Reid were based on a violent hurricane, which occurred in that island in the year 1831. Searching for accounts of previous storms, he met with nothing satisfactory until he fell in with Redfield's earliest paper, respecting the September gale of 1821, published in the American Journal of Science. With the view of testing Redfield's doctrines, he submitted to the closest scrutiny the records which the Barbadoes storm had left of its ravages, — an investigation which ended in a perfect conviction that this storm was a progressive whirlwind. A friendly correspondence was shortly afterwards opened between these two congenial spirits, which resulted in an intimacy, unbroken except by the hand of death. Commodore Perry, in the recent Report of his Japan Expedition, thus expresses himself in an introductory note to Mr. Redfield's Essay (the latest of his published works) on the Cyclones of the Pacific, addressed to Commodore Perry, and forming a part of his volume. "It was my good fortune (says the Commodore) to enjoy, for many years, the friendly acquaintance of one as remarkable for modesty and unassuming pretensions, as for laborious observation and inquiry after knowledge. To him and to Gen. Reid of the Royal Engineers of England (now Governor of Malta) are navigators mainly indebted for the discovery of a law which has already contributed, and will continue to contribute, greatly to the safety of vessels traversing the ocean. It is true that subsequent writers have furnished additional information on this subject; but to Redfield and Reid should be ascribed the credit of the original discovery of this undeniable law of nature, and its application to useful purposes; and there can be nothing more beautiful, as illustrative of the character of these two men, than the fact, well known to myself, that notwithstanding their simultaneous observations and discoveries, in different parts of the world, neither claimed the slightest merit over the other, but each strove to give to his co-worker in research the meed of superior success in the great object of their joint labors; and thus, without ever meeting, a strong friendship was formed between them, growing out of congenial aspirations for an honorable fame, and mutual admiration of the generous and enlightened views exhibited by each other; and this ennobling feeling was kept alive to the last by friendly correspondence."

The idea of whirlwinds is indeed much older than Redfield or Reid, being as old as the writings of the Psalmist and the Prophets; and we safely admit further, that the doctrine of ocean gales being sometimes of a rotary character had been hinted at by several writers, as hints of such a principle as gravitation had long preceded the investigations of Newton; but the honor of having established on satisfactory evidence the rotary and progressive character of ocean storms, and determining their modes of action or laws, it is due alike to the memory of the departed, and to our country's fame, to claim for WILLIAM C. REDFIELD.

Back of the laws that govern these ocean gales, as first determined by Redfield and confirmed by Dové, Reid, Piddington, Thom, and other well-known writers, lies a more profound inquiry, How are these laws themselves to be accounted for? What sets the storm in motion, and gives it the whirlwind character, and at the same time carries it forward, and in so definite a path? What makes it revolve always from right to left on the north side of the equator, and from left to right on the south side? Why does its violence increase towards the centre of the storm, and why is its force there so tremendous? Laws, it must be remembered, are facts, and merely express the modes in which nature acts; they are themselves phenomena to be accounted for. To which of the ultimate causes of physical phenomena is their origin in the present case to be traced? Is it heat? Is it electricity? Is it gravity? Is it connected in some way with the grand system of planetary motion? Questions of this kind were pressed on Mr. Redfield from various sources by those who assailed his theory. At first he declined any attempts at their solution. He claimed that the whirlwind character of storms, and the laws which he had assigned to them, are matters of fact, as established not only by himself, but also by Reid, Milne, Dové, and Piddington; that, never having attempted to establish a theory of winds, nor the origin or first cause of storms, he had no occasion to go into these inquiries, but had long held the proper inquiry to be, *What are storms?* not *How are storms produced?* He, however, incidentally, at different times, indicated his opinions on the ultimate causes of storms. Electricity, Redfield entirely rejected as an agent in the production of winds and storms, considering its presence and development rather as a consequence than as a cause of atmospheric changes. To heat he assigned only a limited and local effect, denying its agency in producing either the great and established movements of the atmosphere, or the extraordinary commotions which constituted the chief objects of his study, hurricanes and tempests. But he considered what he called the "dynamics of the atmosphere" as connected with, and resulting from, the diurnal and annual motions of the earth. While, from the first, I have heartily embraced Redfield's doctrine, that ocean gales are progressive whirlwinds, and have further fully believed that he had established their laws or modes of action on an impregnable basis, a regard to truth and candor obliges me to say, that I have never been a convert to his views respecting the ultimate causes of storms, especially so far as he assigned for these causes what he denominates the "diurnal and orbitual motions of the earth," but his notions on this point have always appeared to me unsatisfactory. Nor, while I have been impressed with the belief that *heat* is, in general, by far the most influential of all natural agents in destroying the equilibrium of the atmosphere, and of causing its motions, both in established currents as the trade winds and the monsoons, and in its violent commotions as in hurricanes and tornadoes, yet I am compelled to think that but little progress has yet been made in determining its *modus operandi*, or in tracing the *connection* between changes of temperature and the actual phenomena of winds and storms: — why, for example, the Atlantic gales originate where they do, in the tropical regions; why they first pursue a path to the north-west as far as the latitude of 30°, and then gracefully wheel in parabolic curves towards the north-east, and pursue this course for the remainder of their way; why they revolve on their axes, and always in one direction; whence they acquire so tremendous a force, especially towards the central parts; why the barometer is so low in the centre and so high on the margin of the storm. These, and various other points connected with the whirlwind character of storms, seem to me to have met hitherto with but a partial and doubtful solution. The laws constitute the true theory of storms; the rest is yet hypothesis.

Various writers have severally displayed great ingenuity and profound knowledge of atmospheric phenomena in their endeavors to solve these problems, but with respect to the causes which lie back of the laws of storms, we still remain to a great degree in ignorance. Each of the combatants appears to me to be more successful in showing the insufficiency of the other's views, than in establishing his own. With respect to him who is more particularly the subject of my remarks, whose logical powers I have always admired, I have almost regretted that he did not adhere to the ground he originally took, namely, that he had not undertaken to explain the reason *why* the winds blow, but only to show *how* they blow. So far was matter of fact; all beyond was hypothesis. His facts are impregnable; his hypothesis doubtful. The conclusions derived legitimately from these facts constitute the laws of storms; and being, as we believe, like the other laws of nature, immutable, the name indissolubly associated with their discovery acquires a fame alike imperishable. Redfield might therefore have safely stopped where Newton stopped. "Newton (says one of his biographers) stopped short at the last fact which he could discover in the solar system — that all bodies were deflected to all other bodies, according to certain regulations of distance and quantity of matter. When told that he had done nothing in philosophy; that he had discovered no cause; and that, to merit any praise, he must show how this deflection was produced; he said, he knew no more than he had told them; that he saw nothing causing this deflection, and was contented with having described it so exactly that a good mathematician could now make tables of the planetary motions as accurate as he pleased, and hoped in a few years to have every purpose of navigation and philosophical curiosity completely answered."

Various other contributions to science of our departed friend must, for want of space, be passed by with hardly a notice.

Such are his published meteorological essays,* his reports of meteorological observations, which contain many original hints of much value, his paper on the currents of the Atlantic, and his researches in geology, which occupied much of his attention during the latter years of his life; all of which speak the skilful observer, the judicious philosopher, the lover of science, the lover of his country and of his kind. His meteorological researches, although they engrossed a large share of the hours he could redeem from the urgent claims of business, did not prevent his taking a strong interest in other branches of science. He attentively watched the progress of knowledge in various departments, but geology had for him special attractions. His powers of observation were early employed, even in his pedestrian tour to Ohio in 1810, in noting facts which appeared to him then to be unaccounted for, but which the progress of the science has since fully explained. In the meetings of the American Association he was an attentive listener to the geological papers, and frequently took part in the discussions which they called forth, exhibiting a thorough acquaintance with the subjects under consideration. The phenomena of the drift period, as evincive of glacial action in various forms, had deeply interested him; and he had collected and closely studied the shells of recent species, which, in the vicinity of New York, are found beneath the deposits of drift. His published geological papers, however, relate chiefly to the sandstones of Connecticut and New Jersey, particularly to their fossils, their ripple-marks, and their rain-drops. His residence in early life was within sight of the extensive quarries of this kind of sandstone, at Portland, Connecticut, and his frequent visits afterwards to that region afforded him opportunity for close observation. In December, 1836, his son, Mr. John H. Redfield, who inherits much of the scientific taste of his father, described † some of the fossil fishes from this locality, and showed that their structural affinities indicated for the so-called "New Red Sandstone" a higher position than had previously been assigned to it. Mr. Redfield pursued the track thus opened by his son, and published in the American Journal of Science descriptions of several new species of Ichthyolites. The last paper which he read before the American Association was upon the geological age of the sandstones of Connecticut and New Jersey, and the cotemporaneous deposits in Virginia and North Carolina. He proposed for them the denomination of the *Newark group*, and showed that the Ichthyolites contained in them pointed unerringly to the Jurassic period. In the course of these investigations he had given close study to the subject of fossil fishes, and had formed a collection of them, probably unequalled in this country, with special reference to a contemplated monograph of the Ichthyolites of the Newark group.

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* Originally prepared for Blunt's Coast Pilot.

† Annals Lyc. Nat. History, New York, vol. iv.

In 1839, Yale College conferred on Mr. Redfield the honorary degree of Master of Arts, and the enlarged sphere in which his labors for the promotion of science and the good of his fellow men were known and appreciated, was evinced by his election into many learned societies in his own and foreign countries.

Three distinguishing marks of the true philosopher met in William C. Redfield,—originality to devise new things; patience to investigate; and logical powers to draw the proper conclusions. The impress of his originality he left, in early life, upon the village where he resided; he afterwards imprinted it still deeper on his professional business, as naval engineer; and most of all on his scientific labors, his observations, and his theories. "Patient thought" was the motto of Newton, and in this attribute Redfield was eminently distinguished. In collecting facts bearing upon his main purpose, and in submitting them to severe and long-continued comparison, he has illustrated this quality in its highest forms, as his laborious investigations of the phenomena of hundreds of storms most fully evince. Originality to invent without patience to investigate, leads to hasty and wild speculations; but united, they lay the deep foundations for a severe logic. His powers of reasoning have always appeared to me to be of high order, and he has been fitly characterized by another eminent writer* on the laws of storms as the "clear-headed" Redfield. Opinions which he had thus formed, after an extensive and patient investigation of the facts and a severe process of reasoning, he held with great tenacity. But though firm, he was not obstinate. *Obstinacy* we define to be an unyielding adherence to our opinions because we have adopted them; *firmness*, a similar adherence to our opinions because we believe them to be right.

Few men have given more signal proofs of an original, inherent love of knowledge. Whether we contemplate the apprentice boy after the toils of the day, seeking for knowledge by the dim light of an open fire; or the father of a young family, through dark scenes of domestic affliction and mournful bereavements, still adding largely, year by year, to his intellectual stores; or the man of business in the whirl of the great metropolis, loaded with onerous and responsible cares, giving every interval of leisure, and the seasons chiefly employed in pleasure or repose, to the study of the laws of nature; or if permitted, as has been my privilege, to be a guest at the house fitted up to be the retreat of his old age, we see the library, the collections of natural history, the many sources of high mental enjoyment, which, in the period gained at last of ease and affluence, distinguish the different apartments of his dwelling; or, finally, whether we call to mind the ever increasing interest with which he attended the meetings of the American Association for the Advancement of Science, and the delight which he experienced in the society of learned men, we observe in all, a mind in love with truth, ever searching and ever expanding. In society he was courteous, sincere, upright, and benevolent; in his family tender, affectionate, wise in counsel, and pure in example; in all his walk and conversation, and especially in the church of God, a devout and humble Christian.

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* Reid.

As the evening of life was passing thus serenely, it hastened to a peaceful close. Mr. Redfield's health had been generally good during his later years, and had seemed particularly so in the early part of the winter which proved his last. On the first of January, he made his usual calls on his friends, and the cheerfulness and vivacity of his manners and healthful expression were never more remarkable. Near the last of January he was seized with alarming symptoms, which indicated effusion in the chest. His disease made rapid and sure progress. The last book which had engaged his attention, previous to his illness, was Dr. Kane's recent Narrative of his Arctic Expedition; and his own feverish dreams, during the earlier nights of his sickness, were confusedly identified with the toils, the difficulties, and the sufferings of that heroic commander and his brave companions. With a general tendency to delirium were mingled intervals of calmness, and throughout his illness his countenance would light up with the smile of affection, as he recognized the relations and friends around him. From the first he entertained but slight hopes of recovery; but as the crisis drew near, his mind was at peace, and in calm resignation to the will of his Maker, and in the full exercise of Christian faith he gently breathed his last.

Happy if we who have so long journeyed with him in the delightful walks of science may enjoy an evening as serene, and find its close as peaceful.

PROCEEDINGS

OF THE

MONTREAL MEETING, 1857.

COMMUNICATIONS.

PART I.

A. MATHEMATICS AND PHYSICS.

I. MATHEMATICS AND ASTRONOMY.

1. ON THE IDEA OF PHYSICAL AND METAPHYSICAL INFINITY.

By LIEUT. E. B. HUNT, Corps of Engineers, U. S. A.

FEW subjects of reflection have engaged the meditative energies of so large a portion of the leading intellects of all ages as that great idea which, under a vast diversity of forms and manifestations, is expressed by the word infinity. So true is this, that the charge of rash confidence might well be preferred against whoever should now profess to contribute any great additional light, where so much thinking has already been expended. In spite of this presumption, I shall venture some suggestions towards a precise definition of the idea of infinity, which have served to make clearer, to my own mind, what before was vague and indefinite.

It has seemed to me a correct criticism on the usual modes of considering the subject of infinity, that they regard it too exclusively under its metaphysical or speculative aspects, and too little in its physical or actualized forms. By at once pushing the idea of infinity into its abstract phases, we banish it from our positive cognizance, and relinquish the aids which nature affords in interpreting it to our finite comprehension. That such a hasty transfer from the concrete to the abstract form of contemplation involves a fault, may be appreciated at once by a simple consideration, in which all healthful minds will doubtless agree. The idea of infinity must dwell in the Divine creative mind in its greatest supposable perfection; consequently, its natural embodiments must possess a wholeness and intellectual truth, far exceeding what could originate from the abstract speculations of human mind. Therefore, whatever hints towards a due appreciation of infinity can be gleaned from the contemplation of actual created nature, will rest on a more solid basis than any unguided speculations can claim.

Whatever is intrinsically measurable may, by continuous quantitative expansion or diminution, grow to such an incomprehensible magnitude, that infinity becomes predicable of its value. Thus time, distance, space, number, force, or any quality of matter or mind, as hardness, temperature, light-producing power, sensational perceptive capacity, intellectual comprehension, force of will, benevolence, veneration, or, indeed, any species of actuality which can be regarded quantitatively, may be supposed so great or so small that the human mind will call it infinitely great or infinitely small. No such predicate would ever be used in speaking of what was not intrinsically measurable. Intrinsic mensurability is, therefore, a fundamental preliminary to any concrete infinity. In other words, whatever is infinite must have a unit of measure, and its infinitude consists in the relation between its aggregate quantity and its unit of quantity. Physical infinities thus fundamentally involve homogeneous physical units of measure as standards of reference. Therefore, the apprehension of infinity involves an apprehension of unity as its initial point, and this is equally true, whether the magnitude, called infinitely great or small, is physical and actual, or metaphysical, and only abstractly conceived. Hence our first necessary step is to analyze the idea of a unit of measure.

When we speak of a foot, a cubic yard, a pound, an hour, a thermometer-degree, etc., the ideas expressed are among the clearest which the human mind can entertain. Our mental and moral qualities, though intrinsically capable of equally exact units of measure, being actually without well-defined units, their quantitative comparisons become vague, and wanting in precision. Yet we are quite as prone to speak of Infinite intelligence or Infinite love, as of infinite distance or infinite time. This mode of speech rests on precisely as real a unit of measure as in the strictly physical cases, though it is less accurately defined to our own minds.

If then we take a single instance of a unit of measure for close analysis, the general results of such a discussion will reach to all the analogous cases.

Taking the unit of linear measure as this instance, we find that the original standards among all nations, whence other units of length are derived, have a pretty close general agreement. The foot, the yard, the metre, the toise, etc., are all evident derivatives from the human body, and stand in close relations to certain convenient modes of measurement, by reference to the geometry of the body. Our optical perception of perspective distances involves an habitual process, — first, of reference of all dimensions seen, to those distances near at hand, which are readily comprehended, and, in turn, of the reference of these to the actual linear distance between the optical centres of the two eyes. This interocular distance, or stereoscopic base line, bears the same relation to exterior visible distances, that the base line, in a geodetic triangulation, does to the entire network of triangles. Thus, when we gauge the perspective of a landscape, there is a direct visual perception or sensational measurement of external distances; — still, we are so habituated to this stereoscopic function of the interocular base line, that we do not make it an object of conscious contemplation in our reference of external distances to this base as a unit of measure. Such a reference, however, really enters as a vital part of every perspective perception. Hence we are constantly applying, unawares, a standard measure essentially unchanged for each individual during his whole life, to all external objects of our earthly surroundings. In like manner, the length of our habitual step enters largely as a basis in our estimation of distances, because we are constantly measuring distances seen by our habitual mechanism of locomotion. Thus, all our means of knowing external distances are found at last to rest solely on the actual dimensions of the human body, to which, as a standard, they are at last referred.

Taking into account all the elements of our perception of linear magnitudes, it will be found, without doubt, that the average linear unit is very nearly that length which is most readily cognizable by a man of average person and capacities. High multiples and low submultiples of the standard of length are difficult of appreciation; thus — a mile and a line are much less clearly apprehended units than a foot. If a thousand miles or a thousandth of a line be submitted to our consciousness, our notion becomes extremely inadequate, and if it be a million or a millionth, we fail almost entirely to conceive the fact. When it is a question of billions or trillions of miles, our apprehension is so totally at fault, that we give over all attempts to comprehend the relation, and so the distance becomes infinite for us, when referred to the mile as a unit. Thus our real idea is, when we speak of an infinite or infinitesimal distance, that when it is compared with our familiar standard of length, our perceptive powers utterly fail to appreciate the relation with any approach to accuracy.

If in place of the unity and infinity of distance we consider those of time, space, or force, we shall find a like genesis of practical standard units for each, based on the actual dimensions or sensational capacities of the human organism. Along the graduated line of connection between those values which by their minuteness utterly elude our perception, and those vast values which in their entirety wholly transcend our comprehension, there is, in each case, a particular value, which is apprehended with the maximum precision and facility by each individual mind. The natural unit of measure for each subject of measure is that particular value which is best apprehended by an average man. The same rule holds in the more transcendental subjects of measure; thus — a man of average intellectual power and capacity becomes a natural unit of mentality, and a man of average morality becomes a unit of morality. If a particular moral quality as benevolence, for instance, be quantitatively considered, we refer all to the man of average benevolence. When we speak of Divine benevolence as infinite, we mean that it is so exhaustless and all prevailing, that when we compare it with the benevolence of an average man, our limited human powers utterly fail to take in its relative immensity. From these considerations, we may conclude that whenever we predicate infinity of any particular existence, attribute, or quality of the external or internal world, man's capacity to appreciate the various values of the subject-matter considered enters directly as the standard of comparison. Thus, to say that space is infinite, is simply to say that the extreme exercise of human power to conceive space, as an existence, is transcended by the actuality of nature. If we speak of infinite time, we but declare that the brief periods of duration, of which man in his earthly life is conscious, are relatively so small that we can by no means conceive the number expressing the true ratio of man's hour or lifetime to the infinite duration referred to. Whatever physical infinity engages our consideration, an analogous limitation of the special powers of the human organism is defined. We might almost say that, for us, the grand sphere of physical infinity is the circumscribing sphere drawn around the aggregate perceptive faculties of man. It is not, at all, the absolute cosmos or circumscribing sphere which contains all the actualities of nature. We may well believe that this true cosmic sphere, in which all created existence is contained, is itself an infinity, as compared with that specific infinity which is, as it were, the defining or tangent surface around the faculties of man.

It is entirely supposable that among the actual organic existences of nature there may be numerous successive grades of perceptive capacity, which stand in the relation of coterminous or successive infinities as judged by each other. A monad may have a direct perception of man's infinitely small, and our sensible distance must be to it an infinitely great, magnitude. There may be intelligences such that the radius of an ultimate atom of matter would be to them what the radius of the earth is to us, as there may be intelligences to which the earth is but an atom, and to which our entire sphere of visible stars makes but a sensible mass of matter. Throughout the entire range of organic existence there will be, in fact, for each species a specific infinity, and we cannot say but in the treasure-house of the actual universe there may be an infinite series of organic perceptive powers, which bear to each other the relation of the successive orders of differences in differential calculus. Whatever may be the fact as to actual nature, such an infinite series of successive infinities is metaphysically conceivable. The clear apprehension of the idea of infinity which may be gleaned from physical grounds gives a basis for indefinite metaphysical fabrications, without in the least departing from the true, inductive idea of infinity. But science has not to deal with the supposable, except as it is involved in the actual, and it belongs not to this place or to true philosophy to go beyond the foundations of fact.

The views now presented have a bearing on the mathematical idea of infinity, which is not without importance. The mathematical symbol of infinity stands for an entirely abstract idea. From it all the definite standards of unity which have been discussed are entirely eliminated. All specific intelligences are in it ignored, and even the Divine Intelligence may be supposed in some way concrete in conditions too specific to be truly stated in respect to limits by the bald abstract infinity of pure analysis. The very possibility of positive definition, as applied to any being, however exalted, excludes the abstract symbol of infinity from entering a correct exegesis of its nature. For what, then, does the abstract symbol of infinity stand? It at least stands as a formula for all specific infinities, which, by the interpolation of the proper constants, expresses the quantitative relations in any actual case of infinity. The abstract symbol is a grouping of specific cases; whether it is more than this may not be for man to say.

One inference from these views of infinity is, that the ordinary definition of an asymptotic curve needs correction. The mathematical formula of incessant and incessantly diminishing approach between a straight line and a curve, or between two curved lines, or the same relative to plane and curved surfaces, is not consistent with the other idea of tangency at an infinite distance. Suppose an intellect of the proper or differential grade to be duly cognizant of asymptotic lines at an infinite distance, it would find no actual contact, but the same law of approach expressed in the analytical formula would still go on, until an intellect of the second differential order would have to be called in as the cognizant power; this, in turn, must give place to a third differential intellect, and so on to infinity. The order of perception required to appreciate the second, third, etc., differentials of the function, would be progressively higher than that demanded for the differential of the variable, which is supposed to be always constant. Here there is no true tangency, but a perpetually decreasing approximation. Hence the definition of asymptotes should give the idea of a perpetually diminishing and never ending approximation, instead of involving the false notion of tangency anywhere. The geometrical method of exhaustions escapes any such criticism, and in the clearest manner embodies the true conception of infinity.

As might be expected, the infinitesimal calculus expresses the notion of infinity in its most precise and purified form. The fundamental idea on which its processes and algorithm rest is one of relation between quantities cognizable by two orders of perceptive intelligence, so remote that the finite quantity of the one is the infinitely great or small of the other. By the hypothecation of three, four, or more coterminous orders of perceptive faculty, the second, third, etc. orders of differences are philosophically originated. The relations of differential calculus, if inversely stated, become those of integral calculus, and fall under the same generalizations relative to orders of perceptive capacity. I will venture the suggestion, that the reason why some eminent mathematicians have contended that a differential is absolutely zero, is simply because of their not having regarded, as they clearly are bound to do, the element of limited perceptive faculties which infinity involves, and the consequently supposable series of cognitions precisely conforming with first, second, third, etc. differentials. It is the same fault of conception which invalidates the definition of asymptotes based on tangency at an infinite distance.

In conclusion, we may lay down the general proposition, that the idea of infinity involves in all cases, as an essential factor in its composition, a specific, actual, or hypothetical limitation of perceptive power in the intelligence, of whose cognition infinity is predicated. In the physical infinities which chiefly interest us, the limitations involved are, *for us*, altogether those which encompass the mind of man. We can, by hypothesis, suppose other limits conformed to other grades of organism, and we can even suppose such an exalted and spiritual organism as that the absolute and entire cosmos shall be the true limit of its perceptive powers. Before the Divine mind, this cosmos must stand in that clear, finite relation necessarily supposed between a creator and the thing created. Beyond the actual cosmos, there may be an immensity of possibility, where the Divine mind may realize analogous limitations to those which hedge in all created minds. In the mathematical or purely abstract idea of infinity there seems a suggestion of such a possibility; and when we consider that a mathematical formula is the nearest possible approach to a literally Divine thought, we shall bow with reverence before the suggestion shadowed forth by the sublime symbol of infinity, after all created limitations are eliminated from its significance. This symbol affords no basis for the commonly received idea that infinity means an *absolute unboundedness*, a quantity *absolutely* without end, a quality or nature *absolutely* transcending all boundaries. Such an idea has no right in the mind of man, for the limitations of human perception forbid our attainment of any knowledge, either of the extent of the absolute cosmos, or of the boundaries around what is abstractly possible. The formula of infinity, so far from stating an absolute boundlessness, endlessness, or illimitable magnitude, states simply the limitations of finite perceptive power. For us it is the expression, not of the immeasurableness of nature or of the Deity, but of the finite limitations of the human mind. It stands for a negative, and not for a positive; it symbolizes, not knowledge, but ignorance. If we group infinite attributes under a Divine name, we have not defined Deity, but we have defined the limits of our own perceptions. The limits of our knowledge lie near at hand; the limits of our ignorance are known only to the All-knowing.

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2. SYSTEMS OF COÖRDINATES IN ONE PLANE. By THOMAS HILL, Waltham, Mass.

IF Mm be an infinitesimal portion of a curve, the position of M may be defined by means of

x, y, the rectangular coördinates,
or r, φ, the polar coördinates.

The character of the element, Mm, may be defined by means of

ε, the angle which r makes with the curve.
τ, the angle which the curve makes with the axis of x.
ϱ, the radius of curvature.
ds, the element Mm of length in the curve.

The nature of the curve, of which Mm is an element, may be defined by an algebraic equation between any two of the quantities, x, y, r, φ, ε, τ, ν, ϱ, s. If either of the quantities, x, y, and r, is used, the position, as well as the form, of the curve is usually fixed. If φ or ε enters the equation, the position *may* be fixed. An equation embracing only two of the quantities, τ, ν, ϱ, s, although defining the form and sometimes the size of the curve, and even its position with reference to a fixed direction, cannot define its position with reference to any fixed point.

From these quantities twenty-two different couples may be selected, and either member of each couple may be taken as the independent variable, and the other considered as a function thereof. Thus we have, in one sense, forty-three general systems of coördinates, by which we may represent a curve without the use of any thing more than ordinary algebra. About one fourth of these systems appear, to me, to be worthy of investigation. Rectangular coördinates in which y is a function of x, and polar, in which r is considered a function of φ, are too well known to need any particular remarks. The first has the advantage over all others, in its generality and flexibility. The second is peculiarly adapted to curves of circular symmetry. Both systems have also this advantage, that an equation in them is readily constructed upon paper; an advantage which would, however, be retained in some other systems; for instance, in one representing x as a function of φ. But all those systems which refer to a fixed origin, have also the disadvantage of not showing readily the evolutes of the curve, and also of being confined in their range to the first order of infinity.

Algebraical language, even under the forms of Hamilton's Quaternions, is an imperfect means of presenting geometrical truths, and although an equation between any two of these quantities will give a law of the curve, yet no two equations will give precisely the same view, even if they give the same law. Both the denotation and the connotation of the language vary in every transformation of the equation. I have illustrated this truth by throwing the equilateral hyperbola into seventeen systems of coördinates, and comparing the geometrical interpretation of the equations; and by throwing the parabola, the cycloids, and other curves, into six or seven systems. The variation of geometrical meaning is very striking.

The cycloid, for example, is, by most systems, represented as generated by a rolling circle, and consisting of an indefinite number of arches. But represented by an equation between ϱ and s, $\left(\varrho = (A^2 - s^2)^{\frac{1}{2}}\right)$, the cycloid becomes finite in length. If ϱ is represented as a function of s, the cycloid is a single arch; if s is considered as a function of ϱ, the cycloid is two half arches including a cusp. All the rest becomes imaginary, thus strikingly demonstrating that the imaginary may be real, since the real may become imaginary.

On the other hand the parabola is usually considered as a conic section, consisting necessarily of a single arch. But an equation of the parabola from which x, y, and r are eliminated,

$$\left(\cot \varepsilon = \cot \tfrac{1}{2}\, \varphi,\ \tau = \tfrac{1}{2}\, \varphi,\ \varrho = \frac{2\,\mathrm{p}}{\cos^3 \tau}\right)$$

will sometimes give us a series of arches like the cycloid, — the arches being infinitely deep and infinitely wide.

If we define an equilateral hyperbola as a curve, the rectangle of whose ordinate and abscissa is constant, we evidently make it consist of four branches represented in rectangular coördinates only by means of the two equations,

$$xy = \sqrt{A^4}.$$

Using the positive root of the second member would give only the right upper and left lower branch. Nine of the seventeen systems unite in preferring these two branches, but differ in the points corresponding to zero of the variable; that is, some begin the branches at one end, and others at the other. Seven systems give us the whole of the four branches, some beginning the branches at one end, some at the other, one at both ends at once, and one at the middle of each branch, running out both ways at once. Two of these systems ($\sin \varepsilon = \sin 2\, \varphi$), ($\tan \varphi = \tan \tau$) give us the curve of four branches, but without defining its size. These equations are equations of pure form, independent of magnitude. Some of these sixteen equations, while defining the form and size, and even the distance from a fixed origin, do not define the direction. They are equations of a circle with a hyperbola, or an indefinite number of equal hyperbolas tangent upon it.

The seventeenth equation, by which I have represented the equilateral hyperbola, does, in fact, represent the whole plane; it is,

$$\varrho = \frac{r^3}{2\,A^2}.$$

But by fixing the arbitrary constants, and drawing some of the curves, it will be found to embrace not only the hyperbola, but other forms.

In like manner $\varrho = Ar$ will give not only logarithmic spirals, but two other forms of new curves.

These results are obtained by the ordinary processes of analytical geometry, including the calculus, without any attempt at the interpretation of imaginary quantities, or any extension of notation in the manner of the Quaternions. They are sufficient to show how much lies concealed in even the ordinary walks of the geometer, and how much may be discovered in geometrical truth by the mere metamorphosis of algebraic notation.

Some of these systems of coördinates appear to me to deserve a more thorough investigation than they have as yet received. They appear to be as natural and as comprehensive as either of the three systems now in use. If, for example, the direction of a curve is represented as a function of its length $\left(\tau = f(s)\right)$, or its radius of curvature as a function of its length $\left(\varrho = f(s)\right)$, you have a very natural and useful mode of defining the curve. Good results are also to be obtained by making φ a function of s. Since arriving at this meeting I have learned that Whewell's "intrinsic equations" of a curve, which I had supposed identical with "Peirce's circular coördinates," are formed between τ and s.

3. On the Warped Surfaces of Ground occurring in Road Excavations and Embankments. By Prof. W. M. Gillespie, of Union College, Schenectady, N. Y.

When an engineer is laying out a road or railway, and has to determine the amount of earth necessary to be moved in making the "cuts" and "fills," he takes "Cross-sections," or "Profiles," of the ground at right angles to the line of road at convenient intervals, and then calculates by various methods, usually near approximations, the volume included between each pair of these cross-sections. The distances apart at which these cross-sections are taken are determined by the engineer according to the nature of the ground, his aim being that there shall not merely be no abrupt change of height between each pair of these cross-sections, but that the surface from one to the other shall *vary uniformly;* gradually passing, for example, from a small to a great degree of slope, or from a slope to the right into a slope to the left, without any sudden variation at any one place.

The surface fulfilling this condition of "varying uniformly," since it is everywhere straight in some direction, is evidently a *ruled surface;* and since the extreme profiles are seldom parallel, it will generally be a *warped* or *twisted* surface.

Our engineers have been accustomed to consider these surfaces as not admitting of precise calculation, but only of a degree of approximation varying with the nearness of the cross-sections.* The object of this paper is to examine the correctness of this position. It will therefore have two parts: firstly, a discussion of the precise nature of the surface; and secondly, an investigation of a formula applying to it.

I. *What sort of a warped surface is the one in question? that is, what is its mode of generation?*

To determine this, we must inquire what the engineer means when he says that the ground "varies uniformly" from the place at which he stands, and at which he has just taken a cross-section, to the place at which he decides it will be proper to take the next cross-section; whether he means that the ground between the two is straight lengthwise or straight crosswise — straight in the direction in which the road runs, or straight at right angles to that direction.

Probably few engineers ask themselves this question in so many words; but it would seem that the latter conception, or *straightness crosswise,* is the more likely to be what is meant, for the reason that any deviation from straightness in that direction, at right angles to the line along which we look, is much more easily seen than in the other direction. We can therefore much more readily determine whether the surface of the road is straight or curved from side to side than from end to end, and the surface which we pronounce uniform is therefore much more likely to be straight crosswise than lengthwise.

In geometrical language, the former surface (shown in plan in fig. 1) is generated by a straight line resting on the two straight lines which join the extremeties of the two profiles, and moving parallel to their planes, or perpendicular to the axis of the road. This surface is a "HYPERBOLIC PARABOLOID."

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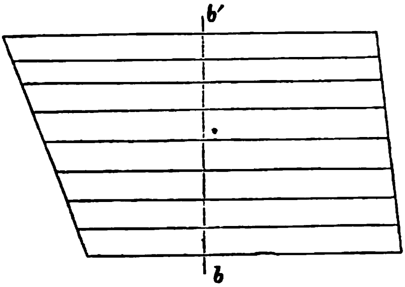
* Thus the preface of a recent "Table, etc.," says: "No practicable method of calculation will give a true result, if the surface of the ground is much warped."

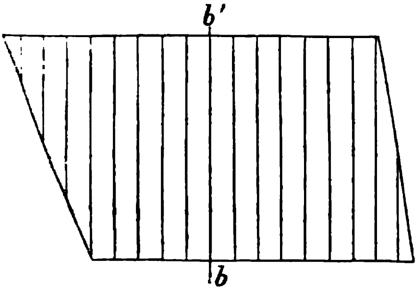
Fig. 1.



The latter surface (fig. 2) is generated by a straight line resting on the two profiles, and moving parallel to the vertical plane which passes through the axis of the road. It also is a hyperbolic paraboloid, though a different one from the former.

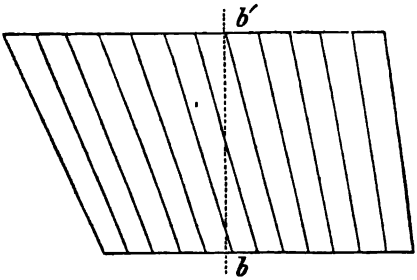
The French engineers adopt the latter hypothesis. We have seen, however, that the former is the more probable one.

Fig. 2.



But fortunately the difference is really very slight; for a very small change in the latter hypothesis will make its result identical with that of the former. Conceive the straight line which rests on the two profiles to move on them in such a way as always to divide them *proportionally*, as in fig. 3. The surface thus generated is *identical* with that of fig. 1, as is proven in the treatises on Descriptive Geometry.*

Fig. 3.



This last conception is also more probably correct than that of fig. 2, even if we suppose the engineer to consider longitudinal straightness; since he is more likely to extend his imagination from all parts of one profile to the corresponding parts of the other, than in lines perpendicular to the profile on which he stands.

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* Olivier, p. 263, etc.; Leroy, p. 249.

The French hypothesis is further objectionable on purely mathematical grounds. As soon as the generating line quits the end line and rests on the side lines, these are new directrices, and consequently the entire surface generated is really composed of three different paraboloids. This want of symmetry is alone sufficient to cause the rejection of this system.

II. We will therefore now proceed to investigate the content of a solid bounded on one face by a warped surface generated on the first hypothesis, the other faces being planes.

We will take the case of an **excavation**, that of an **embankment** being the same inverted.

We will begin by considering the bottom of the excavation to be horizontal, and its sides to be vertical, and will afterwards discuss the more usual form.

Fig. 4.

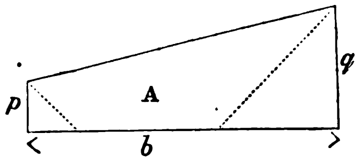
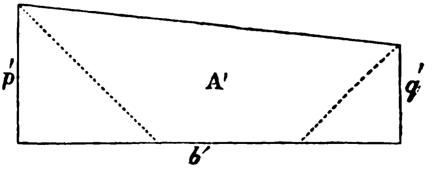


Fig. 5.



Let A and A′ be the parallel sections at each end of the solid under discussion; b and b' their respective breadths; p and q the outside depths of the section A; p' and q' those of the section A′; and l the length of the solid, measured at right angles to the planes of the sections.

The outside depths are supposed to vary uniformly from p to p', and from q to q'.

Then at x feet from A the breadth $= b + \frac{x}{l}(b' - b)$; one outside depth $= p + \frac{x}{l}(p' - p)$; and the other $= q + \frac{x}{l}(q' - q)$. The area of this trapezoidal section will therefore be

$$\tfrac{1}{2}\left[b + \frac{x}{l}(b' - b)\right] \times \left[p + \frac{x}{l}(p' - p) + q + \frac{x}{l}(q' - q)\right] \quad [1]$$

Arranging this expression according to the ascending powers of x, it becomes

$$\tfrac{1}{2}\left[b\,(p+q)+\frac{b\,(p'-p+q'-q)+(b'-b)\,(p+q)}{l}\,x \right.$$
$$\left. +\frac{(b'-b)\,(p'-p+q'-q)}{l^2}\,x^2\right].$$

The product of this by dx being the differential of the solid, its volume will be

$$\int_o^l \tfrac{1}{2}\left[b\,(p+q)\,dx+\frac{b\,(p'-p+q'-q)+(b'-b)\,(p+q)}{l}\,x\,dx \right.$$
$$\left. +\frac{(b'-b)\,(p'-p+q'-q)}{l^2}\,x^2\,dx\right]. \qquad [2]$$

Integrating, we obtain the expression

$$\tfrac{1}{2}\left[b\,(p+q)\,l+\tfrac{1}{2}\,b\,(p'-p+q'-q)\,l+\tfrac{1}{2}\,(b'-b)\,(p+q)\,l \right.$$
$$\left. +\tfrac{1}{3}\,(b'-b)\,(p'-p+q'-q)\,l\right].$$

Performing the operations indicated and factoring, we finally obtain, as the volume of the solid, the symmetrical expression

$$\tfrac{1}{6}l\left[(b+\tfrac{1}{2}\,b')\,(p+q)+(b'+\tfrac{1}{2}\,b)\,(p'+q')\right].^* \qquad [3]$$

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* Two particular cases of this general formula are worthy of special notice.

Let the base of the given solid be a parallelogram.

Then $b=b'$, and formula [3] becomes

$$\tfrac{1}{6}\,l\,[\tfrac{3}{2}\,b\,(p+q)+\tfrac{3}{2}\,b\,(p'+q')=b\,l\times\tfrac{1}{4}\,(p+q+p'+q').$$

That is, the volume of a right prism, having its base a parallelogram, and its upper surface warped, equals the product of its base by one fourth of the sum of its edges or heights.

Next, let the base be a triangle.

Then $b'=o$, and $p'=q'$, and formula [3] becomes

$$\tfrac{1}{6}\;l\;\left[b\,(p+q)+b\,p'\right]=\tfrac{1}{2}\,b\;l\times\tfrac{1}{3}\;(p+q+p')].$$

That is, the volume of a triangular prism, with its upper surface warped, equals the product of its base by one third of the sum of its edges or heights.

The above two rules have been already obtained by the French geometers, by the aid of other considerations derived from the Higher Descriptive Geometry.

Of course they are also true when the upper surface of the prism is a plane, since a plane is only a particular case of a hyperbolic paraboloid. They thus give a general proof of the well-known rules for the content of truncated prisms having triangles or parallelograms for bases.

We now propose to show that the volume given by the preceding formula [3] is the same as would be obtained by applying the familiar prismoidal formula to the given solid.

The area of the section $A = \frac{1}{2} b \, (p + q)$.
The area of the section $B = \frac{1}{2} b' \, (p' + q')$.

The area of the section midway between A and B equals

$$\tfrac{1}{2}\left\{\tfrac{1}{2}\,(b+b') \times \left[\tfrac{1}{2}\,(p+p') + \tfrac{1}{2}\,(q+q')\right]\right\}.$$

Adding together the areas of A and B, and four times the middle area, and multiplying the sum by $\frac{1}{6}\,l$, we obtain

$$\tfrac{1}{6}\,l\,(bp + bq + b'p' + b'q' + \tfrac{1}{2}\,bp' + \tfrac{1}{2}\,bq' + \tfrac{1}{2}\,b'p + \tfrac{1}{2}\,b'q),$$

which can be decomposed into the following:

$$\tfrac{1}{6}\,l\left[(b+\tfrac{1}{2}\,b')\,(p+q) + (b'+\tfrac{1}{2}\,b)\,(p'+q')\right], \qquad [3']$$

an expression identical with the general formula [3].

We thus arrive at the conclusion that the familiar "prismoidal formula" can be applied with perfect accuracy to such solids as we have discussed, having one of their faces a warped surface, generated as in our first (or third) hypothesis.

We have thus far supposed that the road-bed was level, or, in more general terms, that the face of the solid was perpendicular to its ends. It may, however, make oblique angles with them. But the prismoidal rule will still apply to the new solid thus produced, since it applies to the wedge-shaped piece, by the removal of which the original solid can be converted into the new one, and therefore applies to the new one, which is the difference of the two; seeing that all the areas involved enter into the formula only by addition or subtraction, and have a common multiplier.

We have also supposed the sides of the solid to be vertical; but in road excavations, etc., they usually slope, as shown by the dotted lines in figs. 4 and 5. But the prismoidal rule still applies, for the same reasons as in the preceding paragraphs, since it applies to the pyramidal frusta whose removal changes the original solid into the one now proposed.

This rule also applies to the "*three-level ground,*" familiar to engineers, in which the centre level and the outside levels of a cross-section are given. A vertical plane passed through the axis of the road divides it into two solids, such as we have been considering. It can, therefore, be correctly calculated as one prismoid.

The rule may also be applied to "*irregular cross-sections*," or those in which the surface of the ground is so irregular that a number of levels have to be taken at various points of each profile. To do this, we must conceive vertical planes to pass through all the points of each profile at which the transverse slope of the ground changes. They will thus divide the solid into a number of solids with warped surfaces, such as we have been considering. The principle of our third hypothesis will then enable us to determine by a simple proportion the height at which the vertical plane passing through any angular summit of one profile cuts the top line of the other. This will furnish the data for applying the preceding rule.

If the views here presented should meet with general acceptance, engineers would be enabled to economize much time and labor, since they would no longer feel compelled, as now, to take their cross-sections so near together that the ground between them should be approximately plane, but could take them as far apart as the ground *varied uniformly*, no matter how much, or for what distance, that might be.

The adoption of these principles would also seriously modify the methods now employed in the calculation of earthwork, and the results obtained by them, particularly the use of "equivalent mean heights." The numerical details of these professional matters would, however, be out of place here.

4. ON THE LAW OF HUMAN MORTALITY THAT APPEARS TO OBTAIN IN MASSACHUSETTS, WITH TABLES OF PRACTICAL VALUE DEDUCED THEREFROM. By E. B. ELLIOTT, of Boston.

THE accompanying tables comprise part of an original series, that has been prepared for the New England Mutual Life Insurance Company of Boston, from extensive and reliable European and American data. They have been calculated from official abstracts of observations made for the year 1855, respecting the status and the movements of the population in the 166 of the 331 towns of the Commonwealth of Massachusetts, in each of which, the ratio of the number of registered deaths, to the number of the population, was *greater* than one to sixty-three.

With the numbers returned for the 166 towns, have been included two thirds of the numbers of the population, births, and deaths of the three State almshouses; the population of the 166 towns being two thirds (.663) of the population of the entire State.

The aggregate population of these communities, as returned for the first day of June, 1855, was 751,241, and the registered deaths in these towns during the year was 16,086. The well-known Carlisle table of mortality was deduced from only 1,840 deaths, registered during the nine years 1779–87, the mean population of the period being 8,177.

The rate of mortality, or the ratio of deaths to the population, according to the returns, was probably somewhat lower, in these communities, than the rate that actually prevailed in them, in consequence of probable omissions in the registration of deaths in some of the districts.

But these communities embrace a larger proportion of the more populous districts of the State, and of those in which we should expect the prevailing rate of mortality to be higher than the rate for the entire State; and we are probably safe in concluding that the law of mortality obtaining in these districts, according to the returns, does not greatly vary from the law of mortality actually prevailing over the entire population of the Commonwealth.

It is not possible, supplied with only our present information, to indicate the precise line of separation between the reliable and the questionable data; but it is thought that such a division has been made that the data retained are affected by only inconsiderable errors, and the errors of excess and of defect nearly compensate for each other.

The tables deduced from the resulting law of mortality are intended to facilitate the solution of certain problems in political arithmetic, and to furnish replies to questions involving the probable duration of human life.

Tables are also given, comparing the rates of mortality obtaining in Massachusetts at divers intervals of age, with corresponding rates obtaining in certain European communities.

The "Fourteenth Annual Report relating to the Registry and Returns of Births, Marriages, and Deaths, in Massachusetts, for the Year 1855," and the "Abstract of the Census of the Commonwealth of Massachusetts, taken with reference to facts existing on the first day of June, 1855," prepared under the direction of the Hon. Francis DeWitt, Secretary of the Commonwealth, and under the supervision of Dr. Nathaniel B. Shurtleff, of Boston, appear to have been the first and the only official reports, whether State or national, in which either the ages of the persons living, or the ages of those dying, in the State, have been *distinguished by towns;* consequently, they are the first that furnish data, from comparison of which it has been possible to construct a Life Table that can satisfactorily express the law of mortality prevailing over any considerable portion of the people of the Commonwealth.* † In previous reports the ages have been distinguished only *by counties.*

Since the Registration Act of 1849, the registrars, in many of the towns of the Commonwealth, appear to have annually furnished the office of Secretary of State with very valuable and accurate statistics respecting the births, marriages, and deaths occurring in their respective districts.

In other towns the results indicate that this information has been but imperfectly recorded, while in a few cases the officers have uniformly neglected either to register or report.

In registration reports, previous to that for the year 1855, the full and reliable information respecting the ages of the dying furnished by the able and competent registrars, in certain towns, has been vitiated by union with the questionable or obviously defective data obtained from other towns in the same county.

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* A Life Table, prepared by the eminent Dr. Edward Wigglesworth from sixty-two Bills of Mortality recorded previous to the year 1789 in the States of Massachusetts and of New Hampshire, was published in the second volume of the Memoirs of the American Academy of Arts and Sciences. Unfortunately, in constructing the table, allowance was not made for the fact that the population had been rapidly increasing, and the table was framed on the assumption that it had been stationary. This table has been employed by the courts of the Commonwealth in determining the values of "life interests in estates, legacies, and pensions," and the values of "reversions in heritable property."

† A valuable Life Table, constructed from observations respecting the mortality of the Alumni of Harvard University, and, consequently, expressing the law of mortality which prevails, in America, over the more highly educated classes, was laid before the American Association at its late meeting in Montreal, by Prof. Benjamin Peirce, of Cambridge.

In the Registration Report for 1855, the table which distinguishes by age, sex, and locality the deaths registered during the year (Table VI.), was prepared with special reference to its employment in the construction of Life Tables, and is believed to be a faithful abstract from the returns.

The enumeration of the numbers and ages of the population of Massachusetts, according to the State Census of 1855, and the previous enumerations ordered by the General Government, may safely be regarded as reliable.

It is to be regretted that the abstracts of the Census for 1855, in giving the ages of the population, did not distinguish the *sexes*.

This deficiency may, in a measure, be supplied by assuming that the proportional distribution of the sexes at the different ages in 1855, was the same as that obtaining in Massachusetts in 1850, according to the National Census of that year.

Of the 324 statistical districts into which England and Wales are subdivided, in only *two* was there indicated, according to very accurate observations for the seven years, 1838–44, an annual rate of mortality *less* than one death to sixty-three persons living. Of the 331 towns in Massachusetts (nearly the same in number with the English districts just mentioned), there were, in 1855, 165 towns, in each of which the rate of mortality, indicated by the returns, was *less* than one to sixty-three. The returns from the three State almshouses are not included with the returns of the towns in which they happen to be located, namely, Monson, Tewksbury, and Bridgewater. The average population of the 331 towns of Massachusetts is much less than that of the 324 English districts; the population of Massachusetts, in 1855, being one million (1,132,369), and that of England and Wales, in 1841, sixteen millions (15,927,867). The above assumed test, in selecting returns showing a mortality over one to sixty-three, is accordingly confirmed by the English returns, which are made, as is well known, with great accuracy.

The mortality of the 324 English districts varied from 1 in 70 to 1 in 30; in the 331 towns of Massachusetts, according to the returns, from *zero* to 1 in 30. The mortality of Massachusetts, according to the entire returns, 1.84 per cent., or 1 in 54; in the 166 towns it was 2.14 per cent., or 1 in 47.

Average Annual Rates of Mortality obtaining in the whole, or in parts of Seven Countries of Europe, arranged in the Inverse Order of their Respective Intensities.

Years in which the Deaths occurred.	Countries.	To 100 Persons Living,	One Death
		Deaths.	To Pers'ns Living.
7 years 1838–44	England and Wales *	2.19	46.
20 " 1821–40	Sweden †	2.34	43.
4 " 1839–42	France ‡	2.36	42.
9 " 1842–50	Belgium §	2.42	41.
3 " 1839–41	Prussia ‡	2.70	37.
3 " 1834, 7, 9	Austria ‡	3.09	32.
1 " 1842	Russia ‡	3.59	28.

According to the above, it appears that in England and Wales the conditions are most favorable to vitality, and in Russia, least; the several countries, arranged in the inverse order of the respective intensities of mortality, being England, Sweden, France, Belgium, Prussia, Austria, and Russia.

The population of Massachusetts has increased more rapidly than those of the principal countries of Europe. The annual rate of increase of the former, during the years 1850–55, was two and two thirds per cent. (2.63). The effect of an increase by births merely is to diminish, in some degree, the general rate of mortality, even though the intensity of mortality, at different ages, remains unchanged.

We will now proceed to the construction of the Life Table.

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* 9th Report Registrar General (England).

† 8th Report Registrar General (England).

‡ See 6th Report Registrar General (England).

§ Statistique de la Belgique, Publié par le Ministre de l'Interieur.

TABLE I.—NUMBER AND AGES OF THE POPULATION OF MASSACHUSETTS, ACCORDING TO THE NATIONAL CENSUS OF 1850 AND OF THE STATE CENSUS OF 1855; ALSO THE ANNUAL RATIO OF INCREASE, AND THE LOGARITHMS OF THE MONTHLY RATIO OF INCREASE, THE RATIOS BEING CORRECTED FOR THE NUMBERS RETURNED AT AGES NOT SPECIFIED.

Ages.	Population.		Unity plus the Annual Rate of Increase.	Logarithms of the Monthly Ratio of Increase.
	1850.	1855.		
0–1	23,192			
1–5	90,853			
0–5	114,045	132,944	1.03131	.0011159
5–10	102,797	115,862	1.02439	.0008720
10–15	98,024	110,098	1.02367	.0008468
15–20	105,741	117,047	1.02069	.0007413
20–30	210,997	235,678	1.02254	.0008067
30–40	143,931	165,046	1.02793	.0009968
40–50	96,266	111,500	1.02999	.0010694
50–60	60,254	71,829	1.03594	.0012779
60–70	36,837	42,423	1.02881	.0010279
70–80	17,936	20,810	1.02034	.0010818
80–90	5,820	6,138	1.01086	.0003911
90–100	613	634	1.00693	.0002498
100 and upwards	19	19	1.00017	.0000060
Age not specified	1,234	2,341		
All ages	994,514	1,132,369	1.02630	.0009396
Specified ages	993,280	1,130,028		
90 and upwards	632	653		.0002426

From the returns of births for the six years, 1850–55, it appears that the annual rate of increase was 3.49 per cent. But the annual rate of the increase of the living, under the age of five years, was only 3.13. The latter is believed to be the more correct. The difference is probably owing to a gradual improvement in the completeness of the returns of the births.

In that which follows, we shall assume that the population in the selected districts increased, at the different intervals of age, at the same rates as in the entire State.

The population of the 166 towns may be divided into two classes, the *migratory* and the *permanent;* the former comprising immigrants and emigrants.

We assume that the proportional distribution of the ages of those living under the age of *five* years, was the same in the latter as in the former class; that the ratio of the number of births to the number living under age five, was the same in each; and that the same *invariable* law of mortality prevailed over those under the age of five years in each.

In the towns selected, the number of births registered in 1855 was 23,481. The number of persons living under age five, estimated with reference to the middle of the year and corrected for those returned at unspecified ages, was 90,260.

	Ages.	Deaths.
The deaths at different ages under five years, corrected for those returned at unspecified ages, were	0–1	3,622
	1–2	1,654
	2–3	705
	3–4	375
	4–5	252

Assuming correctness of the returns upon which the above values depend, we find the annual rate of increase of births in the *permanent* population to have been 1.1023 per cent.[*] We also find the number of deaths at the different ages demanded by a constant supply of 23,481 annual births in a community influenced by migration, but subject to the above-mentioned invariable law of mortality, to be

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[*] Had the rate of the annual increase of the numbers living under age five (3.13 per cent.) resulted entirely from the increase of births in a permanent population, the number of births of 1855 would have been 24,457, instead of 23,481, the number registered. On the other hand, had the increase resulted wholly from migration (the annual number of births in the permanent population being constant), the number of

Ages.	Deaths.
0–1	3,641.9
1–2	1,681.4
2–3	724.6
3–4	389.7
4–5	264.7

Hence of 10,000 persons born alive, there would survive, ages 1, 2, 3, 4, and 5, as follows: —

TABLE II.

Ages.	Numbers.	Logarithms.
0	10,000.0	4.0000000
1	8,449.0	3.9268053
2	7,732.9	3.8883424
3	7,424.3	3.8706555
4	7,258.3	3.8608349
5	7,145.6	3.8540387

To continue the above table, we shall need to compare the deaths and the population at the different intervals of age.

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births would have been only 22,956. The number of births registered is somewhat nearer the latter than the former of these two values.

Assuming the correctness of the returns of births, deaths, and population in the selected districts, and of the indicated rates of increase of population, it appears that 35 per cent. of the increase of the population *under age five* was due to increase of births in the permanent portion of the population, and 65 per cent. due to the movement of the migratory portion; also that 38 per cent. of the increase of population *at all ages* was due to excess of births over deaths, leaving 62 per cent. to be accounted for by excess of immigration over emigration. (A.)

TABLE III. — DEATHS, POPULATION, MORTALITY, AND LOGARITHMS OF THE PROBABILITY OF LIVING.

Massachusetts, 166 *towns,* 1855.

AGES.	REGISTERED DEATHS.	POPULATION.	MORTALITY.	LOGARITHMS, WITH THE SIGN CHANG'D, OF THE PROBABILITY OF SURVIVING EACH INTERVAL OF AGE.	
	1855.	June 1st, 1855.	Ratio of the Number of Deaths in 1855 to the Number of the Population, estimated for the middle of that year, and corrected for the Numbers at ages not specified.	Duplicate Values, each deduced from *two* consecutive Ratios in the Column of Mortality.	Arithmetrical Mean of the Duplicate Values.
Under 1 year . .	3,595	89,852	.0732094		
1–2	1,642				
2–3	700				
3–4	372				
4–5	250				
3–5	622		.0191772*		
5–10	595	76,566	.0077980	.0168125 .0169100	.0168613
10–15	311	71,851	.0043436	.0094179 .0094488	.0094333
15–20	672	76,854	.0087768	.0190957 .0190705	.0190831
20–30	1,817	161,544	.0112883	.0491767 .0490648	.0491208
30–40	1,388	112,489	.0123781	.0538119 .0538257	.0538188
40–50	1,035	73,604	.0141040	.0613538 .0615053	.0614295
50–60	908	45,134	.0201687	.0881465 .0884615	.0883040
60–70	942	25,766	.0366732	.1624950 .1624646	.1624798
70–80	976	12,265	.0798130	.3729434 .3595270	.3684713†
80–90	635	3,469	.1838871	1.1507632 .3622095	.9536248†
90 and upwards . .	129	374	.3466159		
Age not specified .	119	1,472			
All ages	16,086	751,240	.0213663		
Specified ages . .	15,967	749,768			

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* From deaths and *estimated* population, at the ages of three to five.

† The former of these values was obtained by giving *double,* and the latter by giving *triple, weight* to the antecedent of the respective duplicate values in the preceding column.

The method adopted in calculating the probabilities of living from the annual rate of mortality, is essentially the same as that indicated on pages 60 and 61 of the Proceedings of the American Association for 1856.

From the above Tables II. and III. the following values, from birth to age 90 inclusive, are readily deduced.

TABLE IV.—PROPORTIONS BORN ALIVE, AND SURVIVING CERTAIN AGES.

Ages.	Logarithms.	Numbers.
0	4.0000000	10,000
1	3.9268053	8,449
2	3.8883424	7,733
3	3.8706555	7,424
4	3.8608349	7,258
5	3.8540387	7,146
10	3.8371774	6,873
15	3.8277441	6,726
20	3.8086610	6,437
30	3.7595402	5,748
40	3.7057214	5,078
50	3.6442919	4,409
60	3.5559879	3,597
70	3.3935081	2,475
80	3.0250368	1,059
90	2.0714120	117.9
100	.3430527	2.20

In assigning the average number that may be expected to survive age 100 out of a stated number of births, there is room for some diversity of opinion. The influence, however, of the numbers at this extreme age upon tables of practical utility is inconsiderable.

The logarithms in Table V. were derived from those in Table IV., by the interpolation of eight additional values, namely, those at the ages of 25, 35, 45, 55, 65, 75, 85, and 95. The third differences of the logarithms from age 35 upwards in the following table constitute a constantly increasing series.

TABLE V.—MASSACHUSETTS LIFE TABLE, 1855.*

	The number of persons *living at certain ages, to 10,000 children born alive.* Also, the annual number of *deaths at and over certain ages, in a stationary population* supplied by 10,000 annual births.		The *years* which the (L_x) persons *living at certain ages will live*; also, the *years* which those annually *dying at and over* certain ages, in the stationary population, *have lived over* those ages; *persons living at and over certain ages*, in the stationary population.	*Years* which the (Q_x) pesons *living at and over certain ages*, in the stationary population, *will live*; also, the *years* which they *have lived* over those ages.	Average number of *years* which those *living* at certain ages *will live*; also, which those *dying, at and over* certain ages, *have lived* over those ages.	*Average* number of *years* which those *living at and over certain ages will live*; also, which *they have lived* over those ages.
Ages	Logarithms.	Numbers.	$dx\int_x^\infty L_x.$	$dx\int_x^\infty Q_x=(dx)^2\int_x^\infty\int_x^\infty L_x.$	$\frac{Q_x}{L_x}.$	$\frac{Y_x}{Q}.$
x	λL_x	L_x	Q_x	Y_x	E_x	E'_x
0	4.000000	10,000	397,653	12,857,379	39.77	32.33
1	3.926805	8,449	388,655		46.00	
2	3.888342	7,733	380,616		49.22	
3	3.870656	7,424	373,060		50.25	
4	3.860835	7,258	365,727		50.39	
5	3.854039	7,146	358,530	10,967,776	50.17	30.59
10	3.837177	6,873	323,508	9,263,748	47.07	28.64
15	3.827744	6,726	289,508	7,731,641	43.04	26.71
20	3.808661	6,437	256,561	6,367,019	39.86	24.82
25	3.785307	6,100	225,205	5,163,297	36.92	22.93
30	3.759540	5,748	195,584	4,112,047	34.03	21.02
35	3.733016	5,408	167,699	3,204,550	31.01	19.11
40	3.705721	5,078	141,486	2,432,279	27.86	17.19
45	3.676505	4,748	116,919	1,786,956	24.62	15.28
50	3.644292	4,409	94,015	1,260,344	21.32	13.41
55	3.604475	4,022	72,919	843,806	18.13	11.57
60	3.555988	3,597	53,842	527,821	14.97	9.80
65	3.486461	3,065	37,152	301,421	12.12	8.11
70	3.393508	2,475	23,279	151,568	9.41	6.51
75	3.263163	1,833	12,471	63,578	6.80	5.10
80	3.025037	1,059	5,245.2	20,780	4.95	3.96
85	2.640633	437.1	1,599.7	5,075.5	3.66	3.17
90	2.071412	117.9	336.8	869.0	2.86	2.58
95	1.311480	20.49	46.4	94.8	2.26	2.04
100	0.343053	2.203	3.47	5.12	1.58	1.48

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* This comprehensive form was first given to the Life Table by Dr. Farr, the eminent English statistician. Valuable details respecting the properties and uses of the columns Q and Y (Table V.) and Z (Table VII.) may be found in the Sixth Report of the Registrar-General (Eng.)

The integration of the functions L_x, Q_x, L'_x, and Q'_x, to obtain the values in columns Q, Y, N, and Z respectively, was chiefly effected by the brief methods detailed in the Proceedings of the American Association for 1856. The ordinary process involves a preliminary and formidable interpolation of values at annual intervals of age, and a summation of the values thus obtained. In note B is offered a modification of the method previously given, especially adapted to the computation of values at the higher ages.

A large variety of useful problems may be solved by reference to the table above. We can now only advert to some of the more obvious of its properties.

According to the law of mortality for Massachusetts, it appears, that of 10,000 children born alive, 6,437 persons *will survive* age 20;

That these 6,437 persons *will live*, in the aggregate, 256,651 years;

That the *average* number of years which they will live is 39.86;

And that the *average* number of years which they *have lived* and *will live*, that is, the complete average duration of life, past and future, in years, is 59.86, that is, 20 + 39.86.

In a *stationary population*, supplied by 10,000 annual births, there will annually occur 6,437 *deaths* of persons at and over age 20.

These 6,437 persons *dying* will have lived, in the aggregate, 256,561 years over age 20.

The *average* number of years over age 20 which they will have lived is 39.86;

Their average age at death is consequently (20 + 39.86 =) 59.86 years.

In a *stationary population* supplied by 10,000 annual births, there will be 256,561 persons constantly living at and over age 20.

This generation of 256,561 persons *will live* in the aggregate 6,367,019 years;

They *have already lived* 6,367,019 years over age 20.

The *average* number of years which they *will live* is 24.82.

The *average* number which they *have lived*, over age 20, is 24.82 years; their average age is consequently 44.82 years; and the complete *average* duration, past and future, of the generation of persons now at and over 20 years of age, or their average age at death, is (44.82 + 24.82 =) 69.64 years.

In a stationary population there constantly will be *living*, to one annual death, 39.86 persons, at and over age 20.

In a community the members of which enter in constant and uniform numbers at age 20, and retire at age 60 or before in the event of death, the average number of years that the present members *will continue* with the community is 18.18; they *have* already *been* members 18.18 years; consequently their complete average duration of membership, past and future, is 36.36 years.

According to the English Life Table (1841) these numbers would be 18.23 and 36.46, respectively.

This case approximately represents that of a community of business men, if we assume that its members enter at about the age of 20 years in nearly equal annual numbers, and retire from active life about the age of 60 years, or before in case of decease.

This table will be found of practical utility, not only for the very valuable purposes of Life Insurance, but also to the statesman and to the political economist, in the solution of many important problems, among which may be mentioned those relating to the strength and the decadence of armies in time of peace, and to the influence of immigration and emigration on the growth of populations.

"The applications and uses of National Life Tables," says Dr. Farr,[*] "are almost innumerable: without an intimate knowledge of their properties it is impossible to determine the laws of population, which are the basis of statistics, or to reason upon such matters without falling into great errors, of which, if it were not invidious, too many instances might be cited from current works on population and public health."

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[*] Sixth Rep. Reg. Gen. p. 524

TABLE VI.—PREPARATORY TABLES FOR DETERMINING THE VALUES OF LIFE ANNUITIES, AND OF OTHER SINGLE LIFE BENEFITS, AT DIVERS RATES OF INTEREST FOR MONEY, ACCORDING TO THE MASSACHUSETTS LIFE TABLE.

Age.	3 per cent.		4 per cent.		5 per cent.	
	$L_x\left(\frac{1}{1·03}\right)^x$	$\Sigma_x^\infty L'_x$	$L_x\left(\frac{1}{1·04}\right)^x$	$\Sigma_x^\infty L'_x$	$L_x\left(\frac{1}{1·05}\right)^x$	$\Sigma_x^\infty L'_x$
x	L'_x	N_x	L'_x	N_x	L'_x	N_x
0	10,000	189,437	10,000	158,231	10,000	135,595
1	8,203	179,437	8,124	148,231	8,047	125,595
2	7,289	171,234	7,150	140,107	7,014	117,548
3	6,794	163,945	6,600	132,957	6,413	110,534
4	6,449	157,151	6,204	126,357	5,971	104,121
5	6,164	150,702	5,873	120,153	5,599	98,150
10	5,115	122,031	4,643	93,311	4,220	72,992
15	4,317	98,111	3,735	72,001	3,235	53,976
20	3,564	78,061	2,938	54,969	2,426	39,491
25	2,913	61,585	2,288	41,636	1,801	28,679
30	2,368	48,151	1,772	31,277	1,330	20,671
35	1,922	37,239	1,370	23,442	980.4	14,763
40	1,557	28,388	1,058	17,069	721.4	10,410.3
45	1,256	21,228	812.8	12,291.9	528.4	7,212.3
50	1,006	15,464	620.3	8,630.9	384.4	4,875.0
55	791.5	10,876.5	465.2	5,853.5	247.8	3,184.4
60	610.6	7,291.3	342.0	3,784.4	192.6	1,984.1
65	448.8	4,570.8	239.5	2,287.9	128.6	1,156.0
70	312.5	2,609.2	158.9	1,259.6	81.33	613.51
75	199.7	1,279.4	96.75	595.36	47.20	279.41
80	99.55	490.96	45.96	219.55	21.37	99.00
85	35.44	136.02	15.59	59.44	6.911	25.73
90	8.243	25.989	3.455	10.948	1.460	4.536
95	1.236	3.363	.494	1.3177	.1988	.5210
100	.1146	.235	.0436	.0891	.0168	.0341

TABLE VI. — *continued.* PREPARATORY TABLES FOR DETERMINING THE VALUES OF LIFE ANNUITIES, AND OF OTHER SINGLE LIFE BENEFITS, AT DIVERS RATES OF INTEREST FOR MONEY, ACCORDING TO THE MASSACHUSETTS LIFE TABLE.

Age.	6 per cent.		7 per cent.		8 per cent.	
	$L_x\left(\frac{1}{1.06}\right)^x$	$\Sigma_x^\infty L'_x$	$L_x\left(\frac{1}{1.07}\right)^x$	$\Sigma_x^\infty L'_x$	$L_x\left(\frac{1}{1.08}\right)^x$	$\Sigma_x^\infty L'_x$
x	L'_x	N_x	L'_x	N_x	L'_x	N_x
0	10,000	118,630	10,000	105,552	10,000	95,219
1	7,971	108,630	7,896	95,552	7,823	85,219
2	6,882	100,659	6,754	87,656	6,630	77,396
3	6,234	93,777	6,060	80,902	5,894	70,766
4	5,749	87,543	5,537	74,842	5,335	64,872
5	5,340	81,795	5,095	69,305	4,863	59,537
10	3,838	58,193	3,494	47,141	3,184	38,703
15	2,806	41,207	2,438	31,948	2,120	25,099
20	2,007	28,865	1,663	21,411	1,381	16,091
25	1,421	20,077	1,124	14,256	890.7	10,250.8
30	1,001	13,870	755.1	9,433	571.3	6,493.1
35	703.6	9,502	506.5	6,194.8	365.8	4,085.1
40	493.7	6,433	339.1	4,024.2	233.8	2,544.1
45	344.9	4,283.5	226.1	2,572.9	148.7	1,560.8
50	239.3	2,784.7	149.7	1,607.3	94.00	936.60
55	163.2	1,750.7	97.36	971.6	58.37	544.23
60	109.1	1,050.2	62.08	560.91	35.53	302.20
65	69.44	589.31	37.72	303.09	20.60	157.12
70	41.89	301.41	21.71	149.33	11.32	74.545
75	23.19	132.22	11.47	63.11	5.707	30.335
80	10.01	45.00	4.724	20.62	2.245	9.524
85	3.088	11.230	1.390	4.94	.6304	2.191
90	.6221	1.894	.2672	.7973	.1157	.338
95	.0808	.2078	.0331	.0862	.01368	.0350
100	.0065	.0132	.0025	.0051	.00100	.00200

TABLE VII. — FOR DETERMINING THE AVERAGE VALUES OF LIFE ANNUITIES, AND OF OTHER SINGLE LIFE BENEFITS, ON THE WHOLE OF A STATIONARY POPULATION, OR ON THE PART AT AND OVER CERTAIN AGES, ACCORDING TO THE MASSACHUSETTS LIFE TABLE.

Interest of Money. — Five per cent.

Ages.	$Q_x\left(\frac{1}{1\cdot05}\right)^x$ Q'_x	$\Sigma_x^\infty Q'_x$ Z_x	$\frac{Z_x}{Q'_x}-1$ a'_x ANNUITY.
0	397,653	5,682,478	13.29
1	370,148		
2	345,230		
3	322,263		
4	300,885		
5	280,918	3,946,299	13.05
10	198,606	2,717,810	12.68
15	139,258	1,851,426	12.29
20	96,695	1,246,093	11.89
25	66,504	827,263	11.44
30	45,254	540,311	10.94
35	30,402	345,934	10.38
40	20,097	216,087	9.75
45	13,013	130,868	9.06
50	8,198	76,207	8.30
55	4,982	42,192	7.47
60	2,882	21,860	6.59
65	1,558	10,360	5.65
70	765	4,332	4.66
75	321	1,510	3.71
80	105.8	417.4	2.95
85	25.3	83.2	2.29
90	4.2	12.4	1.95
95	.5	1.2	1.49
100	.03	.06	1.00

The average of the present values of one dollar, payable at the close of each year during the continuance of each of the lives of the persons now at and over the age of 20 years, *in a stationary population*, interest of money being computed at the rate of 5 per cent. per annum, is $11.89. [*Table* VII.]

EXAMPLES. — The present value of one dollar, payable at the close of each year during the continuance of the life of a person now 20 years of age, interest of money being computed at the rate of

$$\left\{\begin{array}{l}\text{4 per cent. per annum, is \$17.71} \\ \text{5 per cent. per annum, is \$15.28}\end{array}\right.$$

[*Table* VIII.]

Our L'_x is commonly written D_x.

The N_x is that used by Dr. Farr and a few other late writers, and is equivalent to the N_{x-1} introduced by Mr. Griffith Davies, and adopted in certain standard treatises on life annuities and reversions.

Q, Q', Y, and Z retain the same signification as in the Reports of the English Registrar-General.

i is any annual rate of interest for money; as, ·03, ·04, or ·05.

Formulas for determining values of annuities, annual premiums, and single premiums, are given in the headings of the respective columns in which those values appear. [Table VIII.]

$$L'_x = L_x\left(\frac{1}{1+i}\right)^x$$

$$Q'_x = Q_x\left(\frac{1}{1+i}\right)^x.$$

N_x (which equals $\Sigma_x^\infty L'_x) = L'_x + L'_{x+1} + L'_{x+2} + \cdots L'_\infty$, and represents the aggregate present values of a constant sum, the $\left(\frac{1}{1+i}\right)^x$ portion of one dollar, payable at the beginning of each year, during the continuance of each of the lives of the L_x persons living *at* the age x.

Z_x (which equals $\Sigma_x^\infty Q'_x) = Q'_x + Q'_{x+1} + Q'_{x+2} + \cdots Q'_\infty$, and represents the aggregate present values of a constant sum, the $\left(\frac{1}{1+i}\right)^x$ portion of one dollar, payable, at the beginning of each year, during the continuance of each of the lives of the Q_x persons living in a stationary population *at and over* age x.

d_x $\left(\text{which equals } \frac{Z_x}{Q'_x \frac{1}{(1+i)^x}} - 1\right)$ represents the average of the present values of one dollar, payable, at the beginning of each year, during the continuance of the lives of each of persons living (Q_x) in a stationary population at and over age x.

A column, represented by the well-known symbol M_x, may be constructed from values in columns L'_x and N_x by the following simple formula:

$$M_x = L'_x - \frac{i}{1+i} N_x.$$

M_x represents the aggregate present values of a constant sum, the $\frac{1}{(1+i)^x}$ portion of one dollar, payable at the end of each of the years in which the deaths of the L_x persons living at the age x will occur.

For methods for deducing from the above the values of other single life benefits, the reader is referred to the writings of Mr. David Jones, in his work on "Annuities and Reversionary Payments," of Professor De Morgan, in the Companions to the British Almanac for 1840 and 1842, and of Dr. Farr, in the Sixth and the Twelfth Reports of the Registrar-General (Eng.). These benefits may be uniform or variable, and may apply either to the entire period of life or to limited portions.

Tables, for determining the values of benefits contingent upon a combination of lives, may be framed by brief processes, in some degree analogous to those already indicated.

TABLE VIII.

Ages.	LIFE ANNUITY. — The present value, after arriving at a certain age, of one dollar, payable at the end of each year during life.		ANNUAL PREMIUM UNAUGMENTED. — The sum payable at the beginning of each year, after arriving at a certain age, which will amount to one hundred dollars at the end of the year of decease.		SINGLE PREMIUM UNAUGMENTED. — The present value, after arriving at a certain age, of one hundred dollars, payable at the end of the year of decease.	
	$a_x = \frac{N_x}{L_x} - 1.$		$\pi_x = 100\left(\frac{1}{1+a_x} - \frac{i}{1+i}\right).$		$V_x = 100\left(1 - \frac{i}{1+i}(1+A_x)\right).$	
	4 per cent.	5 per cent.	4 per cent.	5 per cent.	4 per cent.	5 per cent.
0	14.824	12.560	2.47	2.61	39.14	35.43
1	17.247	14.608	1.63	1.65	29.82	25.68
2	18.595	15.759	1.26	1.21	24.64	20.20
3	19.145	16.236	1.12	1.04	22.52	17.92
4	19.367	16.438	1.06	.97	21.67	16.96
5	19.459	16.530	1.04	.94	21.31	16.52
10	19.097	16.297	1.13	1.02	22.70	17.64
15	18.277	15.685	1.34	1.23	25.86	20.55
20	17.710	15.278	1.50	1.38	28.04	22.48
25	17.198	14.924	1.65	1.52	30.01	24.17
30	16.651	14.542	1.82	1.67	32.11	26.99
35	16.111	14.058	2.00	1.88	34.19	28.30
40	15.133	13.431	2.35	2.17	37.95	31.28
45	14.123	12.649	2.77	2.57	41.83	35.00
50	12.914	11.682	3.34	3.12	46.49	39.61
55	11.583	10.588	4.10	3.37	51.60	44.82
60	10.065	9.301	5.19	4.95	57.44	50.95
65	8.553	7.989	6.62	6.36	63.26	57.19
70	6.927	6.543	8.77	8.50	69.51	64.08
75	5.154	4.920	12.40	12.13	76.33	71.81
80	3.777	3.633	17.09	16.82	81.63	77.94
85	2.813	2.723	22.38	22.10	85.33	82.27
90	2.169	2.101	27.71	27.50	87.81	85.24
95	1.670	1.621	33.61	33.40	89.73	87.49

TABLE IX. — COMPARISON OF THE PRESENT VALUES OF A WIDOW'S RIGHT OF DOWER IN THE INCOME OF AN ESTATE WORTH $1,000, COMPUTED ACCORDING TO THE MASSACHUSETTS, THE ENGLISH, AND THE PRUSSIAN LIFE TABLES.

Ages.	MASSACHUSETTS, 166 Towns, 1855.		ENGLAND, Females, 1841.	PRUSSIA, 1839-40-41.
	5 per cent.	4 per cent.	4 per cent.	4 per cent.
25	251	281	235	226
35	237	217	216	202
45	214	191	191	170
55	180	158	155	130
65	138	118	113	92
75	88	74	74	65
85	52	43	45	45

In computing the above table, the widow's interest in the estate was supposed to continue until the moment of decease. Such tables have been sometimes framed on the assumption that the claim was to cease with the end of the year preceding that in which the death should occur.

We observe a close resemblance between the values from the Massachusetts data, and those derived from the table that expresses the law of mortality that prevails over the females of England.

The values from Prussian data are usually less than those from the English and the American observations.

We now give tables comparing the newly determined law of mortality for Massachusetts, in some of the forms in which it has been presented, with the laws which prevail over the populations of several of the communities of Europe.

The ratios of deaths to population, in Tables X. and XI. do not, in all cases, admit of direct and exact comparison, owing to want of uniformity in the intervals of age. Their relations, however, are sufficiently obvious for our present purpose. If curves be traced, to which the ratio of the number of the living to *one* annual death, at each of the intervals of age, and the age of the middle of the interval shall be coördinates, the relative vitality of the several communities at every age of life may be readily compared, and with sufficient approach to exactness.

TABLE X.—MORTALITY, PER CENT., OR, THE NUMBER OF DEATHS TO 100 PERSONS LIVING, IN DIVERS COMMUNITIES, COMPARED.

Ages.	MASSACHUS'TS, 166 Towns, 1855.	ENGLAND AND WALES,* Seven Years, 1838-44.		SWEDEN,* Thirty Years, 1811-40.		CARLISLE,† Nine Years. 1779-87.
	Persons.	Males.	Females.	Males.	Females.	Persons.
0–5	7.32	7.07	6.04	7.28	6.27	8.23
5–10	.78	.93	.90	.83	.78	1.02
10–15	.43	.50	.55	.52	.49	.50
15–20	.88	.70	.79	.54	.53	.68
20–30	1.13	.94	.94	.90	.73	.75
30–40	1.24	1.09	1.13	1.31	1.06	1.06
40–50	1.41	1.45	1.32	1.96	1.42	1.43
50–60	2.02	2.26	1.98	3.09	2.30	1.83
60–70	3.67	4.28	3.79	5.66	4.72	4.12
70–80	7.98	9.22	8.42	11.81	10.54	8.30
80–90	18.39	20.11	18.32	25.63	23.01	17.57
90 and over	34.66	36.53	34.58	42.15	39.72	28.44
All ages	2.14	2.27	2.10	2.56	2.28	2.50

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* From a paper by T. R. Edmonds, Esq., published in the numbers of "The Lancet" (London) for the 9th and the 16th of March, 1850.

† Derived from values on page 418 of Mr. Milne's Treatise on "Annuities and Assurances."

TABLE XI. — MORTALITY PER CENT., OR THE NUMBER OF DEATHS TO 100 PERSONS LIVING, IN DIVERS COMMUNITIES, COMPARED.

Ages.	BELGIUM	ENGLAND AND WALES.*		SWEDEN. †	Ages.	Ages.	PRUSSIA ‡
	9 years, 1842–50. Persons.	7 years, 1838–44. Mean of Males and Females.	10 years. 1845–54. Mean of Males and Females.	20 years, 1821–40. Mean of Males and Females.			8 years, 39,40,41. Persons.
0–1	20.11	17.92		19.84	0–1		
1–2	7.19	6.55		3.80	1–3		
2–3	3.78	3.51					
3–4	2.61	2.50		1.56	3–5		
4–5	1.80	1.84					
0–5	6.99	6.54	6.85	6.43	0–5	0–5	8.02
5–10	1.09	.91	.91	.76	5–10	5–7	1.52
10–15	.72	.53	.53	.47	10–15	7–14	.78
15–20	.87	.82	.85	.59	15–25	14–20	.63
20–25	1.04					20–25	.89
25–30	1.05	.99	1.05	.97	25–35	25–30	.97
30–35	1.08					30–35	1.08
35–40	1.21	1.25	1.30	1.42	35–45	35–40	1.32
40–45	1.44					40–45	1.45
45–50	1.56	1.66	1.76	2.06	45–55	45–55	2.10
50–55	2.08						
55–60	2.75	2.95	3.04	3.57	55–65	55–60	3.57
60–65	2.77					60–65	5.58
65–70	5.38	6.22	6.43	7.61	65–75	65–75	9.09
70–75	8.41						
75–80	11.69	13.74	14.32	16.93	75–85	75–85	15.15
80–85	16.57						
85–95	22.70	28.42	29.19	32.60	85–95	85 & upw.	26.62
95 & upw.	25.79	41.46	45.22	43.64	95 & upw.		
All ages	2.42	2.19	2.28	2.34			2.70

* Ninth Rep. Reg. Gen., p. 177, and Seventeenth Rep. Reg. Gen., p. xvi.

† Eighth Rep. Reg. Gen. (Eng.), p. 276.

‡ Proceedings Am. Assoc. for the Adv. of Science, 1856, p. 56.

Table XII. — Proportions born and surviving certain Ages in divers Communities, compared.

	Massachu'ts, 166 towns. 1855.	England and Wales, 1841. Farr.	Carlisle, 1779-87. Milne.	Prussia, 1839, 40, 41. Elliott.	Sweden and Finland, 1801-5. Milne.	Belgium, 1842-50. Elliott.
0	10,000	10,000	10,000	10,039	10,000	10,000
1	8,449	8,537	8,461	8,294	8,112	8,504
2	7,733	8,010	7,779	7,721	7,659	7,918
3	7,424	7,739	7,274	7,364	7,403	7,625
4	7,258	7,554	6,998	7,147	7,226	7,429
5	7,146	7,420	6,797	6,992	7,096	7,296
10	6,873	7,061	6,460	6,589	6,729	6,912
15	6,726	6,863	6,300	6,385	6,558	6,671
20	6,437	6,606	6,090	6,165	6,377	6,386
30	5,748	6,033	5,642	5,641	5,918	5,754
40	5,078	5,383	5,075	5,008	5,369	5,130
50	4,409	4,662	4,397	4,243	4,647	4,413
60	3,597	3,800	3,643	3,141	3,590	3,464
70	2,475	2,453	2,401	1,573	2,163	2,185
80	1,059	938	953	444	644	787
90	118	115	142	50	49	110
100	2	1	9	1	0	5

Table XIII. — Average Future Duration of Life in certain Communities, compared.

Ages.	Massachusetts.	England and Wales.			Sweden and Finland.	Prussia.	Carlisle.
	1855.	1841.		1838-44.	1801-05.	1839, 40, 41.	1779-87.
	Persons.	Males.	Females.	Males.	Persons.	Persons.	Persons.
0	39.8	40.2	42.2	40.4	39.4	36.7	38.7
5	50.2	49.6	50.4	50.2	50.0	47.1	51.3
10	47.1	47.1	47.8	47.5	47.6	44.8	48.8
15	43.0	43.4	44.1	43.6	43.8	41.2	45.0
20	39.9	39.9	40.8	40.0	40.0	87.5	41.5
25	36.9	36.5	37.5	36.6	36.3	34.0	37.9
30	34.0	33.1	34.2	33.2	32.7	30.6	34.3
35	31.0	29.8	31.0	29.8	29.1	27.1	31.0
40	27.9	26.6	27.7	26.5	25.5	23.8	27.6
45	24.6	23.3	24.4	23.1	22.1	20.4	24.5
50	21.3	20.0	21.1	19.9	18.7	17.1	21.1
55	18.1	16.7	17.6	16.7	15.6	14.0	17.6
60	15.0	13.6	14.4	13.6	12.6	11.2	14.3
65	12.1	10.9	11.5	10.9	9.9	9.0	11.8
70	9.4	8.5	9.0	8.6	7.5	7.4	9.2
75	6.8	6.6	6.9	6.6	5.7	6.0	7.0
80	5.0	4.9	5.2	5.0	4.2	4.8	5.5
85	3.7	3.7	3.8	3.7	3.2	3.8	4.1
90	2.9	2.7	2.8	2.8	2.4	3.0	3.3
95	2.3	2.0	2.1	2.1	1.7		3.5

TABLE XIV.—AVERAGE FUTURE DURATION OF LIFE OF A GENERATION, OR OF THOSE LIVING AT AND OVER CERTAIN AGES, IN A POPULATION CONSIDERED STATIONARY.

Ages.	MASSACHUSETTS.	ENGLAND AND WALES.*		
	1855.	1841.		1838–44.
	Persons.	Males.	Females.	Males.
0	32.3	31.9	32.5	32.0
10	28.6	28.2	28.7	28.2
20	24.8	24.2	24.8	24.2
30	21.0	20.3	20.9	20.3
40	17.2	16.4	17.0	16.4
50	13.4	12.7	13.2	12.7
60	9.8	9.2	9.6	9.2
70	6.5	6.3	6.5	6.3
80	4.0	4.0	4.1	4.0
90	2.6	2.4	2.5	2.5

* Sixth and Twelfth Reports Reg. Gen.

From inspection of Tables X. and XI. it appears (so far as the data show) that, from a point below age 5 to about age 15, *lower* rates of mortality obtain in Massachusetts than generally in European communities; that, from age 15 to divers ages between 35 and 50, the Massachusetts rates are much *higher;* after which they again *fall* somewhat *below* the European. Under the age of *five* years, mortality in Massachusetts seems more intense than in Europe generally, though less so than in Prussia, and less than was experienced in the town of Carlisle during the nine years, 1779–87, which period was before the introduction of vaccination.

In the first of the above-mentioned intervals (say from age 3 to age 15), the mortality of Massachusetts approaches more closely to that of Sweden than to those of the other European communities.

In the second of the intervals (from about age 17 to 45), it more nearly represents the mortality of Belgium, though *higher;* and from age 45 onwards, it is *lower* than, but nearer to, the average English rates, not varying greatly from the mortality of the females of England.

The mortality of Massachusetts appears to be *lower* than that of

Carlisle previous to about age 17, thence generally *higher* to about age 60, *lower* to about age 80, and *higher* from that point onward.

As a whole, the mortality of the State is better represented by that of England, than of any European country.

From about age 3 to age 35 the mortality of Sweden appears to be *lower* than that of England, and after age 35, *higher*.

In Prussia, with the exception of the intervals between the ages 15 and 25, and between ages 85 and 95, the mortality is uniformly *higher* than in England.

Through much of the interval under the age of 8 years, the mortality of Belgium closely resembles that of England; from that age to about 55 it is *higher*, thence to 85 nearly the same, and above that point, *lower*.

The Belgic rate is higher than the Prussian from age 15 to 32, beyond which point it is generally the lower.

At birth, the *average future duration of life* in Massachusetts (see Table XIII.) appears to be slightly less than in Sweden. From age 5 to age 25 inclusive, it agrees well with that of the males of England, and also with that of the population of Sweden. From age 30 onwards to advanced age, it is usually best represented by that of the females of England.

For much of the period from age 30 to age 75 inclusive, the Carlisle and the Massachusetts results do not greatly differ. Our comparisons have been made with national life tables and with the Carlisle table. The latter is introduced because of its extensive employment in this country and in Europe for insurance and in legal proceedings.

We observe, according to the Massachusetts life table, that of all born alive, somewhat less than one in six (.155) die before arriving at the age of *one* year; that one fourth (.26) die before attaining the age of *three* years; that seven tenths (.71) survive the age of *five* years; one half (.51), the age of *forty* years; one fourth (.25) the age of *seventy* years; one tenth (.11), the age of *eighty* years; and that one of every hundred born alive reaches the advanced age of *ninety* years. Great reliance cannot be reposed in conclusions respecting extreme longevity derived from the data employed in the construction of any of the tables, whether European or American, both in consequence of the less reliable character of the returns at those ages, and of their limited number.

We have seen that in Massachusetts a greater disparity exists than in European countries, between the rates of mortality at the ages of 5 to 15, and the rates from age 15 to about 45.

In the towns of Massachusetts selected, the rate of mortality at the ages of 5 to 15 was but little more than one half (.55) of the rate at the ages of 15 to 40. In England, in 1841, the rate of mortality in the former interval of age was about three fourths (.78) of the rate in the latter interval.

A similar disparity is observable on comparing the returns of deaths for the entire State for the *six* years, 1850–55, with the average of the numbers living at different ages according to *two* enumerations, — the one ordered in connection with the national census for the 1st of June, 1850, and the other in connection with the State census for the 1st of June, 1855. In the six years mentioned (1850–55), the rate of mortality in the entire State, according to the returns, at the ages of 5 to 15, was fifty-six one hundredths (.56) of the rate at the ages of 15 to 40. This ratio (.56) is *almost identical* with (.55) that of the towns selected in 1855, and strengthens the conclusion, that the feature under consideration prevails in the law of mortality of the population of the State.

The returns of deaths for the six years (1850–55) probably comprise but about *eighty-five* per cent. of the actual deaths of the period.

In the foregoing pages has been presented the Life Table for Massachusetts, with divers tables deduced therefrom. Among the more important of the latter may be enumerated: tables of average future duration of life; preparatory tables for finding the values of annuities and other single life benefits, calculated at *six* different rates of interest; and tables of life annuities, annual premiums, and single premiums at two rates of interest. Tables also have been given *comparing* the rates of mortality, the proportions living at certain ages according to the Life Table, and the average future duration of life in Massachusetts, with corresponding values in several European countries.

We defer for the present a comparison of the new results with those derived from other American observations, and with those from observations respecting select classes of lives.

We append two notes, — the former (Note A) giving the formula employed in calculating the influence of immigration and emigration on the population under the age of *five* years, preparatory to the determination of the values in the Life Table under that age; the latter (Note B) presenting the methods employed in constructing, by summary processes, from the Life Table, other tables of practical value.

NOTE A.—In a community unaffected by migration, and in which the births increase by a constant ratio, the following formula expresses the relation which holds between the number of births (L_0) in a given year, their annual ratio of increase $\left(\frac{1}{v}\right)$, the function which determines the number of deaths ($D_{0/x}$) under any age ($_x$) in the same year, according to the prevailing invariable law of mortality, supposed invariable, and the number of those living ($P_{0/5}$) under the age of five years in the middle of that year.

$$L_0 = \left\{ P_{0/5} + \int_0^5 \frac{v^{5-x}\, D_{0/x}}{\int_{-\frac{1}{2}}^{\frac{1}{2}} v^x} \right\} \frac{\int_{-\frac{1}{2}}^{\frac{1}{2}} v^x}{\int_0^5 v^x}.$$

Since the value of $d_x \int_{-\frac{1}{2}}^{\frac{1}{2}} v^x$ closely approximates unity, for the above formula may be substituted

$$L_0 = \frac{P_{0/5} + dx \int_0^5 v^{5-x}\, D_{0/x}}{dx \int_0^5 v^x}.$$

When the births are *constant*, the expression becomes

$$L_0 = \frac{P_{0/5} + dx \int_0^5 D_{0/x}}{5}.$$

This relation is more fully discussed in the Proceedings of the American Association for 1856.

$$\int_0^5 v^x\, dx = \frac{v^5 - 1}{\text{Nap. log. } v.} = \frac{v^5 - 1}{\text{Com. log. } v.} \times \cdot 4342945.$$

To obtain $\int_0^5 v^{5-x}\, D_{0/x}\, dx$, when v and $D_{0/1}$, $D_{0/2}$, $D_{0/3}$, $D_{0/4}$, and $D_{0/5}$ are given, first determine the values of $v^4\, D_{0/1}$, $v^3\, D_{0/2}$, $v^2\, D_{0/3}$, and $v\, D_{0/4}$.

Then putting

S_5 for $D_{0/5} + v\, D_{0/4} + v^2\, D_{0/3} + v^3\, D_{0/2} + v^4\, D_{0/1}$,
S_4 for $v\, D_{0/4} + v^2\, D_{0/3} + v^3\, D_{0/2} + v^4\, D_{0/1}$,
S_3 for $v^2\, D_{0/3} + v^3\, D_{0/2} + v^4\, D_{0/1}$,

and assuming an algebraic law of relation to connect the values S_2, S_4, and S_3, we have

$$\int_0^5 v^{5-x}\, D_{0/x}\, dx = \frac{S_5 + S_4}{2} - \frac{D_{0/5} - v\, D_{0/3}}{24}.$$

NOTE B.—ON METHODS EMPLOYED IN THE CONSTRUCTION OF CERTAIN TABLES.

$$Q_x = dx \int_x^\infty L_x.$$

$$Y_x = dx \int_x^\infty Q_x.$$

$$N_x = \Sigma_x^\infty L'_x = \Sigma_x^\infty \frac{L_x}{(1+i)^x},$$

$$Z_x = \Sigma_x^\infty Q'_x = \Sigma_x^\infty \frac{Q_x}{(1+i)^x}.$$

On account of the obvious similarity of construction of Q and Y, and also of N and Z, we need only present the methods adopted in deducing Q from L, and N from L'.

From age five onwards to advanced age, the values of L and L' are given quinquennially; from birth to age five, annually. The construction of the values in columns Q and Y, at ages earlier than five years, differs. Let S_x and S'_x represent the sum of the values of L_x and L'_x respectively, at and over any age x, at equidistant intervals of n years; that is, let

$$S_x = L_x + L_{x+n} + L_{x+2n} + \cdots\cdot$$

and

$$S'_x = L'_x + L'_{x+n} + L'_{x+2n} + \cdots\cdot$$

L_x and L'_x are general terms of series of *positive* values, that ***vanish*** when x is taken sufficiently great.

We remark, that

$$dx \int_x^\infty L_x = dx \int_x^{x+n} S_x,$$

and

$$\Sigma_x^\infty L'_x - \frac{L'_x}{2} = \Sigma_x^\infty S'_x - \frac{S'_x}{2}.$$

We then assume the following formulas of integration,

$$Q_x \left(\text{or } dx \int_x^\infty L_x, \text{ which equals } dx \int_x^{x+n} S_x \right),$$

equals

$$\text{(A)}\qquad n\left(\frac{S_x+S_{x+n}}{2}-\tfrac{1}{12}\cdot\frac{m\,D_x+D_{x-n}}{m+1}\right);$$

and

$$N_x\left(\text{or } \Sigma_x^{\infty} L'_x,\text{ which equals } \Sigma_x^{x+n} S'_x-\frac{S'_x}{2}+\frac{L'_x}{2}\right),$$

equals

$$\text{(B)}\qquad n\left(\frac{S'_x+S'_{x+n}}{2}+\frac{L'_x}{2}-\frac{n^2-1}{12\,n}\cdot\frac{m\,D'_x+D'_{x-n}}{m+1}\right),$$

in which

$$D_x=L_x-L_{x+n},$$

and

$$D'_x=L'_x-L'_{x+n};$$

Let m equal *unity*, when the ratio, $\frac{D_{x-n}}{D'_x}$ or $\frac{D'_{x-n}}{D'_x}$, is *greater* than $\frac{4}{9}$, and *less* than $2\frac{1}{4}$.

$$\text{Let } m \text{ equal}\left\{\begin{array}{lll}
2, & & 2\frac{1}{4} \text{ and } 3\frac{1}{2}\\
3, & \text{when the ratio is between} & 3\frac{1}{2} \text{ and } 5\frac{1}{2}\\
4, & & 5\frac{1}{2} \text{ and } 7\frac{1}{2}\\
5, & & 7\frac{1}{2} \text{ and } 9\frac{1}{2}\\
5\frac{1}{2} & \textit{tenths of the ratio}\text{, when the} & 9\frac{1}{2} \text{ and } 16\\
5 & \text{ratio is between} & 16 \text{ and } 40\\
4 & & 40 \text{ and } 250\\
\multicolumn{3}{l}{3\ \textit{tenths of the ratio}\text{, when the ratio exceeds 250.}}
\end{array}\right.$$

These ratios, except at quite advanced ages, will commonly be such that m will equal *unity*, and the values of Q and N will not then differ from those that result from the assumption of an *algebraic* law of relation connecting the *four* values of S_x or S'_x, at the ages $x-n$, x, $x+n$, and $x+2n$.

If in (A) for S_x+S_{x+n} the sum of the values of S_x at the limiting ages x, and $x+n$, we put G, and for $\frac{m\,D_x+D_{x-n}}{m+1}$ we put H; and in (B), in like manner, put G' and H', we shall have

$$\text{(C)}\qquad Q_x=n\left(\frac{G}{2}-\frac{H}{12}\right),$$

and

$$\text{(D)}\qquad N_x=\frac{n\,G'}{2}+\frac{L'_x}{2}-\frac{n^2-1}{12\,n}\,H'.$$

When it is desired to determine the values of Q or N from but *three* given equidistant values of S, or S', for H or H' we put the second difference of the three values; this is equivalent to assuming that the three values are connected by an *algebraic* law of relation.

If in (C) we let H be *zero,* Q_x becomes merely the product of the average $\left(\frac{G}{2}\right)$ of the values of S_x at the limiting ages x, and $x + n$, by (n) the number of years in the interval; and is equivalent to assuming that a law of arithmetical progression connects the values of S_x within the limits.* When the interval of age is quinquennial, $\frac{n^2 - 1}{12\,n}$ equals $\frac{4}{10}$.

The operations in (C) and (D) may receive verbal interpretations.

To obtain Q_x; from the average of the limiting values $\left(\frac{S_x + S_{x+n}}{2}\right)$ of S_x, subtract one twelfth $(\frac{1}{12})$ of *a mean* (H) of the second differences $(D_{x-n}$, and $D_x)$ of the four consecutive values $(S_{x-n}, S_x, S_{x+n}$, and $S_{x+2n})$ of S_x, one of which (S_{x-n}) shall precede, and another (S_{x+2n}) follow the values at the limiting ages (x and $x - n$), and multiply by the number of years (n) in the interval of age.

To obtain N_x; multiply the average of the limiting terms of S'_x by the number of years (n) in the interval of age, add one half of the value of L', corresponding to the age, and subtract $\frac{n^2 - 1}{n}$ twelfths of *a mean* (H') of the second differences $(D'_{x-n}$ and $D'_x)$ of the four values of S'_x at the ages $x - n$, x, $x + n$, and $x + 2n$.

We remark that H and H' are *arithmetical* means *only* when m equals *unity;* in other cases the greater weight is commonly given to the less of the second differences.

By giving to m the values which we have mentioned above, we are enabled readily, and without resort to logarithmic tables, to arrive at values that closely approximate those that would have resulted from the integration of the exponential function

$$a + bx + c\,d^x$$

which may be assumed to equal S_x or S'_x. a, b, c, and d are constants to be determined from the *four* values of the functions (S_x or S'_x) corresponding to the specified ages $x - n$, x, $x + n$, and $x + 2\,n$.

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* This very simple form does not differ essentially from that given by Dr. Farr in the Fifth Report of the Reg. Gen. (Eng.), and is sufficiently accurate for the earlier ages, if the uniform interval of age (n) is not larger than quinquennial.

d will, in all cases, be *positive*, and the curve represented by above exponential function, if referred to rectangular coördinates, will have *no point of contrary flexure.*

If S_x or S'_x be represented by the algebraic function

$$a + bx + cx^2 + dx^3,$$

the curve, to which the above is the equation, if referred to rectangular coördinates, will have *a point of contrary flexure* within the limits of the ages $x - n$, and $x + 2n$, whenever the ratios of the second differences $\left(\frac{D_{x-n}}{D_x}\right)$ of the values of S_x corresponding to the ages $x - n$, x, $x + n$, and $x + 2\,n$ is *greater* than 2, or *less* than $\frac{1}{2}$; and the larger the ratio, if greater than 2, or the smaller the ratio, if less than $\frac{1}{2}$, the more eccentric the curve.

If in (A) we give to m the value

$$\frac{(r - 1 - 12\,\delta)\,r}{12\,\delta\,r - r + 1},$$

in which

$$r = \frac{D_{x-n}}{D_x},$$

and

$$\delta = \tfrac{1}{2}\,\frac{r + 1}{r - 1} - \frac{1}{\text{Nap. log.}\,r},$$

we shall obtain for Q_x precisely the values that would have resulted from the direct integration within the limits x and $x + n$, of the exponential expression,

$$S_x = a + b\,x + c\,d^x.$$

Above age 5, the values of Q_x were formed by successively adding to the previously determined value of Q_5, the values of the definite integrals of L_x for the ages 4 to 5, 3 to 4, 2 to 3, and 1 to 2, determined according to algebraic laws of relation, involving, in the first case (that from 4 to 5), *three*, and in the other cases *four* of the given equidistant values of L_x. The integral from *birth* to age 1 was determined by assuming that the values at ages 0, 1, and 2 were connected by the parabolic law of relation,

$$L_x = L_0 - (L_0 - L_1)\,x^b,$$

in which b obviously equals

$$\frac{\log.\,(L_0-L_2)-\log.\,(L_0-L_1)}{\log.\,2}.$$

The value of $\int_0^{x} L_x\,dx$, the required integral, is

$$x\left(L_0-\frac{L_0-L_1}{1+b}\right).$$

5. On a New Form of Arithmetical Complements. By Thomas Hill, of Waltham, Mass.

If we give the name of arithmetical supplement to the arithmetical complement diminished by one, or, in other words, to the complement obtained by subtracting each digit of a number, zeros included, from the highest digit of the system; (that is, in decimal notation from nine) then the following theorem is manifestly true.

If from the supplement of any whole number we subtract the same number that we add to the whole number, the sum and difference thus obtained are supplements of each other.

Thus $1863 + 857 = 2720$ and $8136 - 857 = 7279$; and 1863 is the supplement of 8136, and 2720 of 7279. These supplements may be used in arithmetical machines by printing the supplement of each digit in a smaller type by its side, so that we add by looking at the larger figures, and subtract by looking at the smaller. Thus the example already given may be printed

$$1_8 8_1 6_3 3_6 \pm 857 = 2_7 7_2 2_7 0_9.$$

Thinking that possibly other uses might be found for them, I have thus called the attention of computers to them.

II. ASTRONOMY.

1. ZODIACAL LIGHT.* By CHARLES WILKES, U. S. N., Washington, D. C.

OBSERVATIONS on the phenomenon of the Zodiacal Light were assigned as part of the duties of the Exploring Expedition, and claimed particular attention during the continuance of the voyage. In order to insure that no opportunity should be lost, and that every advantage might be taken of the opportunities when they occurred, they were made a part of the duties of the officers of the watch, with instructions when this phenomenon was visible to report it to the commander, to observe the altitude of its apex, subtension of the base, note its central line among the stars and its boundary as defined by them; the phases of light which it exhibited, as well as the variations which it underwent, with the hour and minute of the observation; and lastly, to draw a diagram of its appearance.

The number of observations taken by others and myself in the squadron were about one hundred and fifty reliable ones. For some of these I am indebted to members of the Scientific Corps, and particularly to Professor James D. Dana, the Geologist of the Expedition.

The first time we passed through the tropics, our observations were few, the weather being unfavorable, and the sky, for the greater part of the time, obscured. The phenomenon, though often seen, was illy defined, and afforded no well-marked outline of its extent or appearance. Its central line was seen to correspond with the ecliptic, and it was observed to change its phase and azimuth on each successive observation as we passed rapidly to the southward. Afterwards, on our various routes during the voyage through the low latitudes, we were more fortunate, and very many opportunities were offered of making observations, which we endeavored to improve. Nevertheless, I regretted the loss of the first opportunities. Had the observations been continuous, I should be enabled to compare advantageously its extent, outline, and phases with the observations as we returned in an opposite direction; notwithstanding, I feel satisfied that I have obtained sufficient data to throw much light on this phenomenon — enough, I trust, to disprove the theories heretofore advanced respecting it, and to explain satisfactorily its cause.

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* This paper on the Zodiacal Light has been prepared for several years as a part of the results of the Exploring Expedition to the South Seas. Its publication has been delayed until the volume of which it forms a part was printed by the government. The delay has placed within my reach many observations which have been made since, all of which have tended to confirm me in the belief that the explanation I offer for this phenomenon, which has so long interested astronomers, is the true and only one which will meet all the facts derived from observations.

All the observations made on the Zodiacal Light, since it first attracted attention (nearly two centuries ago), prove that its phases and general appearance have not changed.

The theories which have been entertained of the Zodiacal Light have been various. Some derive it from the atmosphere of the sun; that it is illuminated matter thrown off from his equator, revolving with immense velocity, which takes a lenticular shape; others, that it is a nebulous ring, with the sun for its centre, extending near to or beyond the earth's orbit; and again, another hypothesis supposes that this nebulous ring has the earth for its centre; and others again surmise that this phenomenon is nebulous matter floating in space, to which the periodical showers of stars may be traced, as the earth happens in its orbit to pass through or encounter them. It seems impossible to reconcile any of these hypotheses or surmises with the facts which close observation has developed.

The theories referred to fail to satisfy us of their correctness, as they are soon perceived to be inconsistent with the facts. It would be unnecessary here to enter into any discussion of the subject of either of them; that which has been of late offered by one who has been so persevering and continuous an observer, has been so effectually disproved by the able pen of the President of the University of Alabama, in his discussion of the hypothesis * advanced, that I need add nothing further. I have carefully examined these observations; and I lend a hearty acknowledgment to the industry and constancy which the observer (Rev. Mr. Jones) has evinced, but I can see nothing which

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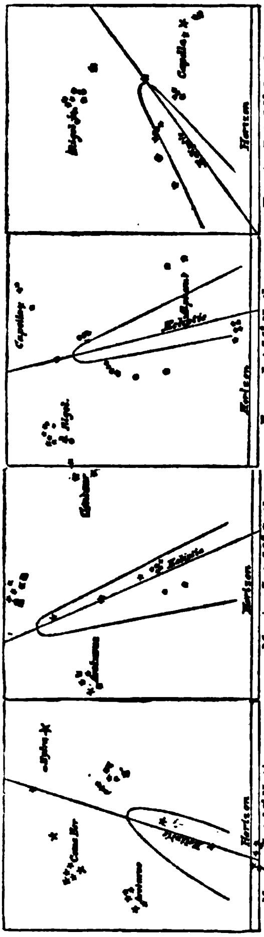
* See Journal of Science and Arts, Second Series, Vol. XXI., March, 1856.

will sanction the adoption of his theory. Although he set out to avoid any bias toward old theories, he seems tenaciously to have clung to the ring theory. I feel much gratified to have so many proofs as his observations offer of the accuracy of those made on the Expedition. I have used a different projection of the phenomenon, and I think Mr. Jones may have been led into a misconception of it by projecting the Zodiacal Light always on the ecliptic, which does not present the phenomenon naturally to the eye, or as seen; if he had projected it on the great circles, passing through the vertical and the horizon, many of the appearances which seem to have puzzled him would have been easily accounted for, and must have led him to a different result.

I will endeavor to describe and illustrate this phenomenon, as derived from our observations, as well as those of others.

The Zodiacal Light, when first visible on a clear horizon, appears as a semicircular arc, with 6° to 10° base, well defined and distinct to the eye, though probably no two persons would trace the same outline of it, unless accustomed to make the observation. As darkness progresses, this semicircular arc elongates upwards in successive altitudes, sometimes to the altitude of 60°. When it has attained its highest point, the diffused light becomes visible, extending on each side over a large area, until lost in the obscurity of night. It is totally different from the extended light of sunset, or the diffused light of twilight. When it becomes visible, it continues so, gradually lessening in height, until the whole is lost beneath the horizon. Its apex is always observed in the ecliptic, to the east or west of the sun, usually at the distance of 60° to 80°, but at times, under favorable circumstances, it is seen to extend as far as 110°.

The evening and morning Zodiacal Light in the same latitude do not correspond in phase or azimuth. This, though among the remarkable facts it exhibits, has never yet been taken into consideration in any of the theories hitherto offered. Its peculiar phase and the constant change of azimuth and inclination, whether the observer remains stationary for any time, or varies his position in latitude, should satisfy every one that it cannot be far removed from the earth. When an observer is stationary, these changes take place very slowly, and it may be said to alter its phase very little during a lunation, or whilst it is visible in high latitudes; but a longer period, more attentive and closer observation, will satisfy every observer that its variations in azimuth correspond to the angular changes of the plane of the ecliptic with the observer's horizon, or with the vertical circle passing through his zenith. Its phases will be seen to be dependent upon the latitude. The vividness of the light, and its extent, are in like manner to be ascribed to the observer's position on the earth. Within the tropics, and when the ecliptic is perpendicular to the horizon, the Zodiacal Light is confined to a slender column, having its diffused light widely extended. Without the tropics, it is always seen very much inclined to the horizon. It then assumes the appearance of a cone, cut more or less obliquely by the horizon. It attains its greatest altitude in the first of these positions, when it not unfrequently may be traced to the zenith. In order to make these remarks fully evident to the reader, the annexed diagrams show the appearance of the morning and evening Zodiacal Light, on the same day, in corresponding north and south latitudes, though not on the same meridian. It will be seen that the cones are more or less inclined, and in opposite directions; this is owing to the observations being made under a different angle with the ecliptic. Corresponding observations on the same day, equally removed from the equator, and on the same meridian, could not be obtained. These diagrams prove most conclusively that it must be the same object, seen from positions to the north and south of the ecliptic, and that the phenomenon must have its locality within the tropics. They mark the characters which have been so frequently noticed, but have failed to claim attention in the formation of theories.



The accompanying plate shows true representations of the morning and evening Zodiacal Light, within the tropics, and in north and south

23° S. Morning

35° N. Evening

48° N. Morning.

latitudes, though under different meridians, with the diffused light, which is at times of greater extent than at others.

The Zodiacal Light is not perceptible till the twilight ceases. This, however, gives a very indefinite idea of the time when it is first seen after sunset, to those situated on different parts of the earth's surface. To an observer within the tropics, there is but a short twilight, and darkness follows soon after the sun sets or dips below the horizon, indicating that there is little reflected light within the tropics, to produce twilight; or, otherwise expressed, the rays of light do not fall sufficiently oblique on the atmosphere to produce twilight. This is more evident when the observer's situation corresponds with the plane of the ecliptic; there night ensues immediately, and the sun's rays appear to be cut off, and not reflected. Although the Zodiacal Light has a higher elevation in the latitudes beyond the tropics, when the sun attains his greatest declination, yet, owing to the long duration of twilight, it is not so visible or distinct as when the twilight is shorter, or about the time of the vernal and autumnal equinox.

The morning Zodiacal Light does not resemble that of the evening either in color, phase, or inclination. Its color is of a cold, silvery hue instead of the warm golden or purplish tint of the evening. It often shows in the morning, within the tropics, as a bright brush of light—like a ray of the aurora, though without its vacillating or transitory pulsations. The brush usually appears about two hours and a half before daylight or sunrise. I have seen it reach the zenith, its width being only one and a half or two degrees; it then spreads rapidly, declines in height, until its altitude is between 50° and 60°, when the diffused light extends along the horizon more than thirty degrees, and over the sky above. An hour and a half before the sun rises, the brush of light sometimes changes its phase rapidly; then the phenomenon becomes extremely beautiful, as if a gossamer veil had been suddenly unfolded from it and stretched across the sky, until it vanishes in thin air, and through which the stars of the smaller magnitudes are seen to twinkle quite distinctly.

Thus it will be seen that there is a great difference in the appearance of the Zodiacal Light, under the effect of a retreating or approaching sun. The tone of the light may easily be accounted for in the cool, grayish tints of the morning atmosphere, while that of the evening preserves the warmth of the closing day.

The Zodiacal Light can never be mistaken for either dawn or twilight; the latter is the effect of reflected light, more diffused, constantly changing, and never well defined. I have never observed any crepuscular rays to accompany the morning or evening Zodiacal Light, nor any reflection of the light on the opposite sky. It stands alone and distinct. Its central line is parallel to the ecliptic, a little to the north or south of it, but more frequently it coincides with it.

I have now given a general description of this phenomenon from careful observations made upon it. In order that the facts derived may be clearly understood, I shall repeat them in a condensed form, before entering into an explanation of the phenomenon, derived from our observations and my investigation. The reader is referred to the diagrams and plates for the appearance which the Zodiacal Light exhibits, when seen from different situations on the earth.

1. The Zodiacal Light occupies a constant relative position in the plane of the ecliptic, preceding or following the sun.
2. Its central line is parallel with or coincides with the ecliptic.
3. Its apex varies in distance, 60° to 110°, from the sun. Its height above the horizon seldom exceeds 60°.
4. Its azimuth changes with the sun and with the observer's position on the earth.
5. Its inclination alters with the position of the observer in latitude, from the vertical down to an acute angle with the horizon.
6. The morning and evening Zodiacal Light are different in phase, color, altitude, and inclination, depending upon the angle subtended between the observer's horizon and the plane of the ecliptic.
7. Its apex lies always *south* of the zenith when the observer is north of the ecliptic, and *north* of the zenith when he is to the south of the ecliptic.
8. When the ecliptic passes through the zenith of the observer, the column of light is vertical to the horizon; it then assumes the appearance of a narrow belt, with a well-defined apex.
9. North or south of the ecliptic, the Zodiacal Light exhibits a broader phase, but less in altitude than when under it.
10. The Zodiacal Light is never seen until the sun has set, and twilight ended, or until all reflected light is cut off; therefore, its visibility in high latitudes depends upon the continuance of twilight.
11. Owing to the length of twilight, the Zodiacal Light is seldom

seen near the limiting parallel. The limiting parallels vary with the sun's declination.

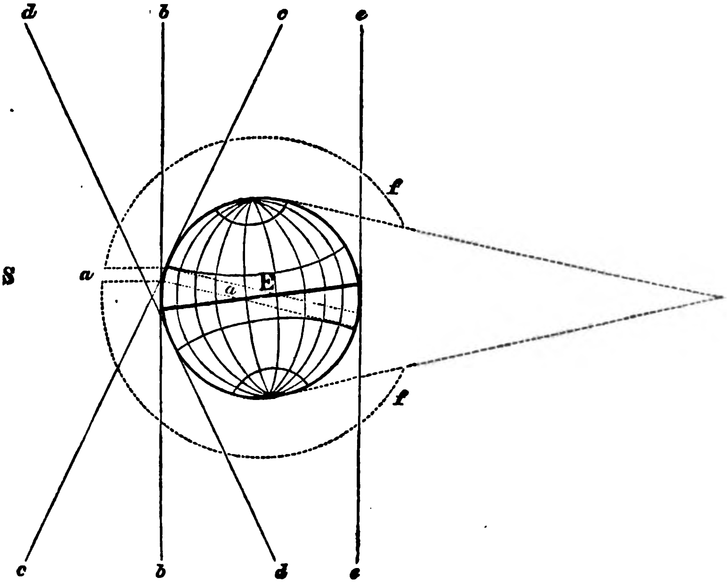
12. The sun's rays falling perpendicularly on the atmosphere within the tropics are not reflected; consequently, after sunset, there is little or no twilight.

These facts go to prove that this phenomenon is the result of the illumination of that portion or section of the earth's atmosphere on which the rays of the sun fall perpendicularly.

It will readily be conceived that rays of light will illuminate a column or portion of atmosphere on which they may fall, when no reflected or diffused light interferes to prevent its being visible. As a well-known illustration, I would cite, that if the direct rays of the sun are admitted through a hole in a shutter into a darkened room, the atmosphere, and the particles floating in it, become as visible and distinct, when all reflected light is cut off, as any well-defined object. It is this which we believe takes place, when the rays of the sun fall perpendicularly on our atmosphere and produce such an effect, which becomes visible on the earth's surface when this column is above the horizon, within the limiting parallels, and after the twilight has ceased.

The whole earth is constantly exposed to and revolves in the sun's rays. A part of these rays only fall perpendicularly on our atmosphere, while all others strike it obliquely, are reflected and refracted by it. As the earth revolves, this column or section of the atmosphere which lies in the ecliptic or earth's orbit becomes illuminated in succession, as marked on the diagram, and thus appears permanently attendant on the sun, either preceding or following him in the ecliptic, and it is this section or cone which is visible when all reflected light is cut off, or darkness reigns, and has been named the Zodiacal Light.

This theory seems to account for all the phenomena which the Zodiacal Light exhibits. In order better to explain our meaning, we refer to the following diagram to illustrate it, wherein *S* represents the sun's rays; *E*, the earth and its atmosphere; *a*, the rays which fall perpendicularly on the atmosphere in the plane of the ecliptic; and *a*, the zone on which they fall as the earth revolves; *b b′*, *c c′*, *d d′*, and *e e′*, the rational horizons of observers at different positions on the earth's surface. It is evident that the illuminated rays will be vertical to an observer when the horizon is *b b′*. To an observer whose horizon is *c c′*, these rays will be a section which is cut obliquely by the horizon, and it will appear to be more inclined the further that observer's distance is removed from the equator, or his latitude increases, until it becomes invisible at the limiting parallel. To an observer whose horizon is at *d d′*, this column of illuminated rays will appear the same, though inclined in the opposite direction; and it will be lost sight of when the limiting parallel on that side is reached. To an observer whose horizon is *e e′*, it is impossible that any part of the column of illuminated rays forming the Zodiacal Light can be seen, but it will readily be perceived that a faint illumination may take place of a small portion of the atmosphere lying beyond the earth's shadow, at *f f′*, so as to produce the "glimmer of eastern light," called by the German writers "Gegenschien," which, it is evident by the diagram, cannot in any way be connected with or caused by the Zodiacal Light.



To observers whose horizons are represented in the diagram, the morning and evening Zodiacal Light must have a different phase and inclination; the observer's position with the ecliptic being changed materially by the rotation of the earth, so as to alter the angle between that circle and the horizon, and this will always be the case, unless he is situated on the part of the earth's surface where the days and nights are equal, or where the angle between the ecliptic and horizon remains the same to the observer after the earth has made a half revolution. When an observer is situated immediately within the perpendicular rays, or on the plane of the ecliptic, and whose horizon is at *b b'*, the phenomenon will appear as a belt, or narrow strip, or column of light, and will apparently be seen to attain a higher altitude. But to those with the horizon at *c c'*, *d d'*, north or south, these illuminated rays will no longer appear as a belt, but take the form of a cone, produced from the rarefied or upper atmosphere being less susceptible of illumination than the lower, and by the effect of a column seen in perspective; and as the angle between the ecliptic and horizon changes, the cone must become more and more oblique. This, our observations have satisfied us, is the case.

We know the azimuthal angle of this belt or cone changes with the declination of the sun or the observer's position, and is so great as to satisfy us that the phenomenon cannot be far removed from our earth, but must be closely connected with the atmosphere.

As this column or cone of illuminated atmosphere has apparently a visible extent in the heavens, along or parallel with the ecliptic, both preceding and following the sun, it has, naturally enough, produced the delusion that it belongs to, or is connected with, the heavens; but it will readily be seen, after a moment's reflection, how this illusion may take place, and will satisfy us that this phenomenon, as seen projected in the heavens, with the starry host twinkling through it, may produce the same effect; and we are only able to overcome this ocular deception by proving, as we have done by attentive and close observation, that it is of this earth, or closely connected with it, from its constantly undergoing great changes in azimuth, phase, inclination, and altitude, as well to a stationary observer as to one who is passing over the earth's surface from north to south, and *vice versa*. If it were distant, or had its origin in the heavens, this would not be the case; these rapid changes could not then take place: therefore, we are compelled to admit that we are deceived, and that its locality must be in the atmosphere surrounding this globe, and be an illuminated section of it which becomes visible to us as soon as twilight ceases or darkness ensues. That it lies between us and the Milky Way is evident, for when bright it nearly eclipses that starry nebulæ. Indeed, the whole phenomena accompanying the Zodiacal Light are fully accounted for by this simple theory, and strictly in accordance with all the observations made upon it, up to the present time.

Now it may be understood how all bodies giving sufficient light to illuminate the atmosphere by perpendicular rays may produce an effect similar to the Zodiacal Light, though in a much less degree. I have seen a corresponding appearance to accompany the moon and the larger planets, after they have set.

The phases of the Zodiacal Light are generally more visible in the absence of the moon, but I have several times observed them before dawn, when they were too bright to be eclipsed by that luminary. As a general rule, the observations are limited to the first and last quarters, those of the morning being visible in the former, and those of the evening in the latter.

It has been remarked above, that it is impossible to see the Zodiacal Light at midnight, the observer's horizon being $e\,e'$, but there are rays of light passing over and under the observer's position, which afford a feeble illumination to the atmosphere beyond the earth's shadow, at $f\,f'$, which may be seen. From this effect, we frequently experience nights less dark; and there are positions in the earth's orbit where this result will be greater and more apparent; and, as remarked before, may have produced those appearances to which the German writers have repeatedly called attention, and led the author of the Japan Expedition to the belief, that they were produced by the morning and evening Zodiacal Lights, visible at the same moment.

III. PHYSICS AND CHEMISTRY.

1. PRELIMINARY RESEARCHES ON THE ALLEGED INFLUENCE OF SOLAR LIGHT ON THE PROCESS OF COMBUSTION. By PROF. JOHN LECONTE, M. D., of Columbia, South Carolina.

A POPULAR opinion has long prevailed in England, and, perhaps, in other countries, that the admission of the light of the sun to an ordinary fire tends to *retard* the process of combustion. In some instances, the practice of placing screens before the fireplace, or of closing the shutters of the apartment, may be traced to the prevalent belief, that the access of sunlight to the burning materials is unfavorable to the continuance of the phenomenon of combustion. Most physical philosophers, very naturally, regard this opinion as a mere *popular prejudice;* probably originating in the well-known apparent dulling or obscuration of flames and of solid bodies in a state of ignition, which takes place when they are exposed to strong light. The flame of a jet of burning hydrogen is scarcely visible in the diffused light of a clear day; that of an ordinary alcohol lamp is barely appreciable to the eye when exposed to the direct sunshine; while a portion of ignited charcoal, which glows in the dark, appears to be extinguished when placed in the sunlight. These familiar phenomena, attributable to well established physico-physiological laws, seem to afford a much more rational explanation of the origin of the popular opinion, than to suppose it to be based upon accurate observations relating to the actual rapidity of burning. About 32 years ago, Dr. Thomas M'Keever published a series of experiments in the "Annals of Philosophy,"* which seemed to show that there is a real foundation for the popular impression, and that solar light *does* actually retard the process of combustion. So far as I am aware, these remarkable experiments have never been repeated. Leopold Gmelin, in his "Hand-Book of Chemistry," † announces Dr. M'Keever's results without comment.

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* Annals of Philosophy, New Series, vol. 10, p. 344. Nov. 1825.

† Leopold Gmelin's Hand-Book of Chemistry (Cavendish Society's Translation), vol. 2, p. 35. London, 1849. A contemporary journal, in noticing these results,

The important bearing which they appear to have on the influence of solar light on chemical processes, as well as on the modern dynamical theory of the mutual convertibility of the so-called imponderables, induced me, during the months of May and June last, to undertake a series of experiments, with the view of testing the validity of Dr. M'Keever's conclusions. The subjoined table will exhibit his results in a convenient form for future use.

Expt.	Green Wax Taper lost in 5 minutes.		Ratio.
1	In dark, temp. 67° F. 9.25 grains.	In sunshine, temp. 78° F. 8.5 grains.	1 : 1.088
	Taper lost, by burning 7 minutes in		
2	Dark, temp. 67° F. 11 grains.	Sunshine, temp. 78° F. 10 grains.	1 : 1.100
	Mould Candle, to consume 1 inch, took, in		
3	Dark, temp. 68° F. $56^m\ 0^s$	Sunshine, temp. 80° F. $59^m\ 0^s$	1 : 1.053
	Taper, to consume 1 inch, took, in		
4	Dark, temp. 67° F. $4^m\ 30^s$	Sunshine, temp. 79° F. $5^m\ 0^s$	1 : 1.111
	Taper, in sunshine, lost, in 10 minutes,		
5	In painted lantern, 16.5 grains.	In uncoated lantern, 15 grains.	1 : 1.100

A *sixth* experiment of a similar character, made in strong *moonlight,* indicated no such diminution in the rate of consumption. The conclusion to which Dr. M'Keever came, was, that solar light *does exercise* a positive retarding influence on the process of combustion.

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remarks : "It has always been considered a vulgar error, that the sun's light extinguishes a fire; but the following experiments by Dr. McKeever put the matter beyond a doubt." (Brewster's Edin. Journal of Science, vol. 5, p. 180.—1826.)

He supposes this effect to be owing to the well-known influence of the solar rays on many chemical processes; in some instances accelerating them, but in others, retarding them. Under this point of view, the *chemical* rays may be supposed to exercise a *deoxidizing* power, which, to some extent, interferes with the rapid oxidation of the combustible matter. In confirmation of this opinion, Dr. M'Keever made an experiment, which appears to indicate, that a taper burns more rapidly in the *red* than in the *violet* extremity of the solar spectrum.

In attempting a repetition of Dr. M'Keever's experiments, I found it impossible to secure that freedom from agitation in the atmosphere, during the exposure of the burning body to the influence of sunshine in the *open air*, which such an investigation demanded. This was his method of conducting the *first four* experiments given in our table. The powerful influence exercised by comparatively slight disturbances in the air, on the rapidity of combustion, renders attention to this circumstance of controlling importance. His method of obviating this difficulty by the use of lanterns (as indicated by experiment 5) is objectionable, from the impossibility of securing precisely identical conditions in relation to the supply of air in the interior. There were, likewise, other considerations which urged me to modify his method of conducting the investigation. It occurred to me, that, as in his experiments the *temperature* of the air which supplied oxygen for combustion in the sunshine was about 12° Fahr. above that in the darkened room, the rarefaction produced by heat might exercise some influence in retarding the rate of burning in the sunlight.

In conducting my experiments, I endeavored to secure *two conditions;* namely,

1. Absolute *calmness* in the atmosphere.
2. Exposure of the flame to the influence of intense solar light, *without heating the surrounding air.*

The *first* condition was secured by performing *all* of the experiments in a large lecture-room, with all the doors and windows closed. To secure the *second* condition, I employed a portion of the apparatus belonging to a large solar microscope, consisting of the reflecting mirror, the condensing lens and tube, together with the mechanical arrangements for adjusting the direction of the light. As the condensing lens was upwards of four inches in diameter, I hoped to *exaggerate* enormously whatever effect the light might exert, by concentrating it on a comparatively small area. Inasmuch as the aperture in the window through which the light was admitted was completely closed by this arrangement, the exterior agitations of the atmosphere were not felt in the room; while the pencil of light, thus thrown on the flame, traversed it, as well as the surrounding air, *without imparting a sensible amount of heat to the latter.*

I used the best *wax-candles* (as they are called in the shops), *four* to the pound, costing about fifteen cents apiece.* By allowing them to burn a sufficient length of time to form a well-defined cup for the melted wax, and carefully turning the wicks, so as to render them self-snuffing, the combustion was found to go on with remarkable uniformity in a calm atmosphere. The rate of burning was determined in the following manner: A portion of candle, three or four inches in length, was secured to the bottom of one of the scale-pans of a tall balance, and ignited; after allowing it to burn for ten or fifteen minutes, so as to secure a steady flame of constant size, it was *nearly* balanced by adding weights to the opposite scale-pan, allowing a slight preponderance to the candle-pan. In a short time the equilibrium was established by the burning of the candle; the precise *time* at which the balance indicated a condition of equilibrium was *accurately noted.* Next, *a given* weight (say sixty or one hundred grains), was withdrawn from the weight-pan, and the *time* of restoring the equilibrium by the loss of weight in the burning candle was, in like manner, recorded. In this manner, the rate of combustion was determined by observing the *time* occupied in consuming a *given weight* of the burning matter. The arrangements described above enabled me to perform such experiments alternately in the darkened room and in the concentrated sunbeam, *without moving* any portion of the apparatus in the room, and under external conditions as nearly identical as could be desired. Many preliminary experiments were made for the purpose of testing the delicacy of the arrangements, which very soon convinced me that no reliable results could be obtained unless the air was calm; and *also*, unless the candle was allowed to burn a *sufficient length of time* to establish *regularity* in the process of combustion. The days selected for the experiments were perfectly *cloudless*. The state of the barometer and thermometer was carefully noted. I now regret that I neglected to record the hygrometric condition of the air. The cone of sunlight was so directed that its *lower* margin illuminated the charred portion of the wick of the candle, while the *upper* boundary of the pencil traversed the flame near its apex. The following table presents the result furnished by *three sets* of experiments performed on as many separate days:—*

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* From the close approximation to *identity* in the rate of consumption, it is probable that these are the same as Dr. Ure's "genuine wax-candles." He found the consumption to be, "upon an average of many experiments, 125 grains per hour." (Dict. of Arts, Manuf. et Mines, 4th Ed. Article, "Illumination, Cost of.") My experiments give respectively 136.7, 125.4, and 124.7 grains per hour. Other kinds of candles burn at a much more rapid rate.

DATE.	BAR. RED'CD TO 32° F.	TEMP. OF AIR FAHR.	TIME OF CONSUMING 60 GRAINS.		AMOUNT CONSUMED IN 10 MINUTES.		DIFF. GRS.
			Dark.	Sunlight.	Dark.	Sunlight.	
May 9.	29.92	67°	26^{m} 24^{s}	26^{m} 15^{s}	22.73 grs.	22.86 grs.	—0.13
June 6.	29.72	75° 5′	28^{m} 39^{s}	28^{m} 45^{s}	20.94 "	20.87 "	+0.07
" 10.	29.62	84°	28^{m} 55^{s}	28^{m} 51^{s}	20.75 "	20.80 "	—0.05

It will be observed, that these experiments indicate *no sensible difference* in the rate of combustion of the candle in the darkened room, and in the same apartment, with a pencil of concentrated sunlight directed on the flame; *provided*, the comparison is restricted to the results

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* The difficulty of keeping the pencil of solar light properly directed on a flame of *variable* altitude, induced me to try a "Burning-Fluid" lamp, having a short cylindrical reservoir, furnished with two wicks. The following results were obtained:—

Date.	Bar.	Temp.	Amount Consumed in 10 Minutes.	
June 3d	29.72	74° F.	Dark = 40.82 grains.	Sun = 40.00 grains.
" 4th	29.78	75° "	" = 40.95 "	" = 39.48 "
" "	" "	" "	" = 38.34 "	
" "	" "	" "	" = 37.52 "	

The progressive *decrease* in the rate of combustion, as indicated by the experiments of the 4th of June, shows that the variations in the *height of the liquid* are sufficient to vitiate the results obtained from burning any *fluid material*.

obtained on *any given day*. In two instances there was a slight *excess* in the rate of burning in the *sunshine;* and in the other, the excess was in favor of the dark; but the differences are probably within the limits of the experimental inaccuracies incident to such delicate investigations.

These *negative results* are the more striking from the fact that, if solar light exercised the decided influence on the process of combustion which Dr. McKeever's experiments seem to indicate, we should expect the effects to be much more marked and conspicuous, when the light was increased in intensity from *eight to tenfold* by the concentration of a lens. The fact that the rays of the sun traversed the *glass lens* before they fell on the flame can scarcely be urged as a possible explanation of the discrepancy; for Dr. M'Keever obtained analogous results when he employed lanterns (vide Exp't No. 5).

The obvious variation in the rapidity of combustion on *different days* (as exhibited in my experiments) illustrates in a most striking manner the decided influence exercised on the process, by comparatively slight alterations in the external conditions. This fact should inspire us with wholesome caution, and check the spirit of rash generalization. Throwing out of consideration the possible fluctuations in the rate of burning, arising from the want of homogeneity in the combustible materials and imperfections in the mechanical arrangements by which they are consumed, there are *three external conditions* which may be supposed to exercise more or less influence on the rapidity of the process. These are, *first*, Barometric Pressure; *second*, Temperature of the Air; and *third*, Amount of Aqueous Vapor present. I propose to consider each of these separately.

1. *Barometric Pressure.*

From *à priori* considerations, we should be led to expect that an increase of barometric pressure, through the consequent condensation of the air, would, *cæteris paribus*, tend to *augment* the rapidity of combustion by furnishing the burning matter with a greater amount of oxygen in a given volume. Unfortunately, direct experiments are wanting to test this in as satisfactory a manner as we should desire. The older experiments in the Boylean vacuum, inasmuch as they relate to the degree of rarefaction at which combustion *ceased*, do not give us information in regard to the *rapidity* of the process at the various stages of exhaustion. The same remark applies to the later experiments of Grotthuss, as well as to the admirable "Researches" of Sir Humphrey Davy on the "Effects of Rarefaction, by partly removing the Pressure of the Atmosphere upon Flame and Explosion."* The experiments of the latter show that rarefaction produces striking alterations in the size and character of the flame, but do not touch the question of the relative *rate* of burning under different pressures; they test the comparative *combustibility* of different bodies, rather than the rapidity of consumption of a *given body* under various degrees of rarefaction. Nevertheless, Davy informs us that he determined from actual experiment that the amount of *heat* developed in a *given time* by combustion is slowly *diminished* by rarefaction, "the diminution of the cooling power of the nitrogen being apparently in a higher ratio than the diminution of the heating powers of the burning bodies." Speaking of the phenomena of combustion in *condensed* air, he says: "I ascertained, however, that both the light and *heat* of the flames of the taper, of sulphur and hydrogen, were increased by acting on them by air condensed four times; but not more than they would have been by an addition of one fifth of oxygen." Again, he says: "But by compression, there can be no doubt, the heat of flames from *pure* supporters and combustible matter may be greatly increased, probably in the ratio of their compression." In the case of air, he does not think the effect would be so great. Inasmuch as the quantity of heat developed in a *given time* by the burning of a *given substance* is known to be a measure of the amount of matter undergoing oxidation, we are justified in the inference, that the foregoing results of Sir H. Davy's experiments show that the *rate* of combustion was retarded by the rarefaction, and accelerated by the condensation of the air.

The most satisfactory results in relation to the influence of *condensed air* on the process of combustion are those incidentally furnished about sixteen years ago, by M. Triger, a French civil engineer, during the operations necessary for working a bed of coal lying under the alluvium bordering the river Loire, near Languin, in the department of *Maine-et-Loire.* In traversing an overlying stratum of quicksand, from fifty-nine to sixty-five and a half feet thick, he found it requisite to devise some means of excluding the semi-fluid quicksand and water, which found their way under every arrangement analogous to ordinary *coffer-dams*, in such quantity as to defy all pumping operations intended to keep them dry. For this purpose, M. Triger employed large sheet-iron cylinders, about 3.39 feet in interior diameter, securely closed at the top, in which — by means of a condensing pump, incessantly worked by a steam-engine — air was condensed to an amount sufficient to counteract the external hydrostatic pressure. The ingenious contrivance fully justified the expectations of the engineer; but the workmen were thus compelled to labor in air condensed under a pressure of about *three* atmospheres. Among other curious results of this state of things noticed by M. Triger were the remarkable effects of condensed air on combustion. Much annoyance was at first experienced from the *rapid combustion* of the candles, which was only obviated by substituting flax for cotton threads in the wicks.[*] Similar phenomena were observed, a few years ago, by the engineers of the Wilmington and Manchester Railway, who employed analogous apparatus for securing the foundations for the piers of the railroad bridge across the Great Pee Dee river, in South Carolina. So far as I have been able to ascertain, the results manifested in this case were identical with those recorded by M. Triger, and afford a most striking confirmation of the influence of condensed air in *accelerating* the process of combustion.

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* Vide Davy's "Researches on Flame," Phil. Trans. for 1817, p. 45, et seq. Also, Works of Sir H. Davy, edited by Dr. John Davy, Vol. VI. p. 51, et seq., London, 1840

On the other hand, facts are not wanting to prove, that combustion is *retarded* at considerable elevations above the ocean, where the air is rarefied by diminished pressure. In a letter recently communicated to the Royal Society of London, from J. Mitchell, Esq., Quartermaster of Artillery at Bangalore, India, "On the Influence of Local Altitude on the Burning of Fuses of Shells," this officer shows, that there was a *progressive retardation* of the rate of combustion of the fuses, at altitudes of 3,000, 6,500, and 7,300 feet, as contrasted with the rapidity of burning at the artillery depôt yard. This difference Mr. Mitchell, very rationally, attributes "to the rarity of the atmospheric air, and of its constituent oxygen, at the higher stations."* The following table, in which I have reduced the barometric heights to the *freezing point*, exhibits the *mean results* of his experiments.

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[*] Vide Comptes Rendus, Tome XIII., p. 884, et seq., Paris, 1841. Also, Annales de Chimie et de Physique, 3d series, Tome III., p. 234, et seq., Paris, 1841. The following are the words of M. Triger: "A la pression de trois atmosphères, cette accélération devient telle que nous avons été obligés de renoncer aux chandelles à mèches de coton pour les remplacer par des chandelles à mèches de fil. Les premières brûlaient avec une telle rapedité, qu'elles duraient à peine un quart d'heure, et elles repandaient en outre une fumée intolérable."

Height in Feet.	Bar. at 32° Fahr.	Temp. Fahr.	Average of Time of Burning 8 inches of Fuse.	No. of Exp'ts.
Depôt	29.610	89°	14.25 Seconds	6
3,000 ft.	26.755	82°	15.78 "	5
6,500 "	23.951	61°.8	17.10 "	3
7,300 "	22.979	54°.2	18.125 "	2

These experiments seem to have been made with great care; all in the presence of artillery officers, who were furnished with the most accurate methods of measuring time. They amply prove the fact, that combustion is retarded at considerable elevations.

Thus a variety of well-established facts concur in fortifying the conclusions to which we are led by *à priori* reasoning; *namely*, that the process of combustion is *retarded* by diminution of the density of the air, while it is *accelerated* by its condensation. It has long been a matter of common observation, that ordinary wood fires burn more freely when the barometer is high; but Mr. Marcus Bull and others maintain,† that this result is not owing to the augmented *density* of the air, but to the greater *dryness* of the atmosphere. The facts brought forward in this paper are strongly opposed to this explanation; for, there are not the slightest grounds for supposing, that there was *less* than the ordinary amount of aqueous vapor present in the condensing cylinders of M. Triger; or *more* than the usual quantity mixed with the air at the elevated stations in India. On the contrary, physical considerations lead us to precisely *opposite* conclusions.

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[*] Philosophical Magazine, 4th Series, vol. 10, p. 48. July, 1855. Fuses burn *without air*; but the *rate* of burning is influenced by atmospheric oxygen.

[†] *Vide* Trans. of Am. Philosophical Society, 2d Series, vol. 3, p. 55, 56. Philadelphia, 1830.

2. *Temperature of the Air.*

In relation to the influence of the *temperature* of the air on the *rate* of combustion, our information is still more meagre. The experiments of Grotthuss and Sir H. Davy on the "Effects of Rarefaction by Heat on Combustion and Explosion," give contradictory results;* but, as they relate exclusively to the influence of temperature on the *ignition* of explosive mixtures of gases, they test its effects on *combustibility*, and are obviously inapplicable to the question under consideration. The well-known effects of the "hot blast," in increasing the temperature of furnaces, cannot be applied as a test of the influence of warm air on the rate of combustion, under ordinary circumstances. *First,* because the air of the "hot blast" is *not* in its natural state of *density;* and, *secondly,* because the augmentation of temperature observed in such cases, probably arises from its greater *availability,* growing out of the fact, that less heat is carried off in the products of combustion; rather than an absolute increase in the *rapidity* of burning.

In the absence of direct experimental evidence, it may be admissible to apply general reasoning based upon well-known physical principles. So far as an increase of temperature influences the *density* of the air, it is sufficiently evident, that its effects must be equivalent to a diminution of barometric pressure; and, consequently, must tend to *retard* the process of combustion. Assuming the temperature of the flame to be *constant,* it is likewise plain, that the *draught* created by it,—depending, as it is known, on the difference of temperature between the flame and that of the surrounding air,—must be *diminished* in a warm atmosphere; and, therefore, also tend to *retard* the rate of combustion, *cæteris paribus,* during hot seasons. But inasmuch as the variations in the velocity of the draught are proportional to the *square roots* of these differences of temperature, it is obvious that its effects must be insignificant under ordinary fluctuations of atmospheric temperature. For example, supposing the temperature of the flame to be 1,500° Fahr., then the fluctuation of the draught between the temperature of 80° and 60° Fahr., would be in the ratio of

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* Phil. Trans. for 1817, p. 53.

$$\sqrt{1500 - 80} : \sqrt{1500 - 60} = \sqrt{1420} : \sqrt{1440}, = 1 : 1.0070.$$

When, however, the comparison is made between bodies burning in summer and in winter, the influence from this cause will be more sensible, and ought not to be entirely overlooked.*

On the contrary, it is *possible* that an augmentation of temperature might tend to *accelerate* the process of combustion, by favoring the liquefaction of the wax, and, perhaps, facilitating the oxidation of the combustible matter. If any such influence is exercised, it is probable, however, that its effect must be inappreciable under ordinary circumstances. Under this view of the subject, the only obvious influence which atmospheric heat exercises on the rapidity of combustion is connected with its effects on the *density* of the air; and that, consequently, an increase of temperature should, *cæteris paribus*, *retard* combustion, and *vice versâ*.

3. *Amount of Aqueous Vapor present.*

Sir Humphrey Davy found that "a very large quantity" of *steam* was required to prevent sulphur from burning; that an explosive mixture of oxygen and hydrogen, when mixed with *five times its volume of steam*, still exploded by the electric spark; and that a mixture of air and carburetted hydrogen gas required "a *third* of steam to prevent its explosion, whereas one fifth of azote produced the effect."† Under any point of view, it is obvious that the presence of aqueous vapor can only tend to *retard* the process of combustion: first, because it diminishes the amount of oxygen in a given volume of air, and secondly, because an admixture of any inactive gas tends to extinguish the burning body, as is abundantly proved by the experiments of Sir H. Davy and others. When vapor is present in large quantities, there can be no doubt of its controlling agency on combustion. This is illustrated by the successful application of the plan proposed by M. Dujardin, of Lille, in 1837, for extinguishing fires occurring in steamships, by permitting the steam from the boilers to escape into the apartment in which the combustion originates.[*] But experiments are still wanting for determining its influence on the rate of burning, when existing in the small quantities in which it is usually associated with the atmosphere.[†] The experimental researches of Mr. David Waldie, in relation to the mixture of various gases with air, led him to the *general law*, that, "of incombustible gases which remain undecomposed, the power of preventing combustion is in the order of their density;" and that "this effect of density in cooling the flame depends on the excessive *diffusion* of the flame in the denser gas."[‡] Under ordinary circumstances, the *density* of the aqueous vapor existing in the air is comparatively *small*, so that, according to Mr. Waldie's law, its influence on combustion ought not to be very striking. It is very desirable that this point should be submitted to a more rigorous experimental investigation.

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* I endeavored to test the influence of *temperature* on the rate of combustion, by placing the burning candle over a *large heated plate;* but, as might have been expected, the *unsteadiness of the flame* rendered the experiment unsatisfactory.

† Phil. Trans. for 1817, p. 65.

Having discussed the probable influence of the *three external conditions* on the rate of combustion, we are, in a measure, prepared to investigate their *adequacy* to explain the variations in the *rapidity of burning*, as indicated by the experiments which I have brought forward. In *none* of them have we the observations necessary for ascertaining the *hygrometric condition* of the atmosphere; this must, therefore, be thrown out of consideration. In Dr. M'Keever's experiments the barometric indications are *not* given; neither is it known how many of them were performed on any *one day*. In my experiments, as well as in those of Mr. Mitchell, we are furnished with the data requisite for estimating the *combined* influence of *pressure and temperature*. Assuming, with Sir H. Davy, that the *rapidity* of combustion is in the *direct ratio of the density of the air*, we may submit these two effects to a *quantitative* estimation, by using Mariotte's law and Regnault's co-efficient of expansion for air.

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[*] Comptes Rendus, Tome V., p. 28, Paris, 1837; also, Tome XXXV., p. 368 et 706. Paris, 1852.

[†] The curious results obtained by Mr. J. F. Dana, and subsequently by Mr. Samuel Morey, in relation to increasing the *brightness* of the flames of highly carbonaceous combustibles, by throwing a jet of steam into them, are obviously inapplicable to candles (vide Silliman's Journal, 1st series, Vol. I., p. 401; Vol. II., p. 118, 122; and Vol. VII., p. 141).

[‡] Vide Philosophical Magazine, 3d series, Vol. XIII., p. 86 et seq., August, 1838.

1. Presuming that *each set* of experiments made by Dr. M'Keever, alternately in the dark and in the sunshine, was performed on the *same day*, and therefore under *identical* barometric conditions, we may form some estimate of the adequacy of *temperature* to account for the difference in the rate of burning observed by him. The subjoined table, which I have constructed from the data previously given, will place this in a clear light.

Experim't.	Temp. Dark.	Temp. Sun.	Consumed in ten minutes		Ratio of Amount in Sun & Dark.	Ratio of Den. Air in Sun & Dark.
			In Dark.	In Sun.		
No. 1	67° F.	78° F.	18.50 grs.	17.00 grs.	1 : 1.088	1 : 1.021
" 2	67° "	78° "	15.72 "	14.29 "	1 : 1.100	1 : 1.021
" 3	68° "	80° "			1 : 1.053	1 : 1.023
" 4	67° "	79° "			1 : 1.111	1 : 1.023

The remarkable discrepancies indicated by the numbers in the first column of *ratios* afford a striking illustration of the existence of some disturbing cause, tending to vitiate the accuracy of these experiments. But a glance at the numbers contained in the two columns of *ratios* is sufficient to show, that *temperature alone* is entirely *inadequate* to account for the diminished rate of combustion in the sunshine. A remarkable difference is observed in the *rate* of consumption in experiments 1 and 2. No. 1 was made with a "green wax taper," and No. 2 with a "taper;" but, as from the context, the *second* experiment appears to be a repetition of the *first*, the presumption is, that the same kind of taper was used in both cases. The rate of burning in experiments Nos. 3 and 4 was determined by the *time* required to consume a *given length;* and as one of them was made with a mould candle, and the other with a taper, no comparison can be extended to them, so far as the rates of consumption in these two cases are concerned. The irregularities exhibited in these results most probably arose from the agitations of the atmosphere, which were incident to the method of exposing the burning body to the sunshine in the *open air.* As the *excess* of consumption in the dark varied from 5 to 11 per cent., whereas the excess in the density of the air was only 2.3 per cent., it is evident that some *other* cause than *temperature* must be evoked to explain the difference.

2. In my experiments, the conditions were such as to eliminate the effects of *temperature* on the results obtained in the dark and in the sunshine on any given day; and it has been shown, that for *each pair* of experiments thus conducted, the variations in rate of combustion *do not* exceed the probable limits of experimental error. In this case, therefore, the question to be determined is, whether the differences in the *rapidity of burning* observed on *different days* can be explained by the variations of the barometer and thermometer? For this purpose, I shall take the average of each *pair* of experiments, as a nearer approximation to the correct rate of burning on each of the three days. The following table, in which the *relative densities* of the air have been calculated by *combining* the effects of barometric and thermometric oscillations, will serve to illustrate this point:—*

Ratio of Consumption.	Ratio of Density of Air.	Difference.
In Exp'ts 1 et 2 = 1 : 1.0904	1 : 1.0230	+ 6.74 per cent.
" " 2 et 3 = 1 : 1.0063	1 : 1.0193	— 1.30 " "
" " 1 et 3 = 1 : 1.0967	1 : 1.0428	+ 5.39 " "

It will be seen, that the rate of combustion increases in a decidedly *higher ratio* than the *density* of the air. If, therefore, we assume that the rapidity of burning is, *cæteris paribus*, in the direct ratio of the density of the air, it follows that some *other agency* must have coöperated in these cases.

3. The results of Mr. Mitchell's experiments, at different altitudes, may, in like manner, be subjected to a similar numerical test. The subjoined table is constructed from the data contained in that which is given on a previous page.

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* In making these calculations I used the following formula, based upon the two well-known physical laws, that the *density* of any permanent gas varies *directly* as the compressing force, and *inversely* as the volume:—

$$d : d' :: \frac{b}{1 + 0.002036\,(t - 32)} : \frac{b'}{1 + 0.002036\,(t' - 32)};$$

in which d and d' represent the *densities*; b and b' the *barometric heights* reduced to the freezing point; and t and t' the *temperatures* on Fahrenheit's scale.

Ratio of Rate of Burning.	Ratio of Density of Air.	Difference.
In Exp'ts 1 et 2 = 1 : 1.1074	1 : 1.0926	+ 1.48 per cent.
" " 2 et 3 = 1 : 1.0836	1 : 1.0755	+ 0.81 " "
" " 3 et 4 = 1 : 1.0599	1 : 1.0270	+ 3.29 " "
" " 1 et 3 = 1 : 1.2000	1 : 1.1751	+ 2.49 " "
" " 2 et 4 = 1 : 1.1486	1 : 1.1045	+ 4.41 " "
" " 1 et 4 = 1 : 1.2719	1 : 1.2068	+ 6.51 " "

This comparison places in a still stronger light the fact, that the augmentation in the rate of burning increases in a somewhat *higher ratio than the density of the air;* while, at the same time, it clearly demonstrates the *controlling influence of atmospheric density* on the phenomenon of combustion. The *extreme rates* of burning are as the numbers 100 to 127, while the corresponding *densities* of the air are as 100 to 121, nearly; in the other cases, the approximation to *identity* in the ratios is still closer. Would the variations in the hygrometric state of the atmosphere, — which we have left out of consideration, — explain this discrepancy? In the absence of the experiments necessary for testing this question, it would be premature to hazard any conjecture. I may remark, however, that in the case of Mr. Mitchell's experiments, the correction for the effects of aqueous vapor would, probably, *in one point of view*, operate in the *wrong direction*, and thus tend to increase the discrepancy in the ratios. For, as the temperature was decidedly higher at the lower stations, it is more than probable that the *tension of vapor* was *greater* there than at the upper ones; and, consequently, that its influence in retarding combustion should be *relatively greater* at the points nearer the sea-level. This, of course, would tend to *equalize* the rates of burning at lower and higher altitudes, when no correction is made for this cause. On the contrary, it is obvious that the influence of vapor having a *given tension*, in altering the relative amount of air in a *given volume*, must be *greater* when the barometer is low. From this cause, the aqueous vapor at the upper stations might have had a *greater* effect in retarding combustion, and thus tended to *exaggerate* the difference in the rates of burning.

The comparatively large rate of consumption indicated by my *first* experiment of the 9th of May, (being more than 9 per cent. above the others,) was most probably attributable to a combination of causes. All of the *three external conditions* concurred in accelerating the process. The barometer was *high*, the temperature *low*, and the atmosphere *excessively dry*. The last-mentioned condition was accidentally forced upon my attention, from the fact, that on that day I *failed* in an experiment for determining the dew-point by means of Daniell's hygrometer.*

From the foregoing discussion it is evident, that the subject demands a thorough experimental investigation, with a minute attention to all of the external conditions which may influence the results. This I propose to undertake during the next twelve months. In the mean time, it is hoped, that these preliminary researches may prepare the way for a clearer appreciation of the difficulties which are to be encountered.

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* Collaterally related to this subject are the effects of condensed and rarefied air and of temperature, on the *process of respiration and the elimination of carbonic acid*, in men and other warm-blooded animals. M. Legallois found, that when warm-blooded animals breathed air under *pressure*, reduced to 11.811 inches, the amount of oxygen consumed was *diminished*. (Ann. de Chimie et de Phys., Tome 4, p. 113. 1817.) M. Theodore Junod's experiments show, that *condensed air* produced deep inspirations and an agreeable glow throughout the system; while *rarefied air* had an opposite effect. (Archives Générales de Médecine, 2d Series, Tome 9, p. 157. Paris, 1835. Also Magendie's Report on the same Memoir, Comptes Rendus, Tome 1, p. 60. Paris, 1835.) The observations of M. Triger, already referred to, indicate analogous effects on those who labored in the condensed air. They could do double work without fatigue; and even old asthmatics seemed to recover their vigor. (Comptes Rendus, Tome 13, p. 884 et seq. Paris, 1841.) M. Vierordt tested the effects of barometric pressure between 29.309 and 30.197 inches. The *average* rise of 0.5036 of an inch,

Increased the air expired, 35.746 cub. inches per minute.
“ “ No. of respirations 0.74 “ “
“ “ “ pulse 1.30 “ “

Dr. Hutchinson found, that in a mine 1,488 feet deep, where the pressure was 1.54 inches more than at the sea-level, the respiration was *increased* 2.4 per minute, and the pulse 1.3 per minute. (Cyc. of Anat. et Physiol. Art. Respiration, vol. 4, p. 348, 349. London, 1852.)

Analogous effects are produced by *temperature*. In the famous experiments of Séguin and Lavoisier, at 82° Fahr., the former (fasting and at rest) consumed 1,210 French cubic inches of oxygen per hour; whereas, at 57°, he consumed 1,344 cubic inches per hour. (Mémoires de l'Acad. Royale, for 1789.) Dr. Crawford found, that a Guinea pig, at 55°.5 F., abstracted *twice* as much oxygen from the air as at 104° F. (Expts. et Obs. on Animal Heat, 2d ed. p. 311–315. London, 1788.) Dr. W. F. Edwards found that birds consume more oxygen in winter

Perhaps, however, in the present stage, we may be warranted in deducing *two* conclusions: FIRST, That *solar light does not* seem to exercise any sensible influence on the process of combustion; and, SECONDLY, that variations in the *density of the air do exert* a striking effect in retarding or accelerating the rapidity of the process; the rate of burning augmenting with every increment of density, and *vice versa;* but the exact ratio between them remains to be determined.

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than in summer. (De l'Influence des Agens Physiques, sur la Vie, chap. 6, p. 195. Paris, 1824.) The best experiments are those of M. Vierordt. (*Op. cit. supra.*) He obtained the following results between 37°.4 and 75°.2 F.: —

	Av. Temp. 47°.24	Av. Temp. 66.92	Diff.
Pulse per minute	72.93	71.29	1.64
Respiration “ “	12.16	11.57	0.59
Vol. air expired “ “ in cubic inches	407.00	367.00	40.00
“ CO_2 “ “ “ “ “	18.25	15.72	2.53

M. Felix Letellier's experiments on warm-blooded animals confirm these results. He found the amount of *carbonic acid* evolved *per hour*, at different temperatures, to be as follows: —

	86° to 104° F.	59° to 68° F.	32° F.
Canary	0.129 grammes	0.250 grammes	0.325 grammes
Pigeon	0.366 “	0.684 “	0.974 “
Two Mice	0.268 “	0.498 “	0.531 “
Guinea Pig	1.453 “	2.080 “	3.006 “

(*Vide* Comptes Rendus, Tome 20, p. 795. Paris, 1845. Also Ann. de Chimie et de Phys. 3d Series, Tome 13, p. 478. Paris, 1845.) Doubtless *physiological reactions* exercise a powerful influence over the results of such experiments; nevertheless, as respiration is *essentially* a process of combustion, they have a *general bearing* on the question under consideration.

2. ON THE VIBRATIONS OF THE FALL OVER THE DAM AT HOLYOKE, MASS. By PROF. E. S. SNELL, of Amherst, Mass.

THE vibrations of dams and waterfalls must have attracted attention long ago; but I find no mention of such facts in scientific records, except in an article by Professor Loomis, (Sill. Jour. Vol. XLV. 1843.) In that paper, seven instances of vibrating dams are cited, in all of which the oscillation is attributed to the friction of the water on the edge of the dam, operating like a violin bow across the edge of a tumbler. The considerations which Professor Loomis adduces in proof are, I think, sufficient to establish the correctness of his conclusion in the cases he has described. But I am satisfied that there is another class of vibrating waterfalls, in which the pulsations are owing to a wholly different cause.

The only example of a vibrating fall which I have had opportunity to observe personally is that of the Connecticut over the dam at Holyoke. The whole river, except the comparatively small part taken off in canals, falls in an unbroken sheet 1,017 feet long, between piers, and 30 feet high, and varying in depth on the top of the dam from *six inches* in low-water, to *ten or twelve feet* in high-water. The dam is necessarily built in the strongest manner, and firmly secured to the rocky bed, which extends entirely across the channel. In looking down from the pier at either end, the edge of the entire dam is plainly seen through the smooth bending mass of water, and appears perfectly straight, and without a sign of tremor. It would be difficult to convince the spectator that the strong pulsation which he sees and hears in the fall, and, in some places, feels in the air, can be caused by invisible vibrations in the dam. If the visitor descends to the opening in the pier, and stands at the end of the vacancy behind the sheet, he perceives the air rushing in and out alternately with a strong puffing motion.

I consider the column of air between the fall and the dam to be the original vibrating body, set in motion by the descending sheet. The water, in all cascades, carries with it into the basin below a portion of the adjacent air, both in front and behind, which appears afterwards in foam on the boiling surface. This action of the water produces a rarefaction behind the sheet, which is a sufficient exciting cause of the vibrations. In high cascades, like those of Niagara, there is produced by the same cause a *current* of air, setting *inward* at the top, and *outward* at the bottom; so that a continual circulation is maintained at each extremity of the fall. But in a tube of air more than a *thousand* feet long, and scarcely *twenty* feet in diameter, there is not room for free *circulation*, and a *pulsation* takes place instead.

I visited the fall on the 25th of July last, and again on the 29th, and repeatedly counted the vibrations, both by the eye and the ear. The temperature at the time of my first visit was about 80° F.; and I made the number of vibrations 136, 137, or 138 per minute, and the mean rate 2.28 vibrations per second. During the second visit, the temperature fell from near 80° to about 70°, as I judged; and the number of vibrations diminished from 137 to 134; i. e. from the rate of 2.28 to 2.23 per second. As there is an opening of eight feet by three feet through the pier at each end, and as the sheet is diverted from the pier some four or five feet by means of an apron at the top of the dam, I regard the space behind the fall as an organ pipe open at both ends, 1,008 feet long, or nine feet less than the distance between the piers (= 1017 ft.) Applying to it the formula $N = n. \frac{V}{L}$, we have $2.28 = n \times \frac{1145}{1008}$, for the temperature of 80°; or $2.23 = n \frac{1133}{1008}$ for 70°; or $n = 2$ very nearly. Hence it appears that the air, in an open tube of this length, if two nodes were formed within it, would vibrate at the observed rate, — about 2¼ times per second.

My attention was of course directed to the inquiry, whether there was any indication of nodes in the flexure of the sheet. But here I found an irregularity and a changeableness which were perplexing. At my first observation, there seemed to be three segments in motion; that next the left bank extending nearly half across the river, and the other two dividing the remainder of the breadth about equally. The vibrations of each two successive segments were alternate with each other. But this arrangement was sometimes confused, and it became difficult to trace any divisions. At the time of my next visit, the segments were, at first, three in number, as before, but now of equal length; and the points of division quite plainly marked by their small motion, and by the small height to which the spray was tossed at each outward swing of the fall. But before I left, (about two hours after,) there was as distinctly seen a division into *two* equal segments, with the greatest motion in the centre of each. This last was the mode of vibration which I had expected to see, as indicative of two nodes; for I supposed the water would suffer the greatest transverse disturbance where the inclosed air changed its density most; i. e. at the nodes; and that it would vibrate least where the density of the air was constant; in other words, at the middle of the ventral segment.

In times of high-water, the oscillations of the fall become invisible; probably because the mass is too great to be perceptibly swayed by the difference of internal and external pressures. At the times of my observations, the depth of water above the dam was near *two* feet; and the foot of the sheet seemed to vibrate through *eight* or *ten* inches.

I am strengthened in my belief of the correctness of the foregoing explanation, by a conversation which I held on the subject with Professor Schaeffer, a gentleman now connected with the Patent Office in Washington. He remarked, that he had observed, in many instances, the production of a musical tone by the small cascades in the rapids of rivers; and that the tone was lower as the cascade was broader; so that he was accustomed to consider the cavities behind such falls as organ pipes, yielding various musical sounds, according to their length.

The vibrations of Holyoke Fall are generally noticeable in Holyoke and the village of South Hadley Falls, and are occasionally perceived at the distance of several miles. At the distance of half a mile, I counted the vibrations of a window sash, and found the number 137, the same as I had, an hour previous, counted in the fall itself. I have noticed the movement of doors occasioned by the fall, in a dwelling three miles distant; and am told that the effect is sometimes observed in Springfield, at the distance of eight or ten miles.

Some days after the foregoing was written, I had the opportunity of observing the vibrations for a few minutes, and found that a striking change had occurred. The water was lower than before, and the number of vibrations was nearly doubled, there being from 256 to 260 per minute, or 4.3 per second. This rate corresponds tolerably well with the supposition of *four* nodes. But, in the short time of my stay, I could not determine by the eye the fact of four vibrating segments in the sheet, though I could plainly perceive that certain parts of it had much motion, while others were nearly at rest.

If I may rely upon my recollection of what I noticed some years ago, I saw the same fall vibrating 69 or 70 times per minute; though, as I made no record at the time, it is possible that I counted the number produced in a *half* minute.

It seems quite probable that there are in the Holyoke Fall, at least three modes of vibration, corresponding to three successive octave notes, though the vibrations are too slow to produce on the ear the effect of a musical sound. The depth of water is doubtless the principal circumstance, but perhaps not the only one, which determines the mode of division in the air column. At the temperature of 80°, a column of air in an open tube, with *one* node, should vibrate 68 times in a minute; I think I once counted 69 or 70. With *two* nodes, the calculated number is 186; I made by observation, 137, as a mean result. With *four* nodes, at 70°, calculation gives 269 vibrations, observation, 258.

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3. ON THE ELECTRICAL HYPOTHESES OF THE AURORA BOREALIS.

By PROF. DENISON OLMSTED, LL. D., of New Haven.

IN a paper which the Smithsonian Institution did me the honor to publish in a late volume of their "Contributions," *On the Recent Secular Period of the Aurora Borealis*, I attempted to present a synopsis of the facts, as exhibited in the remarkable series of auroras, which commenced in 1827, and lasted for more than twenty years. I also, from a review of those facts, and from an extensive comparison of them with those witnessed in other great exhibitions of the same phenomenon, in other countries and in past ages, endeavored to state the *laws* of the Aurora Borealis in a more systematic and definite form than had been done by any other observer, within my knowledge. Finally, with these data before me, I proceeded to discuss the question of the *Origin and Cause of the Aurora Borealis.* Contrary to the opinion which ascribes it to terrestial agents, as electricity or magnetism, I argued that the origin of the Aurora Borealis is *cosmical*, the matter of which it is composed being derived from the planetary spaces.

I inferred the cosmical origin of the material of which the Aurora is composed from the following arguments: First, from the *extent* of the exhibitions, sometimes spreading from east to west for many thousand miles, and reaching to a height of a hundred miles or more above the earth, quite above the region of atmospheric precipitations; secondly, from the fact that in places differing many degrees in longitude, the different stages of the Aurora, (such as the beginning, maximum, and end,) *occur at the same hour of the night*, indicating that a place on the earth, in its diurnal revolution, comes successively under the nearest point of the auroral body situated in space; thirdly, from the *velocity* of the motions, being too small for light itself, and too great to result from any terrestrial force, as magnetic or electric attractions, occasioning a translation of the matter of the Aurora; and, fourthly, from the *periodicity* of the Aurora, especially its *secular* periodicity, appearing, as it does, at long but nearly definite intervals in a grand series of exhibitions, which increase to a maximum, and then diminish in number and intensity, until the phenomenon, in its grander forms, vanishes from our nocturnal sky,— a fact which appears to me to remove it from the pale of terrestrial, and to bring it clearly within the domain of astronomical, causes, implying a nebulous body in the planetary spaces from which the material of the Aurora is derived, having a revolution around the sun, and a period in a nearly simple ratio to the earth's period.

At a meeting of the American Association at New Haven, in 1850, I submitted an outline of the paper since published in the Smithsonian Contributions, and ventured then to declare my belief, founded on the history of the phenomenon, that the brilliant exhibitions of the Aurora Borealis, which for more than twenty years had very often presented astonishing displays in our nocturnal heavens, were nearly over, and would soon cease, and not appear again in the same intensity and frequency until after an interval of forty years or more; the interval from the *maximum* of one secular period to that of another being estimated, from historical records, to be from 60 to 65 years. I would now respectfully ask the members of the Association to remark, that for five or six years past, the number and splendor of these exhibitions have in fact greatly diminished, compared with the period marked off in my paper as the "Secular Period;" and I would solicit their further attention to the subject, in order to see how far the expectation I ventured to intimate in 1850 shall be fulfilled.*

In my paper published by the Smithsonian Institution, while full space was liberally allowed me to express my own opinions as to the origin and cause of the Aurora Borealis, it was deemed inconsistent with the plan of the Smithsonian "Contributions," to admit of the discussions of the opinions of others. Some remarks, therefore, which I had made on hypotheses urged by different writers, in explanation of the Aurora, were not published with the article referred to. These, so far as relates to the electrical hypotheses, I now beg leave to submit to the present meeting.

After the discovery of the identity between electricity and lightning, and the consequent connection of electricity with thunderstorms, it became the practice of the interpreters of nature to ascribe every thing mysterious and not otherwise accounted for, to this wonderful agent.† No student of nature can indeed doubt, that electricity holds a most important place among the ultimate causes of physical phenomena; we only complain of the practice of referring effects to this cause simply on the ground of its known activity, without either proving its presence, or legitimately deriving those effects from its known properties. One evil consequence of such a practice has been, to give a qui etus to any further investigation of the subject, supposing that when it was determined to be an "electrical phenomenon," there was no need of any further inquiry into the cause. Such has been particularly the case in respect to explanations of the Aurora Borealis. To begin with Dr. Priestley: "That the Aurora Borealis is an electrical phenomenon (says the Doctor in his elaborate History of Electricity), was, I believe, never disputed from the time that lightning was proved to be one."* The author urges scarcely an argument in favor of this hypothesis, but deems it a sufficient explanation of the Aurora to call it an electrical phenomenon. Other writers, however, have arrayed their arguments in favor of this hypothesis in form. Thus Morgan, in his able Lectures on Electricity, published in England in 1794, urges the following reasons for the opinion, that the Aurora Borealis arises from great quantities of electricity discharged through the upper regions of the atmosphere. "If," says he, "thunder be caused by the electric fluid, there is a certain height in the atmosphere in which the cause of thunder must necessarily assume the appearance of Northern Lights. In air rarefied to a certain degree, the passage of the electric fluid is attended with all the undulating coruscations of the Aurora Borealis. Indeed, there is not a single circumstance in the experiment of passing the spark through an exhausted tube, which does not bear a resemblance to something observed in the Northern Lights. There is the same peculiarity in their motion, the same variety in their color, and the same quick alternation of flashes in both; the streams are alike vivid and pointed; and if the exhaustion be properly managed, some parts will appear with that reddish tinge which is often observed in the air by the vulgar with fear and consternation."†

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* Those who are not old enough distinctly to remember the great auroral exhibitions of 1837 and the years immediately following, will be liable to class among "brilliant Auroras," displays of a kind greatly inferior to those in which such sights as the following were not unusual. First, before the end of twilight, the northern sky presented a bright illumination, as though the sun were rising in that quarter. Soon there appeared, stretching along the northern horizon, and rising a few degrees above it, a bank of luminous vapor, often more or less obscured by a smoky hue, the whole alive with flickering motions. In due time, but often from 10 to 11 o'clock, a forest of silvery spindles (*streamers*) would shoot up from the bank of auroral vapor, and crimson columns begin to form simultaneously in the north-east and north-west, which would rush upward, in company with innumerable streamers, to a point a little south-east of the zenith, — the pole of the dipping-needle, — around which they would arrange themselves in a magnificent *corona*. Frequently, at a later hour, *auroral waves*, of surprising appearance and incredible velocity, would roll up towards the zenith, which frequently continued until the dawn of day.

It is only such exhibitions as these that are entitled to be denominated "Grand Exhibitions of the Aurora Borealis."

† See Beccaria, Priestley, Morgan, Encyclopædia Britannica, etc.

Upon this argument I remark, that this writer, like all other writers who have insisted on the resemblance between the appearance of the Aurora Borealis and the electric spark passing through rarefied air, has greatly overrated the degree of resemblance. That luminous appearances in one case should resemble luminous appearances in another case is nothing remarkable. A fire, a lamp, the sun, the stars, have all some points of similarity. Now, after multiplied comparisons of the appearances of the Aurora with those of the electric spark in rarefied air, as exhibited in what is called the auroral tube, I am compelled to say that I think the likeness is very faint, both in regard to the shades of color and to the peculiar motions. There is nothing in the auroral tube experiment which resembles the auroral streamers or arches or waves or corona. Between the *flashes* of the aurora and those of the spark in the tube, there is occasionally some resemblance. But light derived from very different sources often exhibits many points of similarity, and it is unsafe to predicate upon such incidental resemblances an identity of origin. Reasonings of this nature were often exemplified by the earlier writers on Natural Philosophy. Thus Wallis, the preceptor of Newton, accounts for thunderstorms by ascribing them to the accidental meeting in the air of the elements of gunpowder; an explanation which was countenanced by the striking resemblance of lightning and thunder to the flash and explosion of gunpowder.

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* History of Electricity, p. 376.

† Morgan's Lect's on Elec. II., 237. Singer's El'm. of Elec., 251.

The same author proceeds: "As the rarefaction of the air increases, so does the striking distance of the charge that passes through it. If two clouds, therefore, the one positive and the other negative, should have no other circuit, is it not highly probable that they will discharge themselves through the higher regions of the atmosphere?" The nature of the reasoning appears to be this: Lightning is known to be owing to the discharge of electricity from one cloud to another differently electrified. Now, from experiments with the electric machine, in passing electricity through a tube containing rarefied air, we know that in proportion as the air is more rarefied, the further the electric spark will pass, and the more feeble will be its light, and consequently more like that of the Aurora Borealis. May we not then suppose that this phenomenon arises from the flowing of electricity from one distant cloud to another, through the upper regions of the atmosphere, where the air is much rarefied? But let us remark upon what feeble grounds this analogy is predicated. Thunder clouds are not usually more than a mile high; the auroral arches, sometimes at least, are seventy or one hundred miles and more. The passage of lightning from one cloud to another is seldom more than a mile or two in length; the same auroral exhibition, as that of November, 1848, has been known to extend from Western Asia over Europe and America to California, covering a large part of the northern hemisphere.

The *crackling or hissing noise* with which the Aurora Borealis is said to be sometimes attended is alleged in proof of its electric origin. The existence of any such sound proceeding from an auroral exhibition is doubtful; but its existence being granted, the supposed sound is very unlike that produced by electrical explosions; and, were the resemblance more striking than it is, the presence of electricity would not necessarily be implied, inasmuch as the sound in question might arise from various other causes, such, for example, as the rapid passage of auroral vapor through the atmosphere.

The foregoing considerations lead me to think that any reasoning which ascribes the Aurora Borealis to electricity, founded on a supposed resemblance between the sensible appearances of the Aurora and known appearances caused by electricity, is inconclusive and generally fallacious; yet this was the mode of argument generally adopted by those who first devised the hypothesis, including most writers on the subject of the age of Priestley. But later defenders of the electrical hypothesis have relied little on these arguments, nor have they agreed among themselves in any thing except in the vague idea that the Aurora Borealis is, *in some way or other*, caused by electricity, while they have differed widely among themselves in regard to the *modus operandi* of this agent. Thus nothing can be more unlike the explanation already recited as that adopted by Beccaria, Priestley, Morgan, and their followers, than the explanations subsequently put forth by Biot, Hare, and De La Rive, men whose philosophical reputation entitles their opinions to the greatest respect, although we may venture to examine them with the freedom allowable in an inquiry after truth.

In the year 1817, M. Biot witnessed at the Shetland Islands, where he chanced to be residing, a great auroral exhibition, of which he has given a minute description in his Précis Élémentaire De Physique, accompanied by various speculations concerning the laws and the origin of the phenomenon. He points out the true mode of determining whether any appearance in the sky is within the atmosphere or beyond it, by observing whether or not it partakes of the diurnal revolution; whether it accompanies the stars from east to west, or is left behind by them; and he comes to the conclusion that the auroral exhibition takes place within the atmosphere. But the test suggested by Biot does not reach the question, which respects the *origin* of the Aurora; for if it is proved that the *phenomena* are exhibited in the atmosphere, still the question returns, was the material of the Aurora, (or what we have ventured to denominate *auroral vapor,*) derived from the earth or the atmosphere itself, or did it come into the atmosphere from the regions of space?

The author proceeds to account for the formation of arches and coronas, out of parallel columns, by the principles of perspective, in the same way that Dalton, and even Roger Coates,* at a much earlier period, had done before him. The columns, he thinks, consist of finely divided metallic matter; and being magnetic, they spontaneously arrange themselves in a direction parallel to the magnetic meridian, and of course parallel to one another, as though they were real magnetic needles. Now such columns would be conductors of electricity; and since the electricity of the lower portions of the atmosphere is (according to him) usually vitreous, as is ascertained by the electrical kite, and the upper portions resinous, as is found by æronauts, the two extremities of these vertical columns will commonly be in opposite states, and a continual current of electricity will flow through them, differing in quantity with the varying intensity, and the condition of the medium; but when the opposite states of the upper and lower extremities of the columns are very intense, the electricity, in forcing its way through the imperfectly conducting medium, gives flashes, and the other luminous appearances of the Aurora.

Although some of the appearances of the Aurora Borealis are well explained on the hypothesis of Biot, yet there are others which it hardly reaches, and others with which it appears to me to be inconsistent. There is a high degree of improbability, that such an amount of metallic vapor, derived from any *terrestrial* source, should be suddenly diffused through the atmosphere, as to cover no inconsiderable portion of the whole earth, and extend to the height of one hundred miles or more. Moreover, the motions observed in the auroral lights, although exceedingly rapid, are still progressive, and not instantaneous, as would be the case with electric flashes. Nor does the hypothesis furnish any reason for the periodicity of the Aurora, which is one of its most important characteristics.

Dr. Hare takes quite a different view of the *modus operandi* of electricity in the production of the Aurora Borealis. He considers the relation between the earth and the upper regions of the atmosphere,

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* See Phil. Trans. *Abridged,* Vol. VI.

(which severally constitute two good conductors,) as analogous to the relation between the inside and the outside coating of a Leyden jar, separated from each other by the intervening glass. "Thus," (says he,) "the atmosphere is situated between two oceans of electricity, of which the tension may often be very different. Between these electric oceans, the clouds floating in the non-conducting air must act as movable insulated conductors; and from the excitement consequent upon induction, from chemical changes, or from their proximity to the celestial electric ocean, must be liable to be electrified differently from each other, and from the terrestrial electric ocean. The Aurora Borealis (he adds) may arise from discharges from one ocean to the other of electricity, which, not being concentrated by its attraction for intervening clouds, within air sufficiently dense to act s an electric, assumes the diffuse form which characterizes that phenomenon." *

If the Aurora Borealis were formed in this way, ought it not to prevail most where the supposed electrical oceans exist in their greatest intensity, namely, in the equatorial regions; whereas there they are scarcely known, while they are at a maximum in the polar regions, where, if the analogy exists at all, they are in their minimum state of activity. We need not insist on the inadequacy of the hypothesis to account for the *production* of the matter of the Aurora itself, or for the progressive motions to which it is subject, which are too rapid to be occasioned in the manner supposed by the hypothesis; namely, by electrical "convection;" and yet too slow to consist of mere flashes of light. Nor need we urge the want of application of this hypothesis to the periodicity of the phenomenon; or to the vast extent of its simultaneous appearance; or of its commencing, reaching its maximum, and ending, at the same hours of the night in places widely differing in longitude.

M. De La Rive, of Geneva, has more recently put forth a still different form of the electrical hypothesis of the Aurora Borealis.† According to this distinguished philosopher, the electricity of the atmosphere owes its origin to an unequal distribution of temperature through the different atmospheric strata. He argues that through every sort of body heated at one of its extremities and cooled at the other, the positive electricity goes from the hot to the cold part, and the negative electricity in the opposite direction. Hence the lower end of an atmospheric column, being in contact with the earth, is constantly negative. The intensity is greater as the difference of temperature is greater; and it is greater, therefore, in summer than in winter, and greater in the equatorial than in the polar regions. The ground being rendered negative by contact with the negative columns, while the upper extremities are positive, there will be a tendency of the electricity to flow down from the top to the bottom, that is, from the upper regions to the earth, which tendency will be greater as the conducting power of the medium is greater, varying with the degree of humidity. On account of the greater humidity of the air over the poles, and consequently its greater conducting power, the electricity flows from the upper portions of the atmosphere within the torrid zone, each way to the north and south poles; and the earth being a good conductor of electricity, as is shown by the electric telegraph, where the ground is made the medium of discharge between two poles at a great distance from each other, the electricity conveyed to the poles through the upper regions, returns to the torrid zone through the earth.

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* Amer. Jour. Science, XXXII. 157.

† Bibliothéque Universelle, *New Series*, 3, 17.

In applying these principles to the Aurora Borealis, the author remarks, that it is the effect of the luminous electric currents, coursing the upper regions of the atmosphere towards the north pole, a consequence of the union of several unusual circumstances. When the sun has left the northern hemisphere, the vapors condense around the pole, and electricity flows in unusual quantities from the equatorial regions. The auroral vapor itself he considers as nothing more than the *icy particles* which are known to float in the upper regions of the atmosphere, sometimes producing lunar and solar halos; their presence in those regions being also affirmed by the testimony of æronauts.*

Upon the views of M. De La Rive I offer the following remarks. First, facts do not warrant the supposition that there is any such tendency of electricity to the poles as the hypothesis assumes, but they indicate just the contrary. Thunderstorms never occur beyond the latitude of 75°, and rarely beyond 65°, while they are most of all frequent and violent in the equatorial regions, where Auroras are seldom seen. Moreover, since the resistance to the passage of electricity *upward* is diminished as the air grows more rare, so much so that some writers (as Dr. Hare), maintain that the upper regions of the atmosphere correspond to the negative side of the Leyden jar, would not the supposed currents find a shorter route to an equilibrium than to go from the equator to the pole? Again, the auroral movements are progressive, and not instantaneous, like those of electricity. We may ask, moreover, are icy or snowy particles, like those which are here supposed to constitute the auroral vapor, ever known to become permanently luminous by being electrified? Are they ever known to be thus rendered magnetic, like the auroral columns? Are they known to reach to the height which, sometimes at least, the auroral matter attains, as, for example, one hundred miles above the earth? Can they be supposed to be all at once developed in such quantities as to overspread so great an extent of the earth as the Aurora sometimes covers, reaching, for instance, from Western Asia to California? Will the hypothesis account at all for the periodicity of the Aurora? Believing that a negative answer must be given to each of these interrogatories, I am compelled to think that the ingenious hypothesis of M. De La Rive is entirely inadequate to account for the phenomenon of the Aurora Borealis, and that it is in fact inconsistent with many of the phenomena.

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* De La Rive's Letter to M. Arago, An. de Chim. et de Phys., March, 1849.

Some who have admitted the inadequacy of any *electrical* hypothesis hitherto advanced to account for the origin of the Aurora Borealis, have ascribed the phenomena to *magnetism*. It must be admitted that magnetism has *some connection or other* with the Aurora. The disturbance of the magnetic needle during an auroral exhibition; the effect occasionally observed on the wires of the magnetic telegraph; the relation of the auroral columns to the magnetic meridian; the formation of the corona around the pole of the dipping needle,—these facts plainly indicate the existence of such a connection. But it may still be uncertain whether magnetism holds to the Aurora the relation of cause or effect. These facts merely prove that the auroral vapor has magnetic *properties;* they prove nothing respecting its *origin*, which is the main point in question. Matter derived from the planetary spaces, as meteoric stones, have usually been, to a great extent, ferruginous, a fact which favors the idea that the nebulous matter of which we suppose the Aurora to consist is of a similar nature, and therefore magnetic.

Scarcely any hypothesis alleged to account for a phenomenon of nature is so poor as not to explain some portion of the facts. Electricity and magnetism may severally account for some of the phenomena of the Aurora Borealis; but there are other facts which neither of these causes appears to me to touch, especially the production of the material itself, or auroral vapor, or the great extent of the auroral exhibitions, or their periodicity; while I believe that each of these facts is fully and satisfactorily accounted for in my paper already referred to, which assigns to the Aurora a cosmical origin.

4. ON A NEW SOURCE OF ELECTRICAL EXCITATION. By Mrs. EUNICE FOOTE, of Seneca Falls, N. Y.

I HAVE ascertained that the compression or the expansion of atmospheric air produces an electrical excitation. So far as I am aware, this has not been before observed, and it seems to me to have an important bearing in the explanation of several atmospheric and electrical phenomena.

The apparatus used was an ordinary air-pump, of rather feeble power, and adapted either to compress or exhaust the air. Its receiver was a glass tube about twenty-two inches in height and three in diameter, with its ends closed by brass caps cemented to it. At the bottom was a stop-cock and a screw, by which it was attached to the air-pump. To the top were soldered two copper wires, one hanging down within the tube, terminating in one or more points, and reaching to within about six inches of the bottom, the other extending from the upper side of the cap to an ordinary electrical condenser.

In experimenting after compressing or exhausting the air within the receiver, the wire reaching to the condenser was disconnected from it, the upper plate was lifted from its place by a glass handle, and its electrical condition tested by a gold-leaf electrometer. I have found it convenient first to compress the air and close the stop-cock, when the condenser would be found to be charged with positive electricity; then after discharging all traces of it both from the condenser and the wire leading to it, the air was allowed to escape, and the condenser would become recharged to an equal extent.

My experiments with this apparatus have extended over about eight months, and I have found the action to bear a strong analogy to that of the electrical machine. In damp or warm weather little or no effect would be produced, whilst at other times, particularly in clear cold weather, the action would be so strong as to diverge the leaves of the electrometer to their utmost extent. In warm weather, when no action would be produced, I have attained the result by cooling the air artificially. A sudden expansion or contraction always increased the effect.

The results with oxygen gas were similar, but I was not successful with either hydrogen or carbonic acid gases.

It is believed that the results which have been obtained on a small scale, in my experiments, may be traced in the great operations of nature. The fluctuations of our atmosphere produce compressions and expansions sufficient to cause great electrical disturbances.

Particularly should this be observed in the dry cold regions of our atmosphere, above the effects of moisture and vapors. And it was established by the experiments of Becquerel, as well as those of Gay Lussac and Biot, that the electricity of the atmosphere increases in strength with the altitude. A manifest relation, moreover, between the electricity of the atmosphere and the oscillations of the barometer has frequently been observed. Humboldt, treating upon the subject in his Cosmos, remarks, amongst other things, that the electricity of the atmosphere, whether considered in the lower or the upper strata of the clouds, in its silent problematical diurnal course, or in the explosion of the lightning and thunder of the tempest, appears to stand in a manifold relation to the pressure of the atmosphere and its disturbances.

The tidal movements of our atmosphere produce regular and systematic compressions twice in twenty-four hours. These occur with so much regularity within the tropics, as observed by Humboldt, that the time of day is indicated, within fifteen or twenty minutes, by the state of the barometer. Saussure observed a diurnal change in the electricity of the atmosphere, corresponding with the diurnal changes of the barometer. The electricity of the atmosphere, he observes, has therefore a daily period, like the sea, increasing and decreasing twice in twenty-four hours. It, generally speaking, reaches its maximum intensity a few hours after sunrise and sunset, and descends again to its minimum before the rising and setting of that luminary.

The incessant change or oscillatory motion which we discover, as remarked by Humboldt, in all magnetic phenomena, whether in those of the inclination, declination, or intensity of these forces, according to the hours of the day and the night, and the seasons and the course of the whole year, leads us to conjecture the existence of very various and partial systems of electric currents on the surface of the earth.

The electrical excitations that accompany the aerial tides are in constant motion. They pass around the entire earth once in twenty-four hours, and encircle it with successive waves of electricity of great extent. And I have imagined, that it might be worth the inquiry, whether this did not afford a cause sufficient to explain the induced magnetism of the earth and polarity of the magnetic needle.

Several circumstances seem to favor the supposition.

1. An intimate connection has been established between magnetic intensity and the electric tension of the atmosphere.

2. All magnetic phenomena show an increase of activity during certain hours of the day, corresponding to, and obviously connected with, diurnal atmospheric changes.

3. The recent investigations of Sabine and others have shown that terrestrial magnetism is subject to an annual variation, which depends upon the relative position of the sun and the earth, being at a maximum when the latter is in perihelion, and a minimum in its aphelion, in the same way that the tides are affected by the distance of the sun.

In like manner the magnetic intensity is affected by the position of the moon, and suffers a monthly change depending upon its distance.

4. Magnetic intensity is greater when the sun and moon are in conjunction than when they are in quadrature.

5. The diurnal variations in the declination and dip of the magnetic needle accord with the position of the sun in its apparent course around the earth.

6. The maximum of these variations also changes its sign at the time of the two equinoxes. Thus whilst the deflection is eastward, up to the 21st of March, a change then occurs, and it passes to the westward with a mean equal to the eastern variations of the preceding six months.

It has also been shown, that there is a variation in magnetic declination, dependent on the change of the moon's position in relation to the meridian of the place of observation.

7. Great magnetic disturbances or storms, extending sometimes to a great distance, I have found to accord with great barometric waves passing over the same space in separate and independent systems.

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5. On the Quantitative Assay of Chromium by Blowpipe Processes. By Eugene W. Hilgard, Ph. D., of Washington, D. C.

The great practical convenience of the mouth blowpipe, and the rapidity with which, in the cases most frequently recurring, its application led to satisfactory results, could not fail to assign to it, at an early period, an important place among the means employed for qualitative researches on inorganic bodies. Nevertheless, its usefulness, even in many quantitative determinations, is comparatively a modern discovery; not so modern, indeed, as not to have had time to become generally known; and it is truly a matter of surprise to those practically conversant with those elegant methods we owe chiefly to the genius of Plattner, that these are not more generally appreciated and employed. This is the case not only in this country, but also in Europe, where the details of the quantitative blowpipe assays are unknown to many analysts of the highest standing; even there, until recently, a full and systematic course of instruction in the use of the blowpipe was hardly to be obtained outside of the Academy of Mines, at Freiberg, in Saxony, where, until a few years past, Plattner himself taught the use of his favorite instrument. Having myself enjoyed the advantage of his personal guidance in the study of this subject, it has ever since continued a favorite with me, and it is thus I have attempted to still further extend the practical usefulness of these processes — not only those involving the immediate use of the blowpipe itself, but all such as can be conveniently and advantageously carried on by the aid of that admirably contrived micro-laboratory known as "Plattner's *Blowpipe Chest*," not excluding, of course, the addition, if necessary, of such articles as do not interfere with one of the cardinal virtues of the whole, *compendiousness.* The latter is one of the main conditions of its *practical usefulness*, *that* being in fact the paramount consideration, to which even that of extreme accuracy must in some cases yield.

The primary object of quantitative blowpipe assays is essentially a practical one; they are, first of all, conjointly with the qualitative tests, performed in like manner, to serve as a reliable guide to those who, while engaged in preliminary researches involving the useful metals, have no laboratory at their disposal, and also, in most cases, but a limited space of time. Thus in geological field surveys and explorations of mines; also in studying the character of the products during the progress of smelting or similar operations, etc. We have here two conditions, namely, *compendiousness* and *quickness of performance*, which notably reduce the number of processes applicable in this case to the quantitative determination of any one substance. The process that fulfils them, and at the same time allows of a degree of accuracy sufficient for practical purposes, will undoubtedly be of great service to practical men. If simultaneously it allows of reaching the degree of accuracy actually obtaining in the laboratory (differing essentially from that pretended to be attained by many who attach so much importance to the third and even the fourth decimal in their percentage calculations of analyses), its value will be greatly enhanced. Several of PLATTNER's quantitative methods fulfil not only the two former, but also the latter, condition. The blowpipe assay of gold, silver, cobalt, nickel, and even copper, if carefully performed, yields results quite as accurate as those usually arrived at by a much more laborious and circumstantial route, in the wet way. With respect to that of nickel and cobalt, we may unhesitatingly prefer it to all methods of determining these metals in the wet way, except that of LIEBIG, which, however, is not frequently resorted to on account of its lengthiness, and the annoyance occasioned by the evolution of so much hydrocyanic acid.

The cobalt and nickel process was entirely PLATTNER's own invention, and we may well call it the climax of all blowpipe achievements, if we consider that by its means we can, in the space of about three hours, determine consecutively in one and the same one hundred milligrammes of ore, no less than six metals, five of which with great accuracy. The silver, gold, copper, lead, and tin processes were, in their main features, known and practised in the assayer's furnace before PLATTNER adapted them to the blowpipe, a labor which, however, for difficulty and ingenuity required in carrying it through, is quite equal in merit to a new invention. This will be readily appreciated when we recollect the main conditions to be fulfilled, as before observed.

All the assays hitherto mentioned are performed in the dry way alone, and this is one of the main causes of their expeditiousness. The operations, constantly recurring in working in the usual wet way, of evaporation and filtration (not to speak of precipitates requiring from twelve to seventy-two hours' rest before they can be filtered), consume by far the largest part of the time of the operator; whilst in the dry way operations just mentioned, the purely mechanical manipulations take up only a very small quotum, most of the time being that which is requisite for the completion of chemical action. In all these cases the substance to be weighed is obtained in the form of a regulus or bead, which rarely requires more than simple wiping or squeezing to clean it from all adhering foreign matter. But even when this is not the case, namely, when in fusions powdery precipitates are formed, which require washing on the filter, an advantage is very generally gained in consequence of the greater expeditiousness which their washing by filtration admits of, as compared with that of the large number of gelatinous, slimy, and other precipitates obtained in the wet way, which so often clog the filter and levy a heavy contribution on the patience of the operator. There seems to be at present prevailing among analytical chemists a tendency to neglect the dry and employ the wet way exclusively, which would hardly seem justified by the comparative accuracy thus obtained. When an analysis requires ten or fifteen filtrations, and perhaps as many pounds of distilled water, so often of doubtful purity, and slowly but surely dissolving even the Bohemian breaker glasses, the chances of error are undoubtedly very great, and greatly enhanced by the lengthiness of the operations. To abbreviate these, and avoid their repetition as much as possible, is an important point, which, in not a few cases, may be gained by suitably combining the dry with the wet method. There are exclusive neptunists, however, in chemistry, as well as in geology, who will boast of "doing *everything* in the wet way."

The faults contingent on the ordinary method of filtering and weighing precipitates obtained in the wet way, are completely avoided in another class of analytical operations, usually designated as the "*titration methods*." Of late, this branch of analytical chemistry has been rapidly growing in importance; the elegant iodometric methods of BUNSEN have of themselves opened a vast field, from which further rich harvests cannot fail to be gathered in time; to the manufacturer the metric methods have already become of the highest importance. To the analyst, weary with filter and balance, when sometimes, after weeks of careful labor, he finds, in summing up his results, that according to them he has either created or annihilated a per cent. or two of matter, the burette is a real solace; through it he can recover his shaken faith, and convince himself that the third decimal may really sometimes have a meaning. The universal application of metric analysis seems indeed to be the promised land of analytical chemistry. Thus far, however, its application is still somewhat limited; and moreover, its usefulness in the field is, and probably ever will be, impaired by the bulkiness of the materials required, being mostly dilute solutions, which cannot be readily prepared in the field; and if so, the distilled water necessary would be sufficiently cumbrous to carry. When, therefore, we have to do with metals difficultly fusible or reducible, and which cannot be brought into any fusible alloy, nothing remains but to partially, at least, resort to the wet way. Such is the case with chromium, which, having at present become of considerable practical importance, I have for some time been attempting to devise a convenient method of determining it quantitatively by the aid of what is conventionally termed blowpipe processes, although all of them do not necessarily involve the use of that instrument. PLATTNER himself has proposed a mixed method of this kind for iron, involving, however, several tedious filtrations, in consequence of which it is, I believe, but rarely employed. I do not know that I have succeeded much better in the method I propose for chromium; yet it may prove of some use as enabling the explorer to execute in the field what thus far has only been done in the laboratory, and shortening the labor of those who, in manufacturing processes, have to repeat with frequency determinations of the metal in complex residues.

Nor can I claim having invented any thing essentially new; I have only attempted to adapt phenomena well understood before, to the particular purposes in view. In my description I shall proceed with reference to PLATTNER's book, and the well-known arrangements of his blowpipe chest; and I shall also follow his example with respect to minuteness in the description of the detail of the operations. In this respect some are inclined to find fault with PLATTNER, considering his minuteness as superfluous and doctrinarian. I believe, however, that all those addicted to the habitual use of the blowpipe will testify to the importance and usefulness of this particular point. In the analytical laboratory, the operations are comparatively few and simple, so that, with some general rules and experience, the analyst may, in most cases, at once successfully execute a new method, of which he only knows the rationale. With him, moreover, a few hours' longer duration of an analysis — in consequence, perhaps, of some precaution neglected, the necessity of which he could not foresee — is of much less consequence; allowing that much latitude, it may frequently be that there are several ways of doing the thing correctly. But no such latitude exists in those cases for which the blowpipe methods are specially intended; it must be done within the shortest possible space of time, and with comparatively limited means at disposal; the filtrates must not exceed in bulk what a certain very small beaker-glass will contain; and a crucible which serves very well in a fusion conducted in a certain way, will allow the mass to run over with a very slight deviation from that precise way. I have always found that I fared best in following to the letter those seemingly pedantic rules of Plattner; and whenever I deviated from them, I found myself almost unconsciously returning to them after a short excursion. They are the results of thorough practical experience of a head as thoroughly practical.

I shall therefore describe minutely that method of manipulation which I have thus far found to answer best. But my experience is short; and as moreover I cannot lay claim to the high qualities of my illustrious teacher, there is room left for a great deal of improvement.

The facility with which chromium may be separated from most other bases, by conversion into chromic acid, by means of fusion with alkalies and simultaneous action of oxidizing agents, immediately suggests this as the first step in the assay.

In the qualitative test, this fusion is effected in the large platinum spoon, by means of the blowpipe flame itself. The great fluidity of the flux, and the strong evolution of gas accompanying this process, render a considerable loss by spirting unavoidable, unless the vessel in which the fusion takes place is covered. In this case, however, the free access of atmospheric oxygen is cut off, and it is necessary to supply this want by the addition of an oxidizing flux. As such, chlorate of potash may occasionally be used; but the *nitrate* is by far the most convenient, because admitting of a higher temperature; besides, it is already a necessary inmate of the re-agent case.

Even when vessels as large as the large porcelain blowpipe dishes are used, loss by spirting occurs. I tried to render the flux less fluid, or more tenacious, so that in bursting, the gas bubbles would not throw up any of the mass. Borax will not do this, but a very slight addition of phosphate of soda (a crystal of microcosmic salt fused) diminishes the spirting very much; yet it does not entirely prevent it.

The common clay crucibles covered with a torrefaction dish, will not, of course, answer, on account of their porosity. Porcelain crucibles are so much attacked by the flux as to be useless after two or three short fusions, and would thus, in the end, turn out very expensive. I therefore resorted to a platinum crucible of precisely the pattern of the clay ones, with a platinum torrefaction dish for a cover.

With these, the fusion may easily be executed without the least loss, the cover being put on with the convex side downwards. It is but slightly out of shape on one side, so as to allow an opening for the escape of gas. This is essential; for if it fits pretty close all around, and the fused mass inside happens to rise near to the edge, the whole seam is instantly closed gas-tight all around, in consequence of capillary attraction, and immediately after the cover is forcibly lifted off by puffs, each of which ejects a few globules of flux. This orifice, therefore, must always be kept very hot during the fusion, to prevent its being closed up by some of the fluid mass solidifying around it.

The use of the hydrate of potash, which of all decomposes quickest the chromic iron ore, involves several inconveniences. The commercial article always contains more than its water of hydration, in consequence of which it fuses at a low temperature, and then boils very noisily in losing its excess of water. Moreover, it attacks the platinum so powerfully in a lengthy fusion, that no crucible of ordinary thickness could survive a great many of these.

The carbonates of potash and soda are much more manageable; and there are cases in which they cannot well be dispensed with, as will appear hereafter. They do not sensibly corrode the platinum, but their strong effervescence with some ores is somewhat troublesome.

In most cases I now confine myself to the use of nitre alone, which I find to answer the purpose perfectly. The anhydrous potash formed by its decomposition at a high temperature, has very little effect on the platinum, — incomparably less than the hydrate, and the action on the ore is nearly or quite as rapid as in presence of the carbonates.

When after this fusion, the mass is lixiviated with water, most of the bases remain either in the free state or in combinations insoluble in an alkaline solution.

When the alkaline earths are present, (which in a fusion with nitre would in many cases remain in the caustic state,) the chromic acid will be rapidly withdrawn from the alkaline solution to form insoluble chromates; it is necessary, therefore, to combine those bases with some other acid forming an insoluble salt with them. In this case some carbonate of potash or soda may be used together with the nitre, since the carbonates thus formed remain unaltered in an alkaline solution of chromic acid.

When lead, tin, bismuth, or cadmium is present, the complete separation of the chromic acid is more difficult. Chromate of lead may indeed be decomposed by fusion with nitre or carbonated alkalies; but as soon as water is added to the fused mass, the reverse decomposition takes place, the lead assuming the chromic acid, and leaving carbonate of alkali in solution. Even the sulphate of lead is converted into chromate by contact with a solution of alkaline chromates. With the other metals mentioned, the case is similar.

The separation may, however, be successfully effected, when in the fusion opportunity is given to the oxides of forming silicates. When, therefore, as in most cases, the ore itself does not contain the requisite amount of silica, the latter must be added in the flux. When chromate of lead is intimately mixed with an excess of finely divided silica, either precipitated, or, better still, such as is obtained from fluosilicic gas, the decomposition, in the fusion with nitre, is complete; the silicate of lead formed does not retain a trace of chromic acid, when washed with water. Tin and bismuth behave in precisely the same way; I have not tried cadmium, but it can be foreseen that it would act similarly. It will be observed, that the introduction of silica into the flux can also be applied to the case of the alkaline earths, which, in some cases, might be preferable to the use of the carbonates. A basic silicate of potash, of course, remains in the alkaline solution; and with it most of the acids that may have been present in the ore, as such, or formed by fusion, namely, chromic, vanadic, manganic, molybdic, tungstic, titanic, tantalic, niobec, antimonic, arsenic, phosphoric, and some of the other non-metallic acids. As for the silicic acid, it is easily got rid of by adding to the solution, before filtering, nitrate or carbonate of ammonia. The manganic acid is, at the same time, thrown down by the addition of some alcohol — not acetic acid — since that might dissolve some of the bases, and would, besides, unnecessarily neutralize the solution. After filtering the solution, it remains to bring the chromium into some weighable shape, separating it at the same time from the foreign substances still in solution. When none of the fixed acids are present besides the chromic, the latter might be at once precipitated by proto-nitrate of mercury in the neutral solution. The condition but just mentioned would alone restrict the use of this method to a small number of cases; but however convenient and accurate, when several hours can be allowed for the separation of the precipitate, the results obtained fall short of the truth seriously, when, as in the field, no such delay is admissible. In practice, therefore, we should probably dispense with its employment altogether. When fixed acids are in solution, a simple method of separation suggests itself, in the evaporation to dryness and fusion of the residue with a mixture of carbonated alkalies and cyanide of potassium. Most of the acids thus enter into combination with the alkali, while the chromic acid is reduced to the sesquioxide, and might, it seems, be obtained pure by filtration. Unfortunately, however, the chromic oxide thus obtained always contains a considerable amount of potash, which it will not part with on any consideration short of a fusion with nitre; the quantity of potash thus contained is, moreover, quite indefinite and variable. One more filtration being about the greatest allowance still to be made if the process was to be useful in the field, I had to reject several other methods on this consideration. Turning to the extreme opposite of my first plan, I sought for a compound of chromium that should be insoluble in an *acid* solution of the accompanying acids. Among these, the insoluble modification of chrome alum seemed to answer best, and I have finally based the determination of the metal upon the formation of this compound. It is well known that when solution of chrome alum, rendered acid by oil of vitriol, is heated to 200° centigrade, a light green powder is precipitated, the formula of which is also, KO,

$SO_3 + Cr_2 O_3, 3 So_3$ — anhydrous chrome alum. This singular substance is almost entirely inert; it is perfectly insoluble in water, is not decomposed by boiling acids, but may be slowly and imperfectly decomposed by long boiling with potash ley. Even nitre attacks it with more difficulty than it does the pure oxide. By ignition, it is converted into a mixture of chromic oxide and sulphate of potash, in the same atonic proportions as in the original salt. The transformation of the chrome alum into this salt is not, however, complete at 200°; it is only an indefinite quotum of the whole which thus passes into the insoluble state. When the solution is further heated, until the oil of vitriol begins to volatilize, the conversion is complete; and upon dissolving the mass in water, traces only of chromium remain in solution, while a light green powder falls to the bottom. This has the same composition with the salt before mentioned, but it differs in being decomposable by hot water, which gradually resolves it into sulphate of potash and insoluble tersulphate of chromic oxide. The decomposition is slow, and requires a long time for completion; yet it begins very soon when the temperature is raised. At ordinary temperature the salt is not changed by water. I have not had leisure to decide experimentally whether soda and ammonia form a similar salt with tersulphate of chromium. Their presence does not influence the result when an excess of potash salt is present.

The formation of the salt, as far as my experience goes, takes place quite independently of other acid substances present, so long as there is an excess of sulphuric acid or of bisulphate of potash; so that all acids soluble in an acid solution of this kind, (to which other solvents not otherwise interfering might, if necessary, be added,) may be separated from the chrome salt by mere washing.

The washing of this salt on the filter presents some difficulties. So long as an abundance of other salts is held in solution by the liquid, the precipitate remains powdery and does not clog the filter; but as soon as the fluid becomes very dilute, the powder diffuses and begins to pass through the filter, which very soon becomes most effectually clogged, putting an end to all filtration.

Washing with a saturated solution of sal ammoniac prevents the diffusion and expedites the washing; yet it cannot thus be done in less than three or four hours. The process becomes more expeditious when, instead of immediately throwing the precipitate of the filter, it is washed by the method of partial decantation, proposed by Rose, which so essentially facilitates the washing, otherwise so tedious, of such precipitates as alumina, etc. The powdery chrome salt settles down pretty quickly into so small a volume, that with even the small beaker glasses of the blowpipe chest, a coefficient of progression as high as 25 or 30 is easily obtained; so that after four or five decantations, the washing is complete, and the precipitate may be thrown on the filter, dried and ignited.

Withal, this is sometimes a very slow process. I have occasionally succeeded in thus washing the precipitate in 35–40 minutes; but sometimes, from causes not always controllable, it requires a much longer time to settle; or if the liquid be filtered while turbid, the filter becomes clogged, and then, in consequence of the large bulk to be filtered, the operation becomes interminable. At the same time the quantity of sal ammoniac necessary to form so much of a saturated solution, is of quite unblowpipelike proportions. I could find no other salt to answer better, or even as well, as the chloride of ammonium.

I therefore attempted to involve the powdery precipitate of the chrome salt in another one, which should settle quickly and take the other with it; being, at the same time, of such a nature as not to interfere with the separations, and completely volatile in the ignition. The method applied by Knapp and Árendt to the washing of the ammonia phospate of uranium, (shaking with a few drops of chloroform,) however well it answers for bulky precipitates, seems to be inapplicable to powdery ones. Sulphuret of carbon did no better than the chloroform; nor any organic precipitate like gum arabic or glue by alcohol or tannin, and others I tried.

Resorting to the salts of mercury, (most of the persalts being soluble in an acid solution,) I tried first the iodide, which answers pretty well, but has the inconvenience of being required to be precipitated in considerable quantity. The chloro-sulphide, such as is precipitated from a solution containing an excess of perchloride by a dilute solution of sulphuret of ammonium, answers better; and but a very small amount need be thrown down to clear the liquid perfectly, when the washing may be done either on the filter or by decantation, in the space of 35–45 minutes.

It might be objected, that in case of the solution containing antimonic oxide, arsenic, or molybdic acids, precipitable by HS, a precipitate of the sulphurets of these metals might be formed, the two latter of which would subsequently increase the weight of the residue. Such a precipitate may indeed be locally formed; but being acted upon by a solution of perchloride of mercury, an exchange of elements would take place, by which the chlorides of those metals would pass into solution, and still chlorosulphide of mercury would ultimately be formed. In most cases, the flocculent olive-green precipitate thus formed settles quickly, leaving a clear supernatent liquid. The presence of some substances, especially antimony, has a tendency to prevent clear settling.

After washing, which must be done with a solution of corrosive sublimate, the filter is dried and ignited. It is incinerated with great difficulty, like filters containing the ammonia-phospate of magnesia — 100 of ignited residue correspond to 47.3 of chromic oxide.

When ignition is continued too long, and with free access of air, at a high temperature, chromic acid is formed, which unites with a part of the potash present, expelling the sulphuric acid. If the ignition is continued before the glass-blower's lamp, at a strong yellow heat, the precipitate keeps losing weight steadily, and a considerable portion may thus be converted into the chromate.

It is best, therefore, before weighing, to moisten the ignited residue with a drop of sulphuret of ammonium, then drying and gently reigniting it. Any chromic acid formed is thus reduced, and the potash reconverted into sulphate.

I shall now describe specially the mode of operating, with the modifications necessary when certain of the metals or acids are present.

A. Fusion with Nitre.

A. When neither Lead, Bismuth, Tin, nor Cadmium, is present in the Substance under Examination.— Such is the case among the minerals, with chromic iron ore and chrome ochre. Among the artificial products, the commercial chrome green frequently comes under this head; but it is also very frequently mixed with foreign substances of various kinds.

The chromic iron ore must be reduced to the finest division possible, otherwise the decomposition remains incomplete, or the fusion is necessarily very lengthy. The finest of the powder of the pure mineral may be obtained by elutriation, by means of two beaker glasses. Or else, the ore having been reduced to considerable fineness in the agate mortar, it may be sifted through fine gauze tied over a beaker glass, so as to form a shallow bag, over which paper is subsequently tied, so that in shaking or striking the glass against the palm of the hand, the powder is retained, and its finest parts, passing through the gauze, are collected in the beaker-glass. But in mixed ores, formed of substances differing much in their physical properties, neither of these processes is applicable; and nothing remains but to reduce them to the proper state of comminution in the mortar.

Chrome ochre and artificial chrome green are oxidized with violence by the nitre, and need not be so carefully comminuted.

Having weighed off 100 milligrammes of the chromic iron ore, or from 50 to 75 of the chrome ochre or chrome green, the substance is poured into the small platinum crucible, (which, like the clay ones, is usually supported on the smaller cupel iron,) and the latter placed on the triangle of the iron ring of PLATTNER's lamp. In the case of the chromic iron ore, about three large spoonfuls of nitre are added at once; the crucible is then covered with the platinum torrefaction dish, in such a manner that the orifice left for the escape of the gas is sideways; in order that by a simple movement of the support round its pivot, this orifice may be approached to, or removed from, the flame. The spirit lamp is then provided with the iron chimney, which in this case is best set on the lamp itself; for which purpose it must have three feet, about five millimeters high, attached at its base, so as to admit a current of air. The flame is so adjusted that the point of the luminous cone is slightly truncated by the bottom end of the crucible.

The heat thus produced is fully sufficient when nitre alone is employed; if raised higher, so that the upper rim of the crucible becomes very hot, the nitre ascends by capillary attraction, and then spreads over the outside of the crucible, until it reaches the triangle wires; in which case, if these are iron, the process must be abandoned. By using a triangle of platinum, this danger would be avoided, since *that* might subsequently be washed off like the crucible itself. Such a triangle would be particularly useful in long fusions, where an efflorescence of the nitre on the outside is unavoidable; although, unless the heat is unnecessarily raised, the difficulty may easily be avoided in most cases. The lamp being placed on the circumference of the circle in which the support moves round its pivot, the crucible is moved over the flame and left until the effervescence of the nitre can be distinctly heard, when it is quickly moved to one side, so as to allow the flame to play mainly on the orifice side of the crucible. In this way it is very easy to regulate the action in such a manner that no loss may accrue from excessive effervescence. Even if the molten mass should rise so as to touch the cover, it cannot lift the latter off so long as the orifice is sufficiently large and kept very hot.

In chromic iron ores, the effervescence generally ceases very soon; the crucible may then be moved vertically above the flame, which may also be raised a little, and the whole left quietly for about twenty-five or thirty minutes, at the end of which, if the ore has been sufficiently comminuted, the decomposition is complete; the crucible is removed from over the flame and left to cool. The oxidation of the chrome green is so rapid and violent, that if the whole of the nitre were added at once, loss would be unavoidable. To fifty milligrammes of this substance, therefore, only about half a spoonful of nitre must be added at first; the whole is then heated with the precautions before described, the nitre being added by half spoonfuls, and no sooner than the effervescence caused by its predecessor has completely subsided. At each of these interruptions, the cover is lifted off while the mass is fluid, by means of the pincers, which must be quite clean at the points, so that if any thing should adhere to them they may subsequently be washed off into the principal solution. Great care must be taken not to lose any thing from the cover, to the lower surface of which a large drop is generally adhering towards the end of the fusion; it must not therefore be laid down, but kept in hand; the mass remaining in the crucible is allowed to cool until the surface solidifies, before the nitre is added. If dropped into the melted mass, small particles are frequently thrown out of the crucible. When, on account of the presence of alkaline earths, carbonate of soda or potash requires to be added (a spoonful being about as much as is ever required), it may be thrown into the crucible at once, with the first dose of nitre. Such fusions require a higher temperature than when nitre alone is used, and there is less danger of loss by efflorescence. A little experience easily enables one in most cases to judge when enough nitre has been added, and the oxidation is complete. In the chromic iron ore, no criterion for the completion of the process is available, save subsequently testing the washed residue for chromium.

b. *When lead, bismuth, tin, or cadmium (either or all of these metals), is present in the ore.*—To this class belong the yellow-lead ore, vanquelinite, and a few rare minerals; besides, mixed ores containing these; and of artificial products, chrome red and chrome yellow; chrome green also sometimes is mixed with carbonate of lead. 100 milligrammes of such substances having been weighed off, are transferred to the agate mortar, and there intimately mixed with fine silica, of which an excess must be used, involving different quantities according to the state of division it is in. Of silica obtained in the preparations of hydrofluosilicic acid, three to four spoonfuls, as full as may be, are requisite; it loses its voluminosity in mixing with the ore. Of silica, such as is usually obtained in analysis, about the same nominal amount, or a little less, may be used,—its spoonfuls being only about half the volume of those of the former; at the same time, not being so finely divided, it does not act so readily, and proportionally more is required. After mixing the ore thoroughly with the silica, two heaped spoonfuls of nitre are added and triturated with the mixture. This serves to remove or loosen most of what has adhered to the mortar and pestle. The crucible is then set on the triangle, a spoonful of the mixture thrown in, and the process conducted as before described, adding a spoonful of the mixture at a time. When all has been removed from the mortar as far as the spoon will do it, half a spoonful of nitre is thrown into the mortar and triturated, removing, as much as possible, all that still adheres. The nitre is then transferred to the crucible, and the same operation performed with three more half spoonfuls. The mortar is thus effectually rinsed, and all the ore removed. When the decomposition is complete, the fluid mass may be observed to be perfectly clear in the centre, while to the sides and bottom of the crucible flakes of silicate are adhering. During the cooling it is generally easy to see whether or not the chromate has been perfectly decomposed, for while the flakes of the silicate appear white or yellowish, and evidently colored by the flux alone, undecomposed particles of metallic chromate will appear dark red, and pass into orange yellow on cooling. When, in consequence of incomplete mixture or an inadequate supply of silica, such particles are still to be seen in the mass, no further addition of silica will remedy the evil, and the process has to be repeated with another quantity of substance.

After such fusions, the sides of the crucible frequently appear blackened, but this does not seem to injure them. In most cases, ignition alone, and in all, fused bisulphate of potash, will remove the discoloration.

Most of these fusions, always excepting that of the chromic iron ore, can be made in the space of from five to ten minutes. When performed at a high temperature, it sometimes happens that much of the nitre is volatilized, and the mass remains almost dry. When this happens it is always best to add another spoonful, lest the decomposition might have remained incomplete.

B. Further Treatment of the Melted Mass.

In but a few cases the fused mass will dissolve in water without leaving an insoluble residue, from which it must be separated by filtration. If manganese or silica were present, they will in part have passed into solution, and must be removed before filtration. The manganate of potash is easily removed by the addition of a few drops of alcohol; in most cases it would be destroyed, and the manganese retained, by the paper of the filter. When, as in the majority of cases, perhaps, there was silica present in the ore, a part of it may usually be supposed to pass into the aqueous solution, from which it is best removed by either nitrate or carbonate of ammonia.

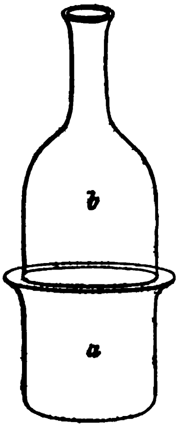
The former rather deserves the preference over the other, for completeness of precipitation. But a solution in which SiO_3 has been precipitated by AmO, No_5 will not filter well unless previously either shaken or boiled; stirring alone will not readily induce the SiO_3 to collect in flakes. When there is any objection to heating the solution, as when antimony was present in the ore, the crucible and cover, after having cooled down sufficiently, are transferred by means of the pincers, to a small beaker containing about one quarter inch high of water, and is there left until the mass is dissolved; the solution is then poured off into a test tube, of which it fills about two thirds; it is not necessary to rinse the beaker, since what little remains in it will not afterwards interfere with the filtration. A spoonful of nitrate of ammonia is then added, and closing the test tube with the thumb, it is well shaken. Subsequently the solution is poured back into the beaker, and test tube and thumb well rinsed. When, as in most cases, the boiling of the solution is admissible, the solution of the melted mass may be accelerated, while, at the same time, the silica is more safely precipitated, by boiling it, crucible and all, in the same vessel which subsequently serves for evaporation, and which will be presently described.

The boiling need not be continued longer than half a minute, after which the solution is poured over into a beaker, the crucible and cover being likewise transferred by means of the pincette. The evaporating vessel and its cover are rinsed as economically as possible, and set up for evaporation; which is to be carried on simultaneously with the filtration.

The filter used ought not to be too small, otherwise the operation becomes too lengthy. When, in washing a precipitate, the rapidity of filtration of the fluid falls short of a certain ratio, it becomes disadvantageous to wait for the draining of the last few drops, which, in consequence of the decrease of hydrostatic pressure, may require as much time as the nineteen twentieths which have preceded them. The same, of course, applies to small filters. A two inch filter is about the average size I have found most advantageous in present instance.

So soon as the principal solution has passed through the filter, it is transferred to the evaporating vessel, which I must say a few words about. The evaporation must be done in the same vessel in which, subsequently, the fusion with bisulphate of potash is to be effected. A common porcelain blowpipe dish answers the purpose imperfectly; it is generally too small; and no *one* vessel will resist many fusions of the bisulphate in it; besides, it requires a long time to cool. But, above all, it has the inconvenience of obliging the evaporation to go on very slowly, for fear of loss by spirting. The evaporation may be performed very safely and expeditiously in a little glass flask, with a neck about two and a half inches long; the fluid may then be kept in a state of violent ebullition without fear of loss, if the flask is not much above half full. It is not easy, however, to find a glass that resists repeated evaporations and bisulphate fusions, and thus the experiment is frequently lost. I find that all these objections may be obviated by using as an evaporator a platinum dish, of the same form as the porcelain ones, but somewhat larger and higher. On this is set a small bell glass, shaped like the upper half of a champagne bottle, open at the top, and of such a diameter at its base as to fit the platinum dish just

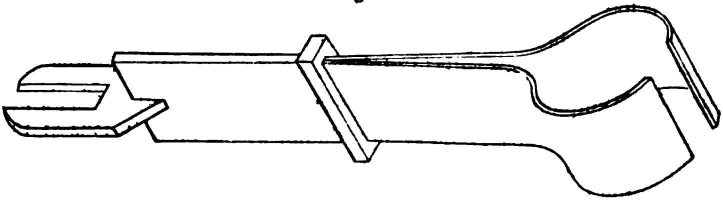
Fig. 1.



at the point where the curvature of the rim begins, as shown in fig. 1, which represents the whole in half size. The dish being set on the grate, placed at the proper height for the spirit-lamp flame, the little bell glass cover is clamped by its neck into a holder (fig. 2, full size), which in its turn is clamped into the arm which, at the same time, carries the filtering support; both must, therefore, be put on before the filtration begins.

In this evaporator, which is useful in a great many other operations, the fluid may be kept boiling violently all the time, even when the dish is filled up to nearly where the glass is set on. The seam where the two meet is closed up with fluid by capillary attraction, and there is no danger whatsoever of any loss occurring there, unless the lamp is so placed as to heat the rim itself and make the fluid boil there. Nor does it matter if the froth inside rises up to the neck of the glass cover; the whole may in fact be considered as a close flask. The evaporation usually goes ahead of the filtration; from time to time, as the filtrate accumulates, it is poured into the evaporator from above, the lamp having for a moment been removed. The fluid ought to be poured into the middle, so as not to touch the glass.

Fig. 2.



When an alkaline solution of chromate of potash and nitre is evaporated in this manner, some of the salt usually effloresces on the rim, causing inconvenience, and liability to loss. To prevent this it is best to add the sulphuric acid and bisulphate of potash before the evaporation begins. The oil of vitriol is added, drop after drop, until the yellow color of the solution changes to orange, and after that, four or five drops more. The amount of *bisulphate* to be added varies according to the amount of nitre that has been previously used; three to six or seven heaped spoonfuls are the average — an excess does no harm.

The last two drachms, or thereabouts, of the filtrate are reserved until the fluid in the evaporator has been reduced nearly to the point when the oil of vitriol begins to volatilize, and heavy fumes of nitric acid are seen to pass off. The bell cover is then removed by raising the arm that carries it so far as to allow of moving it sideways from the dish. It is then unclamped and rinsed into the dish by means of the remaining part of the filtrate, first on the outside of the lower extremity, then, being inverted, on the inside, like a funnel; finally, a few drops of distilled water from the dropping bottle finish the process. It is well also to wash down what may have effloresced on the rim of the basin; all these washings ought not to fill the latter more than one third full. In order to perform the rest of the evaporation and the subsequent fusion quickly and safely, the basin is now covered with a platinum cover of watch-glass shape, which must be smaller than the outer circumference of the rim of the basin. The boiling process is then resumed, the flame being somewhat lowered so as not to make the ebullition too violent; there is generally very little danger of any loss, unless the cover fits *too tight*. After some time, a frothing noise commences inside, announcing that the last of the water is escaping, and shortly after sulphuric acid fumes appear. At this period the mass inside forms a beautifully green syrupy liquid, which, on cooling, crystallizes; but on being further heated, it suddenly turns of a peach-blossom color, and becomes turbid with the double salt of chrome and potash. The heating is continued until *dense fumes* cease to be evolved, when only fused bisulphate of potash remains. The lamp having been removed, the cover is rapidly lifted off with the pincers, taking care not to lose any of the fluid mass which may be adhering to its lower surface; being allowed to cool an instant, it is laid inverted on the bottom of one of the porcelain dishes turned upside down. The basin is then seized by the rim, and, while the salt is still fluid, it is inclined and turned round and round, so as to spread the mass as much as possible. After it has solidified, the basin being replaced on the grate, six to eight drops of oil of vitriol are added, the cover is put on, and the vessel again heated until dense fumes of the acid rush out from under the lid. Removing the fire, the operation before described is repeated. The mass will now be found to remain fluid much longer; it may be rapidly cooled by dipping the bottom of the vessel in water, always taking care to spread the mass as much as possible. When perfectly cooled, it is of a light green color; if any peach-blossom colored particles are observed, it is a proof that too little potash has been used; more must then be added, together with some drops of sulphuric acid, and the whole reheated until the SO_3 is driven off; after which the mass is softened as before. It ought to remain soft enough to be readily dug up with a spatula or glass rod. If quite hard, it would take a long time to dissolve it in water, since it must not be heated; if too soft or quite fluid, so much heat would be evolved in dissolving it that the composition of the precipitate might be changed. In either case, therefore, the mass must be reheated, either to volatilize the excess of acid, or, having added a few drops more, to incorporate these with the rest of the mass. Not unfrequently a tough, green syrup is thus obtained, which very readily dissolves; at others, the mass is soft, but solid and crystalline. Next, the *cover* being taken up with the pincers, it is rinsed off with the washing bottle into a beaker glass. What part of the mass adheres to it is generally very soft and dissolves easily, containing as it does an excess of acid. So much water may be used for this purpose as will half fill the beaker; for to the *main portion* of the mass so much water must be added at once as to prevent entirely its getting sensibly heated, and for the same reason it is advantageous to scratch up and divide the mass as much as possible. A small platinum spatula, made of a piece of thick wire, is of great service in this and many other processes; it also injures the platinum basin less by scratching than a glass rod.

When all is dissolved (which, by aid of stirring and scraping, may generally be effected in the course of a minute), the solution, with the precipitate, is poured back into the beaker. Should any undissolved portions remain in the dish, they are loosened and then washed off by some of the solution, which is poured back for the purpose. A little more water is subsequently poured into the platinum dish, and the index finger of one hand (*clean*, of course,) is used to rub off from the bottom and sides such parts of precipitate as still adhere. I must here observe, that, unless the inner surface be kept very clean and smooth, some of the chrome salt may remain adhering so firmly as to resist all the rubbing that can be applied; it must be removed by fusion with saltpetre. As for the index finger, I know nothing to supply its place in cases of obstinate precipitates, and it is generally much less liable to occasion loss than a feather. The turbid fluid is then poured over into the beaker. Further rinsing may be deferred until the first liquids have passed through the filter.

About one third, or even less, of an even spoonful of powdered Hg Cl is now added, and rapidly dissolved by stirring; after which, some very dilute Am S_5 is added, drop by drop, taking time between each to stir violently for a little and observe whether or not the fluid is clear above the flakes of chlorosulphide of mercury, which collect and encase the powdery chrome salt. The clearing generally takes place when the precipitate becomes of an olive tint, when but a slight excess of Hg Cl remains. Great care must be taken to have all the precipitate well stirred up, for if a little of the chrome salt happens to remain at the bottom, it afterwards diffuses itself and gives trouble.

The flakes of the chlorosulphide settle almost instantly, and the filtration may be begun at once. I generally use two-and-a-half-inch filters, either of Swedish, or other filtering paper which has been extracted with acid, so as to give as little ashes as possible, and sufficiently constant in the same paper. In filters as small as these, advantage may be taken of the circumstance that the filtration is considerably accelerated when the filter does not fit closely to the funnel. I use small funnels made before the lamp, inclosing an angle of only about fifty to fifty-five degrees. When a regularly made filter is put into one of these, the upper edge may be adjusted closely, as it always ought to be; but its sides will gradually diverge from those of the funnel, and thus the greater part of it will hang freely inside; the weight of fluid being too small to break down the paper.

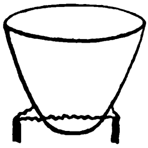
When the first solution has passed through, the precipitate is generally best thrown on the filter and washed with solution of sublimate, a small dose of which is put on the filter each time before the water is poured on, not forgetting, of course, to rinse both the platinum dish and the beaker. This is always best done with the washing bottle, using a powerful stream, so as to stir up the precipitate as much as possible. In this way the washing may generally be finished in from thirty to forty-five minutes, its completion being announced by either the evaporation test, or by a small granule of chloride of barium, added to a portion of the filtrate collected in a test tube not producing any more turbidity.

It is desirable that the precipitate should be spread over the surface of the filter as much as possible, and not collected in the lower part of it. When the last of the water has drained off, the filter being raised with the pincers on its tripled side, is carefully taken out and laid on the one half of a piece of blotting (or filtering) paper doubled several times. The paper is then doubled over the filter and pressed between the fingers, gently at first, so as not to squeeze out any of the precipitate. When dried as far as feasible in this way, it is doubled up in the usual manner, so as to form a small square package, which is further pressed between the paper, so as not only to lose most of its water, but also to remain firmly doubled. In view of this, it is well to avoid the lower end of the filter being outside; usually containing, as it does, most of the precipitate, it easily bursts open, and endangers the experiment.

The filter is now pressed into the platinum torrefaction dish; it may be carbonized, and most of the mercury volatilized over the spirit-lamp flame, being set outside into some place where the fumes can escape without molesting the operator. This done, a charcoal furnace is prepared in the same manner as when a copper ore is to be roasted, and the dish being set on the ring, the whole is heated by the blowpipe flame, with which it is easy to create such a draught of air as to quickly burn the filter, especially when, after incinerating the upper side, it is turned and broken up by means of a spatula, the vessel being, of course, taken out, and set on a piece of white paper to avoid loss.

When the incineration is complete, the torrefaction dish being removed, the furnace door is closed, and a charcoal furnace cover prepared. The incinerated mass is then transferred from the torrefaction dish (in which it lies quite loosely) to the platinum crucible, previously counterpoised on the balance together with a small platinum wire tripod, which serves to keep it upright on the scales (fig. 3, full size). It is advantageous to have a very thin crucible expressly for this purpose, so as to avoid weighting the balance too much; for in order to resist the *fusions*, the crucibles used for these must have some thickness of metal. The crucible, being set on the ring, is covered with the dish, the furnace cover put on, and as high a temperature as possible kept up for five to six minutes. After this, the precipitate will appear of a full green color all over, except

Fig. 3.



some parts which may be semifused. A piece of carbonate of ammonia may be put in, and the heating resumed until it is volatilized; but if a full heat has been given, so as to make the flame dart out at the chimney hole, there is no need of this precaution. The crucible, being taken out, is set on the triangle of the lamp, and a drop of concentrated Am. S_5 (sulphuret of ammonium) is left to fall on the porous mass, which instantly absorbs it. It is then cautiously dried over the spirit-lamp flame, and finally ignited, so as to drive off all the sulphur. If any chromate had been previously formed in the ignition, it has been thus destroyed, and the weight of the substance thus treated, deducting the filter ashes, may at once be taken as correct.

One hundred of ignited residue correspond to 47.3 of chromic oxide. The result may be verified by washing the residue on the filter until the sulphate of potash is dissolved, when the chromic oxide may be ignited and weighed. After deducting the filter ashes, the result will be found to coincide very nearly with that deduced from calculation; generally a slight excess is thus found. In repeating seven or eight times the determination of twenty-five millegrammes of chromic oxide, mixed with various other substances, and varying the circumstances a good deal, the results never varied more than 0.1 of a millegramme. In reading the detailed and circumstantial description, it may appear to some that the process itself must be lengthy and tedious. In practice, however, if all the precautions be observed, a determination of chromium may, in most cases actually occurring, be performed in the course of one and a half to two hours.

It only remains for me to mention a few precautions, necessary in particular cases; some of which, it is true, hardly ever occur in practice. It is sometimes the case that in the first filtration, the filtrate comes through turbid after the solution has become dilute. When this happens with substances containing tin or bismuth, it is a sign that a sufficient amount of silica has not been added in the fusion. It always happens when antimony is present, and may be avoided by throwing some nitre on the filter.

In general, when any of the metallic acids are present, it is advisable to render the sulphate solution more strongly acid, by adding a few drops of oil of vitriol, after solution has taken place. It is also best to wash somewhat longer, especially in the case of phosphoric acid.

Phosphoric, *arsenic*, *boric*, and *molybdic* acids require no precautions beyond those just mentioned. The same is probably true of the *titanic*, which I have not experimented on, for want of material.

Tungstic Acid. When this acid is present, it is precipitated from the acid solution, and causes violent bumping during the evaporation. It does not interfere with the formation of the chromopotassic sulphate. On dissolving the fused bisulphate mass, the tungstic acid prevents entirely the diffusion of the precipitate, which appears flocculent, and settles easily, so as to render superfluous the use of the mercurial salt. After the main solution has passed through, the filter is washed with water once or twice; six or eight drops of strong ammonia are then successively dropped on the filter; and after waiting a few minutes the filter is filled up with solution of sal ammonia. Subsequently the washing is completed with the same solution, (which need not be saturated,) to which some drops of caustic ammonia have been added.

The tungstic acid is thus completely removed, and an accurate result obtained.

Antimonic acid. After the fusion with nitre, the solution must be effected without boiling, and after addition of nitrate of ammonia, the liquid must be shaken in a test tube, to make the silica collect. The filter must be washed with solution of nitre.

After the bisulphate fusion, a spoonful or two of *tartaric acid* are thrown into the beaker before rinsing into it the cover of the platinum dish. The solution is kept very acid, and plenty of corrosive sublimate used in washing.

I have not experimented on *selenic* and *telluric* acids; but it is probable they would cause no difficulty. *Tantalic* and *niobic* acids would seem to require a third filtration; however, they are not likely to give much trouble by their frequent occurrence.

Vanadic acid I have not had an opportunity of investigating, but it would seem that it would also be separated, in the ordinary course of the operation. If present, the larger part of it would be removed in combination with ammonia, while precipitating the silica from the nitre solution.

When *magnesia* is present, which after fusion with nitre would be left in the pure or carbonated state, the nitrate of ammonia may dissolve part of it in boiling the fluid. In most cases this does not essentially interfere, since the sulphate of magnesia is soluble; still, when a great deal of this earth is contained in the ore, it is best to add silica, as in the fusion of ores containing lead.

If the ore contained *copper*, and no silica was added, yellow flakes of chromate of copper appear when the mass is dissolved. In this case a little caustic potash is added, and the solution boiled. The yellow flakes then contract into black scales of oxide, which retain no chromic acid.

The presence of *iodine*, *bromine*, *chlorine*, or *fluorine* in the ore, would interfere, inasmuch as the platinum evaporator would be attacked by them. When the chromic *oxide* alone is present, the halogens may be expelled by a fusion of the ore with a small quantity of bisulphate of potash, previous to that with nitre, and executed in the same vessel. This cannot be done when chromic *acid* is present, since then the volatile oxychloride, fluoride, etc. would be formed. I have not devised any ready general method of separation in this case, which, however, is of rare occurrence. In the presence of lead or other reducible metals, the chromic acid cannot easily be reduced by oxalate of potash, without endangering the platinum crucible; in many cases, however, the difficulty might be overcome by the use of that salt, previous to the bisulphate and nitre.

IV. PHYSICS OF THE GLOBE.

1. THE INFLUENCE OF THE GULF STREAM UPON THE SUMMER CLIMATE OF THE ATLANTIC COAST. By DR. JAMES WYNNE, of New York.

THE object of this paper is strictly hygienic. The medical practitioner is constantly called upon to give an opinion as to the adaptitude of summer resorts for his patients in such a state of health as to require a change of air or climate. To those who resort to the seaside, as, indeed, to those who go inland, the proper or improper selection of the locality may be a matter in which life and death are held in the balance; and any light thrown upon this subject will, I am well assured, be kindly received by the medical profession, and promptly acted on by their patients.

The recent examinations conducted by the officers under the direction of the Superintendent of the Coast Survey have shown that the bottom of the Atlantic Ocean presents a range of mountains pursuing a similar course to the Appalachian chain, some distance back from the coast. These examinations have not yet been completed; but so far as they have been prosecuted, they clearly demonstrate the continuity of this chain of submarine mountains, and its general course.

The effect of this configuration of the bottom of the ocean over the temperature of the water is clear and unequivocal. The whole extent of this influence has not yet been determined, but thus far it shows that the Gulf Stream is not one uniform mass of warm water, pursuing a north-easterly direction along the coast, at a pretty uniform distance from it; but a series of bands of warm water, interspersed with colder ones. These have been laid down by Professor Bache with precision in his map delineating the distribution of temperature of the Gulf Stream. An underlying polar current of cold water, even in the more southerly explorations, is likewise clearly established. The position of this mountain chain in affecting the temperature of the air along the coast, as well as the water overlying it, has been as clearly demonstrated.

Lieut. Maury, in his pilot chart of the North Atlantic, has recorded the direction of the wind for each month in the year with great accuracy. The information contained in his chart is taken from the most authoritative sources, and, in some instances, extends back as far as 1810. This chart subdivides the ocean into squares of five degrees each of latitude and longitude, and the monthly observations within each subdivision are made to extend over this surface. For the purpose of the present inquiry, subdivisions of a single degree would have afforded greater definiteness; but the facts, deduced from the chart as it is, are of the highest value. As this inquiry is confined to the summer months, when invalids in search of health, or those who are well in pursuit of a more temperate air than is to be found in the cities, or even in rural districts, visit the sea-shore, the deductions from the chart will be confined to the months of June, July, and August, and to that portion of the Atlantic stretching along the sea-coast.

In the subdivision between lat. 30° and 35°, and lon. 70° and 75°, embracing the sea-coast from St. Augustine, in Florida, to Cape Hatteras, in North Carolina, there were made ninety-nine observations of winds in the month of June. Of these forty-one, or nearly one half, were from the south and south-west. In July, eighty-four winds were noticed, of which fifty-two came from the south and south-west. In August, one hundred and thirty-eight, of which sixty came from the south and south-west.

In the subdivision embracing lat. 35° to 40°, and in lon. 70° to 75°, extending from Cape Hatteras to Cold Spring, New Jersey, three hundred and fifty winds were observed in the month of June, of which one hundred and forty-three were from the south and south-west. In July, three hundred and ten, of which one hundred and sixteen came from the south and south-west. In August, three hundred and sixty-six, of which one hundred and twenty-three were from the south and south-west.

In the subdivision between lat. 40° and 45°, lon. 70° and 75°, embracing Long Island and the southern exposure of the New England coast. In June, two hundred and thirty-one winds were noted, of which one hundred and eight came from the south and south-west. In July, three hundred and eight winds, of which one hundred and sixty were southerly and south-westerly winds. In August, one hundred and eighty-three, of which sixty were from the south and south-west.

These are the facts. The deductions from them are important. It appears from these observations, that of the prevailing winds in the summer months, never less than one third of their number, and, in many instances, one half, come from the south and south-west. It must be remarked, that the winds noted were sailing winds with some degree of force, and not the slight ruffling wind, which, although insufficient for the purpose of rapid sailing, is yet most grateful in its effects over the health and comfort of those so circumstanced as to come within its range. The usual direction of this lighter breeze on the Atlantic in the summer months is from the south and south-west, directly over the current of warm water comprising the Gulf Stream. The winds from this quarter are, for the most part, gentle, balmy, exhilarating, and peculiarly happy in their influence upon the human body. Those from the north and east, on the contrary, are violent, raw, and depressing. While the former should be courted by the invalid, the latter should be as sedulously avoided.

It by no means follows that the wind on shore is the same as that upon the ocean. A very slight obstruction, as an intervening range of hills or indentation of the coast may leave any particular situation in calm, or subject to the influence of a less grateful wind, while the whole surface of the water is swept by a delightful air from the south or south-west. An example of this may be given in the Highlands of Navesink, situated a short distance from New York, and immediately behind the light-house on Sandy Hook point. These highlands, whose sides are covered to the water's edge by a rich growth of vegetation, and are highly picturesque in their effect, are shut off from the south-westerly ocean winds by a small promontory, the effect of which is to render the air upon the sheltered localities calm and oppressive, while on the sandy point, directly in front, and scarcely more than a stone's throw distant, it is agitated by a balmy and refreshing breeze.

The traveller over the New York and New Haven and the New Haven and New London Railways, which pursue an easterly course along Long Island Sound, cannot fail to remark the perceptible difference almost always observed in the temperature, after leaving New Haven for the East. However exalted the temperature may have been, or oppressive the condition of the atmosphere between New York and New Haven, yet he is almost certain to be met by a delightful ocean air from the south-west a few miles east of New Haven, and which accompanies him on his passage to New London. This is due to the configuration of Long Island.

Near New York, the northern shore of Long Island rises into elevations of greater or less extent, but sufficiently so, at most places, to intercept the sweep of wind from the ocean on its southern border. These elevations gradually diminish in an easterly direction, until a point is reached a few miles east of New Haven, where the whole island becomes flat and sandy, and but a few miles in width. This low plateau offers but slight interruption to the progress of the southerly ocean winds, and allows them to play over the surface of the water in the sound itself, and fan the opposite New England coast.

I do not purpose to institute a comparison into the various places of summer resort along the sea-coast, but merely to present the facts on which the advantages or disadvantages of any particular locality may be examined and decided. The greatest advantage, so far as air is concerned, is a free exposure to the south and south-west, and a corresponding protection from the north and north-east. This protection from north-east winds is of the greatest importance in diseases of debility, or where the nervous system is at fault. I have often seen the good effects of sea air of weeks rapidly dispelled by exposure to a harsh north-easter of a day or two's continuance; and I would add, that what is beneficial in a state of disease, is equally so in a state of health.

The islands off the coast of South Carolina, as well as the coast of North Carolina, Virginia, and New Jersey, have a greater or less south and south-western exposure. Within these limits are found Old Point Comfort, Cape May, and Long Branch, which have great celebrity as sea-side places, and attract large numbers of visitors. Each of these places is subject, however, to the depressing effects of north-easterly gales. The more southerly points are less affected from this cause than the more northern.

The whole stretch of Long Island on its southern side, which is at present, with but few exceptions, little better than an inhospitable sand-bar drifted up from the waves of the ocean, — that portion of the Connecticut coast to which we have alluded east of New Haven, as well as Rhode Island and a part of Massachusetts, enjoy in the highest degree the advantage of exposure to the south and south-west, and are, at the same time, best protected from the winds from the north-east. Newport, with many disadvantages, not the least of which is the compactness of its houses, and their almost total exclusion, in the more populous part of the town, from a sea-view, enjoys a world-wide reputation as a seaside residence, for which it is wholly indebted to the salubrity of its air, derived from its sheltered position on the one side, and its free exposure upon the other.

Many sheltered positions may, doubtless, be found on the coast south of Long Island, uniting many, if not all, the advantages already pointed out, which it is earnestly to be hoped will be discovered and improved. The advantage of seaside resorts, in such positions as to render them available for those whose occupations or means do not permit them to take long journeys, cannot be too highly estimated.

2. FLUCTUATIONS OF LEVEL IN THE NORTH AMERICAN LAKES.

By CHARLES WHITTLESEY, of Cleveland, Ohio.

SINCE the year 1838, I have observed the changes of level that occur in Lake Erie and Lake Superior, and have collected, so far as I know, all the observations made by others on these and other lakes.

These observations go back to the year 1810, with considerable regularity; and beyond that date to the year 1796, we have some information that may be relied upon. The results are in the course of publication by the Smithsonian Institute, and for this reason, I do not now propose to do more than call the attention of the Association to one phase of the phenomena of fluctuation.

During several years I have been enabled to make, or have been able to procure from others, daily, or in some cases tridaily, readings of the stage of the water. Of this kind, the most valuable and the most complete are those of Col. T. B. Stockton, and Geo. C. Davies, Esq., of Cleveland; John Lothrop, Civil Engineer, Buffalo; and M. P. Hatch, Esq., of Oswego, New York; Gen. Henry Whiting, A. E. Hathan, and Dr. Douglass Houghton, of Detroit; Capt. B. Stanard of Cleveland, and Mr. H. T. Spencer of Rochester, under the direction of Prof. Dewey; and also Messrs. Wm. Finney and M. B. Sherwood, under the direction of John Burt, Esq., at the Sault St. Mary's Canal, have contributed valuable registers. There are, by these numerous readings, *three* kinds of fluctuations found to exist, which are due to distinct causes.

There is, 1st, A general rise and fall extending through *long periods* of time, but without any regularity in the period; which I have styled the *secular fluctuation.*

2d. An annual rise and fall, that occur regularly within the period of *each year*, without reference to the general stage of the water, which is called the *annual fluctuation.*

3d. A local, fitful, irregular oscillation, of a few inches to a few feet, not to be predicted; its period of oscillation being from three to five minutes, and which continues from one to twenty-four hours. I have no difficulty in explaining the general rise and fall in our lakes. It occurs, no doubt, from the differences of the seasons, acting through a long period over a large area. This explanation has not been rendered certain by meteorological observations, because there are too few of them as yet made within the region of the lakes. Starting, however, without any hypothesis, we should conclude that there *should be changes* of level, unless every season was precisely alike from year to year, and from generation to generation.

The lakes are merely the reservoirs of drainage for the country, whose streams discharge therein. The surplus water passes off by the river St. Lawrence as a final outlet. The several lakes are mere ponds of water, caused by natural obstructions along the course of the stream, like the dams constructed by men on smaller streams. Because the extent of country drained is very large, and the climate various, it requires a series of years for the meteorological differences of the seasons to become equalized, and produce general results.

In wet and cold seasons, the river Mississippi is found to be higher than in cold and dry ones. The same is witnessed on the Ohio and all lesser streams; but the shorter their course and the smaller the tract of country which is drained, in just this proportion is the effect more rapid.

In the case of a river like the Ottawa or the Hudson, the rains that fall, or the snows that melt, in the region at their sources, produce high water in a few days, or, perhaps, in a few hours.

On the Mississippi, the Oronoco, and the Ganges, it is months before the inhabitants of the lower portions of these rivers perceive the rise of the water. Considering the St. Lawrence as one great stream, with branches reaching to the Mesabi range, and to the prairies of Wisconsin, the result would be still more tardy, on account of the repeated checks and delays in the flow of its waters through so many pools of still water. The course of the prevailing winds has its effect in increasing or retarding the flow of water, and the amount of vapor taken up in this lengthened passage is very variable.

That there should be found a change in the general level of the *North American lakes* is nothing mysterious, and nothing that may not be readily explained by visible causes, like the fact of change of level in lesser streams and pools of water. The greatest range of the "*secular fluctuations*," between the most extreme high and low stage, determined by the *yearly mean*, is for Lake Erie, 4 feet 5 inches. The greatest extreme in the same lake, determined by the *monthly mean*, is (6) *six feet.* The extreme temporary difference from the highest known stage, June 25, 1838, to the lowest known stage in February, 1819, is about (7) *seven feet.*

There are evidences along the shores of Lakes Huron and Michigan, that the extreme fluctuations there within the life of trees that are now living has been about (12) *twelve feet.* The observations upon the two last-named lakes are few. According to them, the greatest general difference of level is about (6) *six feet* since the year 1819. On Lake Superior, since 1845, when my observations commenced there, the extremes of fluctuation are about (3) *three feet.*

On Lake Ontario, by Mr. Hatch's tables, between 1838 and 1854, the greatest range was (4 feet 9 inches) *four feet nine inches.* As to the second class of variations, those annual changes that occur systematically within the period of each year, — they are not, in general, greater than one foot and one half.

The registers in my possession do not in any degree confirm the traditionary and popular belief of a regular rise and fall in every period of *seven years.*

From 1819 to 1838 there was a continual rise during a period of *nineteen years.*

From 1838 to 1841 a regular decline for three years.

From 1841 to 1851 it has been fluctuating up and down at short intervals. Going further back, it is reported that from 1796 to 1811, the water was low, — fourteen years. From 1811 to 1816 continually rising, — *five years,* — and from 1815 to 1819 always falling, being a period *of three* years. Throughout all these changes there happens to be no period of *seven years;* but I found that nineteen twentieths of the inhabitants of the lake coast firmly believe there is a change from high to low exactly at the expiration of that number of years.

The season or period of highest water within the year is not the same in all the lakes. In Lake Superior it occurs in the month of September or October, and the low-water in February or March. In Lake Erie, June is the high-water month, almost without exception and the months of February and March are the low ones. It is very much the same on Lake Ontario.

With all the lakes, their affluents discharge less water in the winter than in spring. For this reason they are all lower in the winter months than they are in June and July, ranging from *a foot* to a *foot and one half*, and this difference exists without reference to the general stage of the water, whether high or low.

The explanation of this for the lower lakes is, therefore, very simple; it is owing to the temporary surplus of the streams, and the diminution of evaporation. For Lake Superior, its late climate and prevailing winds must be taken into account. Low-water occurs there about the same time as it does in Lake Erie, in February or March; but the maximum height of the annual flood does not take place till September or October, instead of in June or July.

I account for this by the lateness of the thawing in spring, which is from four to six weeks behind Lake Erie; by the greater size of Lake Superior, requiring more time to fill up; by the want of large streams and of other lakes that discharge into it, and also by the prevalence of easterly winds in the spring, that keep back its waters.

The phenomena to which it is my intention to direct the attention of the Association belongs to the *third class* of fluctuations, to the irregular, temporary, and fitful oscillations which occur without any visible cause, but which have attracted the notice of all travellers, from the times of the Jesuit fathers. Although these pulsations were the first to be noticed, and although they excited the astonishment of all beholders, filling the imagination with vague terrors, and the reasoning powers with speculations, this class of movements is the last to receive examination. Dr. Jackson styled them the "Barometric wave" of Lake Superior, from an opinion that they are due to sudden variations of atmospheric pressure. I do not repeat here the various descriptions that have been given by the Jesuit missionaries, and the English and American travellers, since the country around the lake came into the possession of Great Britain and the United States. The accounts are very numerous and graphic, and are probably familiar to those who hear me. In clear, calm weather, when the surface of the water is perfectly placid, and no clouds or winds are visible in the circuit of the horizon, a succession of short swells arise upon the lake, and rapidly approach the shore. Some have represented them as of a terrific height, rolling onward with a bold and foaming crest, threatening the light canoe of the voyager with immediate destruction.

But I have never witnessed any that exceeded two feet (2) in height in stormy weather, and in calm weather not exceeding one and one-half feet (1½).

I had an excellent opportunity to study this wave, not only during ten years of exploration on the waters and shores of Lake Superior, but also during two years' residence at the mouth of Eagle River, a small creek that discharges into the Lake. The pulsations were here more marked, as they entered the still water of the creek, than they were as they broke upon the shore. Among several hundred observations upon the time of the oscillation, the period of a complete movement from one reflux to another is in average *four and one half minutes* (4½). For present purposes I do not deem it necessary to transcribe those observations, but give the general results. The vertical range of the wave in the Lake is about *four inches* (4). It always comes in from the open sea in a line *parallel* to the shore. The time between the breaking of each wave upon the beach of course corresponds to the period of the oscillations, as observed on my water-gauge, in the creek, ordinarily 4 to 4½ minutes.

The pulsations are less regular in stormy and windy weather than in still weather; and the vertical range is greater. My memoranda show, however, that they occur in all conditions of the atmosphere,—when it is calm, when the wind is off shore, or along shore, or on shore, and when it is blowing a gentle breeze, as well as in a gale amounting to a resistless tempest.

The crest line of the wave is always *parallel* to the coast line, without regard to the direction of the wind. In still weather, or when the Lake is but slightly agitated, as by a breeze off land, it is a beautiful sight to witness, over a long stretch of coast, these low, regular undulations, as they break languidly upon a sandy beach. This continues an hour, two hours, ten hours, a day, and even longer; but are seldom witnessed beyond eight or ten hours at a time.

They occur at night, as well as by day. I have endeavored to discover some connection between the period of their occurrence and the presence of the moon, but as yet without effect.

They are not confined to the upper Lakes, but are noticed equally on Lakes Erie and Ontario, and the smaller lakes of interior New York; and it is very probable that observations made at the sea-coast would detect them there. As yet it is not settled whether they occur in winter, because on the Northern lakes the shores, for several months, are skirted by ice. So far as I am informed, the idea that these undulations are due to barometrical variations in the atmosphere is purely theoretical, and not supported by any observed facts. The first recorded observations of them made in connection with barometrical readings that I know of, are those of Professor Mather, at Copper Harbor, in July, 1847. During the greater part of a day he observed the state of the water, as to flux and reflux. While the oscillations were marked and rapid, the mercurial column was continually moving in one direction: it was *falling* during all the time of his observations.

In truth, it would seem that a movement which is completed in the space of three to five minutes is too rapid to allow itself to be felt in a column of mercury, even if the oscillation is due to variation of pressure. Until near the close of my observations at Eagle River I had no barometer; but in October, 1856, was enabled to compare this movement of the water with the movement of the mercury during *three days* (3). All of this time the barometer was quite regular, and during most of it the mercury *fell gradually*. There were no sudden changes, and the readings of the instrument are very uniform for the length of time it was observed. The instrument was an Aneroid compared with a cistern barometer, and was read at the moment of the culmination of each wave, as well as at the period of its complete reflux or lowest stage. Without entertaining any fixed theory of these phenomena, I have been for several years inclined to look to electro-magnetic causes for an explanation.

I have noticed the Northern Lights in connection with the oscillations, but cannot discover any relation in the time of their occurrence. A series of observations with electrical and magnetic instruments would be necessary to discuss this theory, and as yet none have been taken. These observations should be made at the same time at different and distant points on the shore of one of the lakes. As this class of movements is more frequent and more prominent on the waters of Lake Superior than on the lower lakes, it would be preferable to take them there.

If there is a barometric wave in the atmosphere corresponding to that in the water, it should, as it seems to me, be due to agencies that are seated on the land. In cloudy, and especially in foggy, weather, the movements are most striking. The mountain ranges that approach the coast no doubt affect the electrical condition of the atmosphere. They condense its vapor, especially when winds sweep over their summits. The increase or diminution of vapor changes the quantity and the force of the electrical fluid. The presence or absence of the sun, which depends upon the degree of cloudiness, produces the same effect. We observe every day that storms arise in this way, particularly in the afternoon of hot days; the atmosphere is agitated, lightning is produced, and rains fall. I think it not unphilosophical, therefore, to look in that direction for a solution of this question. To surmise that a vibratory motion is thus given to the water, or, more probably, to the atmosphere that rests upon the water and communicates with it, which movement is always to and from the general trend of the coast. This, over local and limited spaces, would not be appreciable to the barometer, being rather an agitation within the atmospheric column than a change in its total weight.

3. Notes on the Measurement of the Base for the Primary Triangulation of the Eastern Section of the Coast of the United States, on Epping Plains, Maine. By A. D. Bache, Superintendent U. S. Coast Survey.

[Communicated, by authority of the Treasury Department, to the American Association for the Advancement of Science.]

The reconnaissance for a base of verification at the eastern extremity of the primary triangulation in section I. of the coast was commenced by Charles O. Boutelle, Esq., and Major Henry Prince, U. S. A., assistants in the Coast Survey in 1853, and continued through 1854 and 1855. The absence of long and straight sea beaches on this coast rendered it absolutely necessary to look for an interior site.

The reconnaissance resulted in the selection of Epping Plains, Washington county, Maine, as the most suitable site for the purpose, considering the character of the ground itself, and the facility of connecting the ends of the base with the primary triangulation. In this selection and the examination of the plains, these officers were much assisted by the local knowledge and the kind offices of J. A. Milliken, Esq., now of Cherryfield, to whom I beg leave here to return the thanks of the Coast Survey.

Major Prince being relieved from the survey, the final minute examination of the site, and the determination of the best line which could be obtained on the plain, devolved upon Assistant Boutelle, who was assisted at different times by Sub-assistant J. A. Sullivan, Lieutenant J. C. Clark, U. S. A., and Mr. F. P. Webber.

Epping Plains or "Barrens," as they are called, lie between the Narraguagus and Pleasant Rivers. They present a moderately rolling surface of sand, generally destitute of trees, except in the lower and swampy parts, and are traversed by sand ridges of different elevations, resembling very much the surface which the sounding line develops in such regions as the Nantucket shoals, at present below the surface of the water.

The plain is quite elevated, and falls suddenly from an irregularly curved margin by a steep slope to a lower plain or a wide valley.

Portions of the plain are strewed with boulders of various sizes, some of them containing not less than four thousand cubic feet, and of various granitic materials. Schoodiac hill was found to limit the position of the base, so that the problem became to draw the longest line through a point at the base of that hill, the ends of which would be easily visible from the secondary and primary stations.

Before the final selection of the line, a topographical survey was made, under the direction of Assistant C. O. Boutelle, by Sub-assistant J. A. Sullivan and Mr. Webber, and the profile was studied upon a sketch of the plain made by Lieutenant Clark.

In 1856, I examined the site, and took steps to obtain the necessary estimates of the cost of preparing it for measurement. The profile of the line as graded gives a good general idea of the ground, as it varied but little from the natural profile (see Sketch). The whole length of the line is about 8,716 metres, or 5.4 miles. Its general direction is E. 16° S. (*true bearing*). From the eastern end, for about four miles, the plain is quite level, rising in the first mile pretty regularly about fifteen feet, descending nearly as much in the next to rise by the same quantity in the third mile. It then runs along an elevated level for a fourth of a mile, and descends gradually to the rougher part of the base, which is included between the three and three fourths miles from the east end and the western end of the base.

This line was skilfully graded by Mr. Boutelle so as to follow the natural surface when the grade did not run above three degrees, and to give as long slopes as possible of the same grade for the convenience of measuring (see Sketch).

As it was found more economical to make the temporary embankments than to excavate, a profile giving a considerable excess of embankment was selected.

This was executed in the cheapest way which would give stability for the time during which it was required to stand. The least width was twelve feet, of which nine feet was on the south and three feet on the north side of the line to be measured. The base was very carefully aligned. High signals were placed over the termini, which are intervisible. On the Schoodiac, a signal of moderate elevation is visible from both, and the distances between this point and the termini were gradually subdivided until the smallest limit, the distance easily reached by a small transit, was obtained.

The verification of the alignment at different points of the measurement, when the seeing was good, was complete.

In all these preliminary operations Mr. Boutelle was assisted by Sub-assistant J. A. Sullivan and Mr. Webber.

His grading party consisted of the farmers and lumbermen of the district, who served with great cheerfulness and skill in the use of the heavy implements for rough grading.

One of the greatest difficulties was the removal of such boulders as were in the line, many of them being of such size as to require blasting to break them up, and some being actually removed to the required distance from the line by heavy blasts.

The signals erected at the two ends are very substantial, each forty-three feet in height to the top of the tripod, and fifty-three to the cone which surmounts them.

The base apparatus, already described before the Association, and described and figured in my report for 1854 by Lieutenant E. B. Hunt, of the Corps of Engineers, was used in this measurement, preliminary trials being made in the office to test its steadiness under the greatest inclinations to which it would be subjected, and the accuracy of the surface upon which the measuring stem traverses, and which determines the length of the apparatus. I was assisted in the measurements by Assistant G. W. Dean, Professor Fairman Rogers, who volunteered for the purpose, Sub-assistants Goodfellow, Stephen Harris, and Sullivan, and Mr. Thomas McDonnell, among whom the different operations were divided.

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istant

The usual comparisons of the apparatus with the standard six-metre bar were made before and after the measurement, to ascertain that no change had taken place in the length from damage by transportation, and to add to the results of former comparisons.

The measurement was begun at the west end of the line on Saturday, the 18th of July, but the next week proved so rainy that it was only resumed in earnest on Monday, the 27th.

The work of the first Saturday (24 tubes) was remeasured on the following Monday with precisely the same result as to length, the end of the second measurement falling exactly on the marks which had been placed as terminating the first, and which were fine dots upon the head of a copper nail placed in a stake, some eighteen inches in length, driven into the ground until its head just projected above the surface. The position of the mark was determined and verified, as all others of the sort in our measurements, by using a transit placed at right angles to the line and at a moderate distance from it.

This was on a descending slope of the strongest grade adopted, and there was a difference of temperature of some five degrees in the two measurements. On Tuesday, a length of eighteen tubes, which had been measured on Monday, was remeasured with an identical result. This was on an ascending slope.

On Monday the work was in part interrupted by the arrangements for photographing the apparatus, on Tuesday by a fog, and on Wednesday by showers in the beginning of the day; we made, however, half a mile on both days. On Wednesday began a series of four unbroken days, during the first of which we measured about seven eighths of a mile, and on the three others, a mile, or more than a mile each day, reaching the east end of the base on Monday evening. Thus, counting in the broken days, 5.4 miles were measured in eight days.

This time included the marking of five permanent points near to the ends of the successive miles, where stone posts have since been placed. The ends of the base will be marked by regular monuments. The base of the monument at the west end is cut from the ledge of rocks upon which the signal stands.

By the kindness of Professor Fairman Rogers, I have been enabled to collect approximately some of the statistics of the measurement in a tabular form (No. 1). A second table contains the comparison with the other five Coast Survey Bases which I have measured.

EPPING BASE. — TABLE I.

Whole length of Base in tubes	1453
" " " metres	8714.52
1.4250 added at East Base, making	8715.97
or 28,594 feet, or about 5.4 miles.	
Difference of level between highest and lowest points	104 ft. nearly
Mean level of base above mean tide	251 ft. or 76.5
Approx. corr. for reduction to the level of the sea	$0^m.10438$
or 4 inches, nearly.	
No. of tubes inclined	643
" " level	810
Ratio of tubes inclined to the whole number	0.442, nearly
" " level " " "	0.550
Correction for versed sine for whole base	$2^m.804$
or 9.2 feet to be substracted.	

			Ratio to whole number inclined
Maximum inclination	3° 14′		
Number of tubes inclined	3°, and over	31	0.048
" " "	2° 30′ "	234	0.364
" " "	2° "	79	0.123
" " "	1° 30′ "	120	0.186
" " "	1° "	110	0.171
" " "	0° 30′ "	21	0.032
" " "		48	0.74
		643	

Greatest day's work, 281 tubes, 1.05 miles in $11^h\ 10^m$ working time.
Averaging 1 tube in $2^m\ 27^s$.
Greatest No. in 1 hour, 37, or $1^m\ 37^s$ for each tube.

TABLE II. — COMPARATIVE TABLE OF THE MEASUREMENTS OF SIX UNITED STATES COAST SURVEY BASES.

	Dauphine Island.	Bodies Island.	Edisto Island.	Key Biscayne.	Cape Sable.	Epping Plains.
Whole No. of Tubes measured	1777	1807	1787	965	1072	1453
" " days employed	17	10	13	9	8	8
" " hours "	$143^h\ 17^m$	$81^h\ 08^m$	$97^h\ 28^m$	$66^h\ 31^m$	$46^h\ 26^m$	$69^h\ 43^m$
" " tubes level	961	1496	862	473	994	810
" " " inclined	816	311	925	492	78	643
Average length of working day	$8^h\ 25^m\ 7^s$	$8^h\ .07$	$7^h\ 30^m$	$7^h\ 23^m$	$5^h\ 48^m\ 18^s$	$10^h\ 07^m$
" time of one tube	$5^m\ 32^s$	$2^m\ 54^s$	$3^m\ 22^s$	$4^m\ 20^s$	$2^m\ 51^s$	$2^m\ 58^s$
" No. of tubes per day	104.5	180.7	137.5	107.2	134	181.6
" " " " of 9 hours	108.0	197.9	165.9	130.0	200.0	187.2
" " " per hour	11.85	21.98	18.40	14.40	22.47	20.8
" plus inclination	17′.6	16′.2	24′.5	31′.0	12′.0	1° 53′
" minus "	16′.6	19′.6	23′.0	26′.0	10′.0	1° 54′
" of greatest plus inclination	40′.8	23′.7	55′.4	58′.0	14′.0	2° 52′
" " minus "	42′.6	29′.5	48′.4	54′.0	11′.0	2° 46′
" temperature Fahr.	84° 50	52° 01	59° 50	82° 90	87° 90	70° 0

The photographs of the apparatus and operations which I submit to the Association were taken by Mr. Black, of the firm of Whipple & Black of Boston, who exerted himself especially in the matter, and succeeded, under many disadvantages from variable weather and the roughness of field arrangements for photography, in making satisfactory representations.

The views of the apparatus and operations, include the placing the apparatus over a mark, the aligning, the setting of the trestles in advance of the measurement, the transfer of the measuring tube, the making of contact. The comparing apparatus and tent are also shown.

The sketch shows the topographical features of the ground, and gives the profile of the base as graded for measurement.

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4. Notice of the Determination of the Longitude of Fernandina, Amelia Island, Florida, by means of Chronometer Exchanges from Savannah, Georgia. By A. D. Bache, Superintendent, and Chas. A. Schott, Assistant, U. S. Coast Survey.

[Communicated by authority of the Treasury Department.]

It is proposed to connect the triangulations of the Atlantic coast and of the Gulf of Mexico by a series of triangles across the peninsula from near Fernandina to near Cedar Keys, the termini of the Air Line Railroad. The importance of this connection will be obvious when the distance around the peninsula, and the nature of the triangulation which will envelop it, are considered. A reconnaissance had shown such a triangulation to be practicable, and it was desirable to keep the general direction of the line already referred to. The latitude of the two termini had been observed, and the longitude of a point on one of the Cedar Keys.

The determination of the longitude of Fernandina would furnish the remaining element needed, and also an important datum for the geodetic work of the coast of Georgia and Florida, already in progress. The longitude of Savannah had been determined by telegraph, and the connection of Fernandina with it was easily made by the transportation of chronometers in the steamers running twice a week between Savannah and the St. John's, and stopping at Fernandina.

It is proposed to give an account of the operations of this expedition, with the results for personal equation, for the performance of the chronometers under different circumstances, and others, and of the final determination of the difference of longitude.

The incidental results will probably have more interest for the American Association than the mere longitude question; though as the two best maps differ some nine miles in the longitude of Fernandina, the final result must, in a practical point of view, be esteemed of great importance.

Early in the spring of the present year, arrangements were made for executing this work by a party under the immediate direction of the Superintendent of the Coast Survey. The old astronomical station near the Exchange, in the city of Savannah, was rearranged and furnished with suitable instruments. This station is on the bank of the Savannah River, and forty-four feet above mean low water, commanding a good view of the heavens both to the south and the north. It has one drawback, namely, that the stone supporting the transit instrument rests upon an arch, and that the vibration from heavy vehicles passing in the street is communicated to the instrument, affecting in a degree the accordance of individual results, but not sensibly the final determination of time.

The outfit of the station consisted of transit instrument No. 3, by Troughton and Simms, with a focal length of forty inches and a magnifying power of seventy, of an observing chronometer (sidereal), No. 1707, by John Fletcher, and of the usual meteorological instruments. The observations generally were made at this station by the Superintendent, replaced for a few days, in the early part of the work, by Assistants Charles A. Schott and Charles O. Boutelle. Mr. J. H. Toomer was the aid in the party. The transit was mounted on the 25th of March, and the observations were closed on the 30th of April.

At the other end of the line, a smaller transit, by Würdemann, C. S. No. 10, was mounted at the station near the railroad wharf, occupied the year previous by Sub-assistant Edward Goodfellow for the determination of the latitude of Fernandina. The observations were made by Assistant Charles A. Schott and Mr. J. E. Blankenship. The transit instrument used has a focal length of twenty-six inches, and a magnifying power of nearly thirty-five. The observing chronometer was by J. Hutton, No. 311 (sidereal). The same meteorological instruments were observed as at Savannah, daily at 6 A. M. and at 2 and 10 P. M. The barometers and thermometers used were afterwards compared for index error. This station is but a few feet above the level of the tide of Amelia River, and the wooden blocks upon which the transit was placed actually reached that level, which caused an instability in azimuth, requiring a determination of that element by suitable stars every observing night. The observations here were begun on the 1st of April, and continued until the 27th of the same month.

Observations for personal equation in observing transits were made at Savannah at the beginning of the work and near its close, and at Fernandina at the close. No difference appeared in the values found by the two series at Savannah, nor by the use of the two different transit instruments.

Ten chronometers were employed, divided into two sets, and packed in two of the boxes used in the chronometer expedition of 1855, the sides and top and bottom of which were thickly padded to prevent abrupt changes of temperature and the effects of jarring. Eight of these chronometers were regulated to solar time, the other two as comparing chronometers, so as to lose about four minutes in mean time in twenty-four hours. With set No. 1 was the thermometric chronometer of Mr. Bond, without compensation, and indicating the mean temperature of its exposure by its gain or loss. Each boat of the line between Savannah and Fernandina made usually one trip to and fro within a week, the days of departure and arrival being so arranged as to require that the same set of chronometers should go and return in the same boat, the first boat from Savannah in the week not returning in time to permit the rating of the chronometers for the trip of the second. Mr. D. Hinkle, Aid, was charged with the exchange of the boxes, and accompanied one of the sets going and returning. The other set was placed in charge of the captain of the boat, and both were always kept in the captain's office. Twice the observers, when changing pieces, accompanied the instruments. They were transported by hand between the wharf and the stations. This precaution secured general uniformity of rate. Some of the chronometers were supplied with a thermom-

U.S. COAST SURVEY

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Diagrams illustrative of the determination eter, which was read at each comparison. The average duration of the trip was twenty-nine hours from comparison to comparison, these being made as near as possible to the time of arrival and departure.

Diagram No. 1 shows at a glance the number of trips made, and the dates of departure and arrival of each set of chronometers at the two stations. One of the chronometers of the set No. 2 being required for other use was withdrawn, and another instrument, kindly loaned by Mr. Richmond, of Wilmot & Co., of Savannah, substituted for it about the middle of the operations, causing no inconvenience from the change.

The weather was generally favorable, the clear sky permitting regular observations, and only one very abrupt change of temperature occurring during the series. As far as practicable the same stars were used at both stations. The routine of observations was as follows: 1st. The level was read; 2d. A low star was observed for azimuth; 3d. Two zenith stars for time; 4th. The level was again read. Next the instrument was reversed, the level read, two stars near the zenith, and one low star observed, and the level again read. Occasionally special observations for collimation were made by reversals on Polaris on the middle wire. The Savannah transit had seven wires, and the Fernandina nine. The probable error of time for the transit over all the wires of each transit was about the same; namely, $\pm 0^s.11$. This includes any error of right ascension. The places of the stars were taken from the Greenwich Nautical Almanac. The equatorial intervals of the wires of the two transits were known to within a probable error of $\pm 0^s.05$ and $\pm 0^s.04$ respectively.

The value of one division of the level of transit No. 3 was found to be $1''.05 \pm 0''.02$, and of No. 10, $0''.73 \pm 0''.02$. Observations made for the purpose show that the pivots of the first-named instrument are sensibly equal, and gave for the second a correction which, at the greatest, is $0^s.02$, the clamp pivot being the larger.

Fifteen hundred and twelve transits were observed at Savannah, and twelve hundred and sixty-nine at Fernandina. They were all reduced by the method of least squares.

In observing, time was marked for the superintendent by Mr. Toomer. Mr. Schott marked time for himself. The records were kept by Mr. Toomer and Mr. Blankenship.

Personal Equation. — Observations on alternate wires for personal equation between the superintendent and assistant Boutelle showed no sensible difference, time being marked for both by Mr. Toomer. Ten stars, observed on April 4th, gave for personal equation —$0^{s}.01$, ± $0^{s}.02$; and seven stars on the 6th, + $0^{s}.05$, ± $0^{s}.02$. The following are the results for personal equation between the superintendent and assistant Schott: —

Locality.	Date.	Transit No.	Magnifying Power.	No. of Stars.	B S.	Prob. Error.
	1857.					
Savannah.	March 30.	3	70	23	— $0^{s}.62$	± $0^{s}.03$
"	April 22.	3	70	10	— 0 .64	0 .04
"	" 23.	3	70	12	— 0 .50	0 .03
Fernandina.	" 26.	10	35	12	— 0 .73	0 .10

The result is B–S = — $0^{s}.58$, ± $0^{s}.04$, which has been applied when necessary. The separate results are remarkably accordant when the variety of circumstances is considered. The first were obtained when the first observer had the day before arrived from a fatiguing journey; and the second had had several days of practice. In the second and third both observers were in full practice, the second having had but a brief interval from work for the short trip from Fernandina, by water. The practice had been, it is true, with the smaller instrument at Fernandina. In the last series, both were fresh from the short trip to Fernandina; the first had been in full practice with the large instrument, but had not used the smaller one. The probable error of one determination varies under the different circumstances, within the limits of ± $0^{s}.10$ and ± $0^{s}.14$, under the circumstances already stated, while the average is still preserved. In a few cases, the first observer took the stars' passage over the first four; and the last, the second three wires of the transit.

The results for personal equation were grouped for stars observed; for illumination east, and then west; for the two observers leading respectively; for stars north and south of the zenith; and for low and high stars, without sensible deviations from the mean value.

To determine how much of this large personal equation was due to the difference between the marking of time by Mr. Schott and Mr. Toomer, and how much to the difference of observation of the stars' passage over the wires, Mr. Toomer was directed, on the 26th of April, to mark time for both observers. Eleven stars were thus observed, with a personal equation of B–S $= -0^s.30, \pm 0^s.03$; thus, $0^s.28$, or nearly one half of the personal equation was due to the difference in marking time or ear perception; and the other half to the difference in the observation of the transit or the eye perception. This supposes the tapping to be simultaneous with the perception of the transit; and, on the average, this is probably really so.

Diagram No. 2 shows the separate values found for personal equation, and presents some results worthy, when time and opportunity serve, to be further pursued. The steadiness to the mean, with large accidental fluctuations on the first night, and the running up on the nights of the 23d and 24th, towards the close, and down on the 26th, are very curious.

Temperature Compensation.—The compensation of chronometers is never perfect for any considerable range of temperature. Prof. W. C. Bond pointed out, in the Coast Survey Chronometer Expedition between Cambridge and Liverpool, the consequences of this, and elaborately examined the rates at the different temperatures to which the chronometers were likely to be subjected in the different voyages. Diagrams Nos. 3 and 4 show conclusively the over-compensation of the observing chronometers used at the two stations, the curves of rate and of temperature corresponding remarkably.

Twenty-four conditional equations between the observed rate and temperature were found for Savannah, and seventeen for Fernandina; and the solution of the normal equations gave the correction to be applied to the rate of each chronometer for temperature. The results were thus reduced to a mean temperature, and the remaining irregularities were equalized graphically. These corrections were applied during the interval between the time of observations of transits and of the comparison of the chronometers, in determining the error of the chronometer or sidereal time for the epoch of the comparison.

Chronometer Comparisons.—Each set of chronometer comparisons consists of a double operation, the repetition being considered necessary to insure correctness. The chronometers beating half seconds, there is a coincidence of beat between the comparing chronometer and a mean time chronometer every minute, and between the comparing and sidereal chronometer every minute and a half. The comparing chronometer was observed in turn in a regular order with each of the others, passing from one end of the box containing them to the other, and then back again to the first. To the mean of the observed times of coincidence of beats, a small correction is applied for the gain of the comparing chronometer on mean or on sidereal time, giving the chronometer time at the common epoch of comparison.

To these times the corrections already referred to were applied.

Stationary and Travelling Rates.—The next step is the determination of the stationary and travelling rates of the chronometers. The stationary rates resulted directly from the observed change (corrected), and the interval between the time of arrival and departure at each station. In making these reductions it was found most convenient to regard the chronometers as sidereal ones, with a large rate. Stationary rates were reduced for eight intervals, four at each station, for each chronometer.

The travelling rates were obtained by deducting from the whole interval between the departure from, and return to, the same station, the shorter stationary interval at the other station, to obtain the sidereal travelling interval, and from the whole change of chronometer time, the change during the stationary interval, to obtain the travelling change.

These travelling rates were ascertained for the trips from Savannah and back again, as well as from Fernandina and back, and by using these, approximate values for the difference of longitude were obtained.

The error arising from the assumption of equal travelling rates for the outward and home trip of each journey, implied in this process for deducing the travelling rates, is partially corrected by taking the mean of the longitudes from and to Savannah, and from and to Fernandina.

Taking the mean of seven values for travelling rates of each chronometer, the stationary and travelling rates compare as follows:—

Set No. 1.			Set No. 2.		
Chron's.	Mean Stationary Rate.	Mean Travelling Rate.	Chron's.	Mean Stationary Rate.	Mean Travelling Rate.
	s.	s.		s.	s.
1,507	— 484.75	— 484.12	1,508	— 485.42	— 485.60
175	+ 232.07	+ 232.75	184	+ 230.82	+ 229.92
177	+ 236.48	+ 237.09	193	+ 236.40	+ 235.53
191	+ 236.55	+ 236.60	{ 195	+ 235.86	+ 236.84
301	+ 231.07	+ 231.19	{ 442	+ 240.44	+ 239.67
			1,285	+ 235.06	+ 234.78

This table shows that travelling affected the two sets differently, all the five chronometers of the first set, and only two of the second set, lost by travelling. The separate comparisons of the travelling rates, in the different trips, agreed well among themselves.

Having the travelling rates deduced from the voyages or trips from and back, the travelling rate for each trip was deduced by the application of the method of least squares. The following table gives a specimen of the result for one of the chronometers of the second set (No. 1,285).

Travelling Rate of Chronometer.				Duration of Trip.	
From Voyage to and fro.		From Single Trip.			
From Sav. and back.	From Fern. and back.	From Sav. to Fern.	From Fern. to Sav.	Savannah to Fern.	Fernandina to Sav.
s.	s.	s.	s.	d.	d.
235.31		235.25	235.38	1.092	1.073
	235.50	235.64		1.076	
235.02			234.50		1.202
	234.61	234.72		1.322	
234.60			234.49		1.361
	234.17	233.81		1.265	
234.24			234.76		0.995

Comparing the travelling rates of the separate trips with the average travelling rate before found for the voyage or trip to and fro, the differences will be seen to be quite small, showing the satisfactory character of the first hypothesis from which the approximate longitude was deduced.

Having thus the individual travelling rates for the several chronometers of each set, a difference of longitude was deduced by using it in combination with the stationary rate.

The following table gives the separate longitudes deduced from each chronometer.

Set No. 1.			Set No. 2.		
Chron.	Diff. Long.	Prob. Error of each Chron.	Chron.	Diff. Long.	Prob. Error of each Chron.
	m. s.	s.		s.	s.
1,507	+1 29.47	±0.11	1,508	+29.78	±0.09
175	29.19	0.30	184	30.40	0.15
177	29.59	0.10	193	29.45	0.20
191	29.57	0.07	195	30.29	0.29
301	29.79	0.05	442	29.93	0.10
			1,285	29.94	0.09

m. s. s.
Mean +1 29.73 ± 0.07.

This table shows such a decided difference between the longitudes given by the two sets of chronometers, or what may be called a personal equation between the two sets even after the application of corrections for the temperature deduced for each instrument, that it was deemed advisable to try whether if the two sets were exposed to identically the same circumstances, this difference would remain. They were, therefore, carried side by side from Savannah to Fernandina and back, and carefully rated at both places. The resulting longitude

m. s.
for set No. 1 was 1 28.6,
and for set No. 2 was 1 29.3,

agreeing with the former results.

If, in accordance with the method of reduction of these observations we allow weights according to the inverse squares of the errors, allowing also for the difference in duration of the trips, the several chronometers would show very different values for use on such an expedition.

Set No. 1.			Set No. 2.		
Chron's.	Prob. Error.	Weights.	Chron's.	Prob. Error.	Weights.
	s			s.	
1,507	±0.11	76	1,508	±0.09	114
175	0.30	11	184	0.15	45
177	0.10	106	193	0.20	25
191	0.07	190	195	0.29	12
301	0.05	359	442	0.10	94
			1,285	0.09	129

$\Sigma = 1161.$

Using these weights, we deduce for the final difference of longitude of Savannah and Fernandina

$$1^{m}\ 29^{s}.76,$$

with a probable error of $\pm\ 0^{s}.06$.

The difference between this result and the contradictory ones of two of the best charts of Fernandina, has already been stated in our prefatory remarks.

5. On the Heights of the Tides of the Atlantic Coast of the United States, from Observations in the Coast Survey. By A. D. Bache, Superintendent.

[Communicated by authority of the Treasury Department to the American Association for the Advancement of Science.]

It is well known, that when a bay or indentation of the coast presents its opening favorably to the tide wave, and decreases in width from the entrance towards its head, the tides rise higher and higher from the mouth upwards. The Rev. Mr. Whewell has stated that in a general way the same fact is deduced from the observations on the coast of Great Britain and Ireland discussed by him.

The Coast Survey observations of the tides of the Atlantic Coast, the results of which, from time to time, I have brought before the Association, furnish the means of a complete discussion of heights as well as of times, and very simple generalizations result from their examination. Through the kindness of Capt. Shortland, R. N., and of

Admiral Bayfield, R. N., I have been enabled to extend these results to the coast of New Brunswick, Nova Scotia, and to part of Newfoundland.

I beg leave to make my best acknowledgments to these distinguished hydrographers for the prompt and liberal communication of the results of their observations.

The Coast Survey Observations have been worked up in the tidal division under the direction of Assistant L. F. Pourtales, and I am indebted to him for giving the results the shape desired, and for the diagrams representing them.

The following table of stations on or near the exterior coast line of the United States is taken from the more extended tables of the Coast Survey, omitting stations which are up rivers or bays, except in special cases, the object of inserting which will be obvious.

Table A contains a number for reference, the locality of the tidal station, the State to which it belongs, the latitude, longitude, and the mean height of the tide in feet and tenths.

TABLE A. — HEIGHTS OF TIDES ON THE ATLANTIC COAST OF THE UNITED STATES.

No.	Locality.	State.	Latitude.		Longitude.		Heights in feet.
			°	′	°	′	
1	Portland,	Me.	43	39	70	14	8.8
2	Portsmouth,	N. H.	43	04	70	42	8.6
3	Newburyport,	Mass.	42	48	70	52	7.8
4	Gloucester,	"	42	37	70	40	8.9
5	Salem,	"	42	31	70	54	9.2
6	Boston,	"	42	22	71	03	10.0
7	Plymouth,	"	41	57	70	40	10.1
8	Provincetown,	"	42	03	70	11	9.2*
9	George's Shoals,	"	41	40	67	45	7.0†
10	Monomoy,	"	41	33	69	59	3.8
11	Siasconsett,	"	41	15	70	00	2.2
12	Weweeder,	"	41	15	70	05	1.2
13	Smith's Point,	"	41	17	70	16	2.1
14	Wasque,	"	41	21	70	30	1.7
15	Menemsha,	"	41	20	70	45	2.7
16	Point Judith,	R. I.	41	22	71	29	3.1
17	Newport,	"	41	29	71	20	3.9
18	Block Island,	"	41	10	71	34	2.8
19	Montauk Point,	N. Y.	41	04	71	51	1.9
20	Stonington,	Conn.	41	20	71	54	2.3
21	New Haven,	"	41	18	72	54	5.8
22	Fire Island,	N. Y.	40	38	73	13	2.1
23	Sand's Point,	"	40	52	73	43	7.7
24	Sandy Hook,	"	40	28	74	00	4.8
25	Cold Spring Inlet,	N. J.	38	57	74	45	4.4
26	Cape May,	"	38	56	74	57	4.8
27	Old Point Comfort,	Va.	37	00	76	18	2.5
28	Hatteras Inlet,	N. C.	35	12	75	43	2.0
29	Beaufort,	"	34	42	76	40	2.8
30	Cape Fear,	"	33	52	78	00	4.4
31	Winyah Bay,	S. C.	33	14	79	8	3.8
32	Charleston,	"	32	46	79	54	5.3
33	North Edisto River,	"	32	33	80	13	5.8
34	Port Royal,	"	32	17	80	40	7.0
35	Savannah Entrance,	Geo.	32	02	80	53	7.0
36	Sapelo,	"	31	21	81	24	6.6
37	St. Simons,	"	31	8	81	36	6.8
38	St. Mary's River,	"	30	42	81	36	5.9
39	St. John's River,	Fla.	30	20	81	33	4.6
40	St. Augustine,	"	29	52	81	25	4.2
41	Indian River Inlet,	"	27	28	80	19	2.5
42	Cape Florida,	"	25	40	80	09	1.5

* Major Graham, U. S. A. † Capt. Wilkes, U. S. N.

The following table of tides of localities on the coast of Cape Breton, Nova Scotia, and New Brunswick, is from the observations of

Admiral Bayfield and Captain Shortland. The authorities are given in the column of remarks, which also contains the remarks of Admiral Bayfield on the tidal results communicated by him. I have taken from his table the heights which were derived from the greatest number of observations. The column of means is the average of the heights of spring and neap tides in feet and tenths. The localities are arranged from the north, southward on the outer coast, and in the Bay of Fundy from the entrance up the bay.

From the table of Captain Shortland I have selected only a few localities as specimens, having no wish to anticipate through his generosity the use which he will doubtless make of his own results.

TABLE B.—HEIGHTS OF TIDES ON THE COAST OF CAPE BRETON, NOVA SCOTIA, AND NEW BRUNSWICK.

No.	Localities.	Remarks on Localities.	Latitude.		Long.		Rise of Tide.					Remarks and Authorities.
							Ord. Spring.		Ord. Neap.		Mean.	
			°	′	°	′	ft.	in.	ft.	in.		
	Island of Cape Breton.											Admiral Bayfield.
1	St. Ann's Harbor.	Entrance.	46	17	60	33	5	0	3	3	4.1	A complete semi-lunation observed.
2	Sydney Harbor.	S. E. Bar.	46	12	60	13	3	9	2	4	3.1	At full moon, and a day or two before and after.
3	Menadon Harbor.	Near Scataria Island.	46	00	59	50	5	6	3	4	4.4	Good. A complete semi-lunation observed. [bef. & aft. each.
4	St. Peter's Island.		45	36	60	49	6	0	4	0	5.0	At new moon, and a day or two bef. and after. [with sev. days
5	St. Peter's Bay.	Haulover, at head of Bay.	45	39	60	52	5	9	4	1	4.9	Good obs. 4 times observed, twice at full & twice at new moon,
6	Grandigue.	In Lennox Passage.	45	36	61	01	6	4	4	6	5.4	Good. A complete semi-lunation observed.
7	Arichat Harbor.	Jerseyman Island, N. Point.	45	30	61	03	5	0	4	0	4.5	Ditto. Ditto. Ditto. Ext. tides rise 6 ft.
	Nova Scotia.											
8	Canso Harbor.	E. end of Cutler's Island.	45	21	60	59	6	6	4	6	5.5	A complete semi-lunation observed, but tides very irregular.
9	White Haven.	Marshall Cove.	45	15	61	11	6	1	4	1	5.1	A complete semi-lunation. Good observations.
10	Harbor Island.	N. E. Point.	45	08	61	36	6	6	4	6	5.5	A complete semi-lunation observed. Extraord. tides rise 7 ft.
11	Liscomb Harbor.	Pye's Wharf.	45	00	62	01	6	0	4	0	5.0	3 times obs.: at full and new moon, and sev. days bef. & aft.
12	Sheet Harbor.	Watering Cove.	44	54	62	30	6	8	4	6	5.6	Good. Two complete semi-lunations observed.
13	Pope Harbor.	Harbor Island, N. E. Point.	44	48	62	39	6	6	4	2	5.3	3 times obs.: at full and new moon. [extr. neaps, only 4 ft.
14	Ship Harbor.	Salmon Point.	44	47	62	49	6	5	4	10	5.6	Good. A compl. semi-lunation. Extr. spr. tides rise 7 ft., and
15	Iedore Harbor.	Marsh Point.	44	43	63	00	6	6	4	8	5.6	Two good and complete semi-lunations observed.
16	Halifax Harbor.	Naval Yard.	44	40	63	35	6	0	4	6	5.2	Mean of a complete year's observations with a tide-gauge.
	Bay of Fundy.											Captain Shortland, R. N.
17	Cape Sable.	Cape Sable Isl., Clarke's Har.	43	25	65	39	11	6	4	11	8.2	
18	Ellenwood's Island.	Bird Rock.	43	39	66	04	12	7	7	0	9.7	
19	Yarmouth Harbor.	Fourchue Isl. Light-House.	43	47	66	10	10	9	16	0	13.3	
20	Bryer's Island.	Peter's Island Light-House.	44	15	66	21	20	6	9	3	14.8	
21	Campbell's Island.	Owen's House.	44	54	66	58	25	0	11	0	18.0	
22	St. John, N. B.	Battery Point Rock.	45	16	66	04	26	8	12	0	19.3	
23	Shadwood Point.	Cumberland Basin.	45	54	64	22	50	0	22	0	36.0	

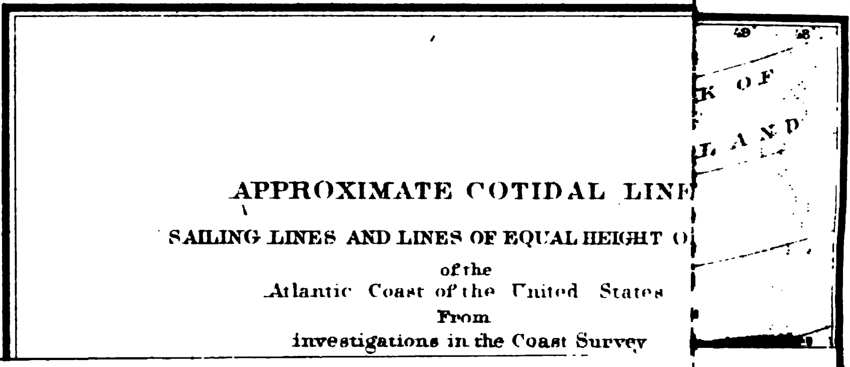
These numbers may be extended beyond the turn of Cape Race, where the coast trends to the west of north, by further results of Admiral Bayfield, though the remarks which he makes show them to be only approximate. Thus two stations on the coast of Labrador, St. Lewis Bay, in latitude 52° 19′ and longitude 55° 37′, and Henley Island, in latitude 52° 00′ and longitude 55° 53′, give each for the mean of the height of spring and neap tides, 2.3 feet. St. Johns, Newfoundland, gives 5.0 ft. Trepassey Harbor, south of it, 5.8 ft.

Beginning with the southern end of Table A, and following the results northward and eastward, we find from Cape Florida to Savannah and Port Royal a gradual increase of the tides, and then a gradual decrease to Cape Hatteras, with a single contradiction easily explained. Next following the stations on the coast, and omitting those in the bays and sounds, we have a less regular increase to Sandy Hook, and a decrease to Weweeder on Nantucket Island. Next is a less regular regimen, requiring a more detailed examination.

By developing the curved line of the coast into a straight line, and marking upon it the tide stations, which will be thus at nearly their proper distances from each other, and by erecting ordinates at each of the station points, and setting off on a suitable vertical scale the heights of the tides at those points, and connecting the extremities of the several ordinates, we have the broken line shown in diagram A. In drawing this line, the stations of the coast only are joined, and the irregularities are cut off by the curve.

This curve shows distinctly the *physical* division of the coast between Cape Florida and Cape Sable into three great bays. The great southern, from Cape Florida to Cape Hatteras. The great middle, from Cape Hatteras to Siasconsett. The great eastern, from Siasconsett to Cape Sable. Perhaps this latter may be considered as only a portion of a great bay from Siasconsett to Cape Race; but this generalization is at present hardly safe, and I confine myself, therefore, to the more limited view. The tide wave setting into the southern bay rises as the bay contracts; and the heights of the tides along the shores increase as the places are more distant from the chord spanning the entrance.

If we suppose the lines of equal height to be straight lines, and draw them upon the diagram, transferring them to a map of the coast, we shall find that they are more crowded on the more curved side, and



APPROXIMATE COTIDAL LIN

SAILING LINES AND LINES OF EQUAL HEIGHT O

of the
Atlantic Coast of the United States
From
investigations in the Coast Survey

more open on the less curved. The curve indicates Cape Hatteras, and not the inlet which was the tidal station, as the point of least height. The physical cause of this phenomenon is well understood, if it has not yet been reduced to measure.

The next curve shows us plainly the middle bay, having Hatteras for its south-western cape, and Smith's Point, or Weeweeder, for its north-eastern entrance.

The form of the shore is less favorable to regularity, but the result is nevertheless well marked. The interference of tidal waves which takes place off Nantucket tends also in a degree to confuse the results.

The chart shows how simple the system of cotidal lines is in the three bays, running nearly parallel to the shores.

The eastern bay lies between the eastern part of Nantucket (Siasconsett) and Cape Sable, Massachusetts Bay being subsidiary to this.

The tide wave entering the eastern bay follows the deep water, and thus the cotidal lines take generally the directions of the shores, until the tide wave enters the Bay of Fundy. The most probable form of the cotidal lines from XI. to XV. hours inclusive, is shown upon the chart, which is merely an extension of the chart of cotidal lines of the United States coast formerly presented to the Association. The heights increase rapidly from Nantucket to Cape Cod, being 2.2 feet at Siasconsett, and 9.2 feet at Provincetown. At Cape Ann they are nearly of this same height, and increase in passing up and into the bay to 10.0 feet at Boston, and 10.1 feet at Plymouth.

The height at Newburyport is probably local, depending upon the position of the tide gauge. There is but little change from Portsmouth to Portland, and from Cape Sable to Ellenwood's Island. Soon after passing Mt. Desert on the west side, and Ellenwood's Island on the east side, the tide wave has turned into the Bay of Fundy, and the rise increases with extraordinary rapidity.

Shall we look to the greater bay between the Nantucket and Newfoundland Shoals for the cause of the 8 feet rise at Cape Sable, and of the heights from Admiral Bayfield's table? We find the heights along the coast of Nova Scotia to vary from seven to six feet, not with regularity, however. At Cape Breton Island they vary from 6.4 to 4.6 feet, decreasing thus in going northward and eastward. Are these heights due to the crowding of the waters into this greater bay? If so, why are not the heights of Cape Breton greater than those of Nova

Scotia? We require results on the south shores of Newfoundland, and on the Great Bank, to give us clear ideas on these points, and I hesitate to extend the generalization to this tempting field.

The Shoals from Nantucket and broken ground near George's Bank, and the comparatively shoal water in their vicinity on the one side, and the Great Bank of Newfoundland on the other, look as if full of meaning of this sort. Further results may, however, show that this is not the interpretation of the phenomena. The tides of Labrador are but 2.3 feet, bringing us back to the standard of Hatteras, and of Montauk Point, and what probably would be that of Nantucket, but for interferences.

The complicated character of the cotidal lines in this immediate vicinity is indicated by the chart, the lines from XII. to XV. hours being crowded into the very small space of a few miles on the south side of Nantucket.

To return to the more limited scale, within which our inductions are safe, Delaware Bay, New York Bay, Long Island Sound, Narragansett and Buzzard's Bays, Nantucket and the Vineyard Sounds, present, on a smaller scale, the same phenomena of increase in the height of the tide in ascending. On the contrary, in Chesapeake Bay, which widens and changes direction at a right angle immediately from the entrance, the tides diminish in height, as a general rule, in going up the bay.

The results of the heights of tides along the coast are very satisfactorily shown, upon a model which is now before the Association, for superintending the execution of which I am indebted to Mr. Pourtales. The basis is a map of the Atlantic coast from Cape Florida to Cape Race, upon which the cotidal lines of the United States are traced. The tidal stations are marked upon this, and rods cut to length, and proportionate to the rise and fall of the tides at the several stations, are inserted in holes drilled at the station points. The steel rods refer to the heights at exterior stations, and the brass rods to interior ones. Paper cut to the form of the general curve of heights, which has already been explained, and placed behind these rods, serves to show the generalizations with great distinctness.

I propose to call the bay between Cape Florida and Cape Hatteras, the Southern Bay; that between Cape Hatteras and Nantucket, the

Middle Bay; and that between Nantucket and Cape Sable, the Eastern Bay, of the coast of the United States.

The general figure of the coast line has of course heretofore attracted the attention of geographers. The connection with the heights of the tides could only satisfactorily be made out by such a series of tidal observations as those embraced in the Coast Survey.

V. METEOROLOGY.

1. ON THE WINDS OF THE WESTERN COAST OF THE UNITED STATES, FROM OBSERVATIONS IN CONNECTION WITH THE U. S. COAST SURVEY. By. A. D. BACHE, Superintendent.

[Communicated by authority of the Treasury Department to the American Association for the Advancement of Science.]

THE observations, of which I propose at present to communicate the results, were made in the year 1855, in connection with the tidal observations on the Pacific coast, at three permanent stations — Astoria, San Francisco, and San Diego.

The approximate latitude and longitude of each of the stations are as follows: Astoria, Oregon, lat. 46° 11′ N., long. 123° 49′ W.; San Francisco, California, lat. 37° 48′ N., long. 122° 28′ W.; San Diego, California, lat. 32° 40′ N., long. 117° 12′ W.

The mode of observing was that described in my paper on the winds at Cat Island, read before the Association in 1850. The observers were posted and practised together by Lieutenant W. P. Trowbridge, of the U. S. Corps of Engineers, under whose supervision the observations were made.

The directions of the winds were noted in points, and the force in the conventional scale before referred to. These numbers were reduced to velocity in miles per hour, by the tables given in my former paper, and the quantity of wind blowing from any quarter during a given period was thence readily found.

The tables and diagrams are thus of the same kind as those which I have before presented to the Association. They were made under the direction of Assistant L. F. Pourtales, of the U. S. Coast Survey, to whose care, assiduity, and knowledge I am indebted for the opportunity of presenting them. The computations and diagrams were made by Miss Mary Thomas.

The observations were taken three times each day, at 6 A. M. and P. M., and at noon, except on Monday of each week, when hourly observations took the place of the regular daily ones. From these latter results the reference of the three daily observations to the mean of the day has been made.

The quantities of wind for each hour and for each direction were computed and grouped by months, and then plotted. The eye readily takes in the characteristics of the winds at different periods of the day and year, and for the various directions. To apply these to the reduction of the daily observations, tables were formed of the average time during which each wind blowing would give from observations at the three hours already named the result for the day. For example, the west wind at San Francisco gave for the quantity in twenty-four hours by the daily observations, 505. The mean hourly quantity at 6 A. M. being six, at 12 M., twenty-seven, and at 6 P. M., thirty-one. These quantities respectively, being supposed continued for nine hours, five hours, and ten hours, which agrees with the diagram, would give 499, a number differing but little from the total found for the day. In this way the following table was formed, which was applied to the reductions of the daily observations.

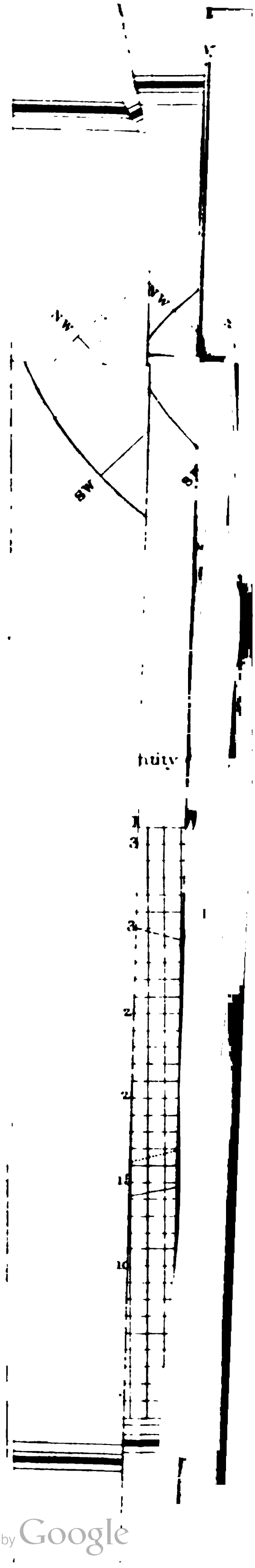
TABLE FOR DEDUCING FROM THE THREE DAILY OBSERVATIONS THE MEAN OF THE DAY.

ASTORIA.				SAN FRANCISCO.				SAN DIEGO.			
Wind.	6 A.M.	12 M.	6 P.M.	Wind.	6 A.M.	12 M.	6 P.M.	Wind.	6 A.M.	12 M.	6 P.M.
	h.	h.	h.		h.	h.	h.		h.	h.	h.
N. E.	6	6	6	N. & N. E.	9	6	9	N. & N. E.	6	6	6
E. Oct., Nov., Dec.,	9	6	9	E.	3	3	3	E.	3	3	3
Jan., Feb., Mar.,				S.E., S., & S.W.	9	6	9	S. E. & S.	9	6	9
Apr., May, June,	3	3	3	W.	9	5	10	S. W. & W.	18	6	6
July, Aug., Sept.				N. W.	9	6	9	N. W.	18	7	6
S. E.	6	6	6								
S.	9	6	9								
S. W. & W.	9	6	9								
N. W.	8	5	7								
N.	3	3	3								

From the tables of velocities in miles per hour, deduced from the observations by the method just explained, the following table of quantities of wind from different directions for each month is found. The rhumbs are written at the top of the table, the months at the side, and at the meeting point of a vertical and horizontal line from the head and side titles are found the quantities. The last column at the side of the table gives the total quantities for the several months, and below is found the total for each direction for the year.

QUANTITIES OF WIND.

	Month.	N.	N. E.	E.	S. E.	S.	S. W.	W.	N. W.	
ASTORIA.	January	6	795	987	150	324	2,719	1,539		6,520
	February	138	282	1,623	295	3,079	225	850		6,492
	March			1,253	509	933	585	428		3,708
	April	24	180	805	282	1,218	969	1,651	1,801	6,430
	May		102	105	186	536	1,437	876	1,393	4,635
	June		60	33		9	258	2,104	2,760	5,224
	July		150	18			633	700	4,221	5,722
	August		6	12	60	30	279	180	2,543	3,110
	September		54		102	872		246	431	1,705
	October		754	294		1,245	291	123	361	3,068
	November		583	182	399	2,252	2,927	348	447	7,138
	December		1,535	456	693	2,849	138	105	91	5,867
		168	4,501	5,268	2,676	13,347	10,461	9,150	14,048	
SAN FRANCISCO.	January	2,302	375	147	177	21	1,471	264	1,161	5,918
	February	618	180	45	558	147	992	860	306	3,706
	March	218	60	28	639	470	1,934	2,447	426	6,222
	April	30	72		168	312	2,992	4,845	209	8,628
	May				348	24	1,578	4,928	2,530	9,408
	June					18	8,338	3,428		11,784
	July					9	8,725	2,020	362	11,116
	August						2,608	3,908	2,168	8,684
	September					54	2,588	4,219	500	7,361
	October		6			63	136	4,396	96	4,697
	November	630	252		231	232	508	2,056	222	4,131
	December	414	850	18	652	489	1,290	1,576	234	5,523
		4,212	1,795	238	2,773	1,839	33,160	34,947	8,214	
SAN DIEGO.	January	91	147	45	431	813	384	643	987	3,541
	February	204	48	207	1,291	777	688	278	2,461	5,954
	March	284	114	114	1,095	726	912	1,124	3,219	7,588
	April	186	180	36	900	1,019	798	810	2,505	6,434
	May	156	24	57	396	834	1,296	930	2,296	5,989
	June	72	48	66	1,100	827	882	892	2,145	6,032
	July	39	192	16	285	1,320	607	120	4,066	6,645
	August	46	79	24	104	93	595	690	3,823	5,456
	September	48	186	39	54	265	338	426	2,418	3,774
	October	72	156	21	36	132	186	254	2,411	3,268
	November		108	93	240	231	542	198	1,226	2,638
	December	114		24	162	345	686	528	754	2,613
		1,314	1,282	742	6,094	7,382	7,914	6,893	28,311	


NW
NW
SW
SW
tuity
3
3
2
2
15
10

From this table the diagrams representing the quantities for each month from each direction (Plate, Nos. 1 to 12) are taken, and also those showing the total annual quantities of wind from each direction, and the total quantity of wind from all directions for each month. (Plate, Nos. 16 and 17.)

It seems to me altogether probable, from the study of the figures of these tables, that the scale adopted by the observer at San Francisco is greater than that at the other two points. The total quantities at Astoria, San Francisco, and San Diego, are as 59, 87, and 60, and it is hardly probable that there is so large an excess of quantity at San Francisco.

I have also the same remark to make as on the observations at Cat Island, on the absence of observations upon the intermediate points between the cardinal ones, showing the tendency to designate the winds only by the cardinal points.

From these diagrams we see at once the simple general regimen of the winds on this coast.

1. The great prevalence of westerly winds, representing a flow of the air at the surface from the ocean in upon the land.

2. The general absence of easterly winds, showing the absence of a return current at the surface.

The proportion of westerly to easterly winds is as 8 to 1.

3. The increase of westerly winds in the summer, and their decrease in the winter.

4. That when easterly winds blow at all, it is, as a rule, during the winter.

5. The N., N. E., and E. winds blow more frequently in the morning than in the afternoon hours.

6. The S. E., S., and S. W. winds are in general pretty equally distributed over the morning and evening hours.

7. The N. W. is the prevailing direction of the ordinary sea-breeze at Astoria and San Diego, and the W. at San Francisco.

Sometimes the W. wind has that character at the first-named stations, and sometimes the S. W. wind at the last named.

A closer inspection of the same diagrams will lead to other interesting results.

Considering the quantities of wind at the three places for the whole year, (Diagram No. 13,) San Diego and Astoria present remarkable similarities. There is more N. E., E. and S. wind at Astoria, and more N. W. wind at San Diego. The axis dividing the area symmetrically is in the same direction. On the contrary, at San Francisco the W. and S. W. winds give the character to the rose, and the axis makes an angle of some sixty-seven degrees with that of the other spaces. All show the same deficiency of easterly winds, and San Francisco is deficient also in southerly ones.

The monthly curves grouped in two periods, from November to March, both included, and from April to October, (Plate, Nos. 14 and 15,) show that the annual curve has the summer type impressed upon it. The summer is, in fact, the windy part of the year. The N. W. wind prevails in August at Astoria and San Diego, and the W. and S. W. at San Francisco.

The scale of Diagram No. 14 is less than that of 15, in the proportion of 10 to 14. There is scarcely any wind from points between North round by East and South. The form of the rose is exceedingly simple, and the generalization very obvious.

The winter system is less simple. The axes for the spaces for Astoria and San Diego make angles of more than 110° with each other. The N. E., E., S., and S. W. winds are considerable at Astoria, and the N. W. wind is deficient. At San Francisco the W. winds give the prominent feature to the rose curve.

As the winter is not the windy season, so the months of March and September are not the windy months; on the contrary, July is one of the windiest months of the year.

SAN FRANCISCO.

At San Francisco, the great current of air flowing from the sea to the land comes generally from the W. or S. W., rarely from the N. W.

In the period from November to March inclusive, (Diagram No. 14,) the W. is the prevailing wind, exceeding in quantity both the others; the S. W. wind exceeding in quantity the N. W. In the period from April to October, (Diagram No. 15,) the W. and S. W. winds are nearly equal, and each exceeds the N. W.

The W. wind has in general the features attributed to the sea-breeze, beginning after the rising of the sun, increasing until after the hottest part of the day, and dying out, or much diminishing, at nightfall.

The W. and S. W. winds give the prominent features to the wind rose at San Francisco.

The S. W. is the prevailing wind in June and July, S. W. and W. winds blowing nearly the whole of those months, not succeeded by an easterly land breeze, but rising and falling.

The rose curves for May and August resemble each other. The N. W. and S. W. winds being nearly equal in quantity, and each less than the W. wind. So the curves for April and September, when the N. W. wind has nearly died out.

The W. wind diminishes in quantity through March and February, and through October, November, and December, to January.

The N. W. wind increases again from April towards December, and is very small in October and November. The S. W. wind disappears in October, changing the form of the rose curve, but reappearing in November and December, and increasing toward January.

The W. wind has a maximum in April and May, and another in September and October; the minimum being July and January.

The N. wind in December, January, and February, reaching a maximum in January, is the only other point to be noticed. San Francisco partakes with the other places in the general absence of easterly winds. The tables show a little in the winter. There is also but little S. wind there.

ASTORIA AND SAN DIEGO.

In general, the winds at these two places resemble each other more than those at San Francisco do either. The rose curves for April, May, June, July, and August (Nos. 4 to 8) have the same general character. The mean curve for the year (No. 13) and for the summer period (No. 15) has also the same general character.

The N. W. wind is the summer wind, and has the characteristics of the sea-breeze, but there is no return land breeze. The N. W. wind reaches a maximum in July, and a minimum in December. It is the great prevailing wind of the year (Diagram, No. 13) at San Diego. As it decreases, it is generally replaced by W. and S. W. winds of less quantity. In December the quantities of the three winds are nearly equal.

The resemblance of these winds at San Diego and Astoria is remarkable, the remarks just made applying generally to both places.

There is, however, much less N. W. wind at Astoria than at San Diego. Except in June, July, and August, there is some S. wind each month at Astoria; and especially from September through October, November, December, and February, it presents a marked feature of the rose.

At San Diego this is less marked, the two agreeing most nearly in quantity in March, April, and May.

The S. E. wind is a distinct feature in both places in February and March, and at San Diego in April and June.

The E. wind is prominent at Astoria in January, February, and March, and the N. E. from October to January inclusive.

Astoria has the most easterly wind, the N. E. beginning in October, and blowing until February, and being replaced by the E. wind in March.

2. ON OZONE. By PROF. CHARLES SMALLWOOD, of Montreal, C. E.

It would be unbecoming in me, as forming a part of the deputation to Albany last year for the purpose of inviting the Association to meet at this place, were I not to take advantage of the present moment, to greet you, gentlemen Members of the American Association, with a cordial and hearty welcome, and I need scarcely add, that the like sentiment inspires the whole of the inhabitants of this city.

Until the present time, these Annual Meetings have been confined to the United States alone, (although not exclusively American,) and separated only by an imaginary boundary, which has now been removed, for we here meet, united as one family, having one common object in view, "the advancement of science;" we are treading the same peaceful path of knowledge; we are assembled under the broad, the vast, canopy of the American firmament; the gentle breeze that wafts the red cross banner of St. George and merry England alike unfurls the stars and stripes, the emblem of your land of freedom. Long may these two flags entwine in peace, in kindred folds, and may that master-piece of scientific genius, the electric cable, which is at this moment being laid beneath the Atlantic sea, whose waves science has measured with a mighty span, be the peaceful band, that will cement more firmly the destinies of the two great nations of the earth, under the benign and able guidance of your worthy President and our beloved Queen, and may science, which knows no country, no nation, no language, be rendered more subservient to the happiness and welfare of the whole human family.

A year has now passed since the deputation from this place enjoyed the hospitalities of one of your large cities; the familiar and friendly faces of many we met there, and now present, call to mind many pleasant recollections, but as in all things mundane, we have here some cause for sadness, — for in the few fleeting moons that have waned since last we met, death has taken from our midst a Redfield, a Bailey, and a Mitchell, each preëminent in his department of scientific research, and to science and us, an irreparable loss, and the Association has done itself honor in paying a tribute to their memories; but the midnight lamp of the man of science must grow dim, the experimentalist must for ever quit the busy scenes of his laboratory, the eye of the astronomer must be closed, for the life of the philosopher is but mortal.

It is my intention to lay before the section the results of observations made on the amount of ozone present in the atmosphere. The place of observation is at St. Martin's, about 9 miles due west of Montreal, and is 118 feet above the mean level of the sea; it is situated in the centre nearly of the Isle Jesus, an island surrounded by the branches of the Ottawa; the place of observation is a little more than three miles from the river, thus being sufficiently inland to be removed from any transient vapor or fog, which is often present in the proximity of rivers; it is a flat island, and the whole of the neighborhood is under cultivation.

It is not my purpose to enter into a lengthy detail of the chemical composition of ozone; enough for our present purpose to define it to be a compound of oxygen, analogous to the peroxide of hydrogen, or that it is oxygen in an allotropic state, that is, with the capability of immediate and ready action impressed upon it. To Schonbein is awarded the discovery, who, in 1840, applied the term ozone to the peculiar smell which is perceptible during the action of the electrical machine, and also during the decomposition of water by the galvanic apparatus. It was subsequently ascertained that a similar smell is developed by the influence of phosphorus on moist air, and also by a great many chemical changes, and for some time its existence was recognized by its smell, or odor, alone; but in April, 1848, Schonbein became possessed of another of its characters, namely, its oxidizing principle, and it is this property which it possesses more particularly, when we direct our attention to its presence in the atmosphere, although these oxidizing properties may be common to some other bodies, as nitrous acid, which is said to be generated in the atmosphere by atmospheric electricity.

When largely diffused in the atmosphere, it causes, like chlorine, (to which it is somewhat allied,) very unpleasant sensations, such as difficult respiration, and it acts powerfully on the mucous membrane; it kills small animals very quickly; it is insoluble in water, and oxidizes very quickly all metallic bodies; and it has the power, in a large degree, of destroying *miasma* arising from the decomposition of animal and vegetable substances; and Schonbein came to the conclusion, that its formation depended upon the action or formation of atmospheric electricity, and he referred the beneficial effects of thunderstorms to the action of the ozone formed, neutralizing the miasma arising from the decomposition of animal, and may be vegetable, substances; and it possesses, in a powerful degree, bleaching properties, and in this it is again analogous to chlorine.

Since Schonbein brought its properties before the scientific world, it has received more or less attention both from the physician and the meteorologist.

It has been advanced, that during the presence of cholera and other epidemic disease, its absence was remarked; while, on the other hand, when the atmosphere has indicated a great amount present, diseases of the lungs and mucous membrane have been more prevalent; it has been still further stated that its action on the vegetable kingdom is similar in its effects as in the animal economy; the potato disease or rot especially, and other diseases in vegetables have, it is said, been caused by either its absence or presence in too large quantities.

It would far exceed the limits of time allotted to me, to enter fully into the progressive steps of the investigation or history of ozone, for it has engaged the attention of physicians in England and on the continent of Europe, and I am happy to say, that some members of the American Association have devoted considerable attention to it, and I have deemed it of sufficient import to lay before the section the result of some eight years of investigation, or nearly six thousand observations. This includes observations during the visitation of the cholera in 1854, and I heartily trust that the Association may, by its influence, extend these observations through the whole of the United States territory, and, as far as practical, throw some light on its action in the animal and vegetable kingdom; and I am sure a subject of so much importance, and which must (if we are to believe the report of some investigators) exert an influence on both the health of animals and of plants, will be at once a sufficient ground for extending such observations, which should be as uniform as possible.

The method of estimating and detecting the amount of ozone is by what is called the *Ozoneometer*, which is nothing more than slips of paper, wetted with the solution of starch and iodide of potassium; these became blue on exposure, owing to the oxidization of the potassium by the ozone, and the setting free of the iodine; the formula I use, and the one generally adopted, is ʒ i of starch boiled in ℥ i of distilled water, and when cold 10 grains of the iodide of potassium is mixed with it; it is quickly spread on paper and dried in the dark, and must be kept in a dry place and free from light until required, when they are placed in a situation shaded from the sun and rain; these strips are one half an inch wide, and from three to four inches long. Dr. Moffatt, an eminent English physician, and who has paid much attention to the subject, places his slips of paper in a box without a bottom, so as to be *excluded* from the light; but so far as my observations go, I have found so little difference in the two methods, that I have continued that of Schonbein's, as I have before stated, and expose the slips of paper to light, but *excluded from the sun and rain.* The amount of ozone present is estimated in 10ths; the deep shade or saturation being 10, and diminishing in depth of shade to 0.

It has also been asserted that slips of paper placed at high elevations have exhibited a deeper shade. To test this fact I exposed slips of prepared paper at an altitude of 80 feet, on the top of a pole or mast, which is used for collecting atmospheric electricity; and as far as my observations go, I could detect no appreciable difference from those exposed 5 feet from the ground; and if I might be permitted to suggest, that, to insure uniformity, the elevation of 5 feet might be considered the standard height, and which is at once convenient, and far enough removed from the effects of terrestrial radiation or deposit of dew, leaving it, of course, to observers to adopt at the same time any other method which might suggest itself, during the observations on these phenomena.

So far I have, as concisely as the subject would permit, traced its history, properties, and method of observations, and the propriety of so doing may indeed be questionable, before so learned a body; but I have felt that the subject might be new to some present; and with a wish that uniform observations should be made, I deemed it well to state very briefly its prominent character, and in so doing I have thrown myself on your indulgence. I may just state that the color of the test paper may be brought more fully out by moistening it with water.

I shall now proceed to give the *Section* the results of observations made by these means.

The questions for our investigation, and which naturally arise, are these: What is the effect of the presence of ozone on the meteorological conditions of the atmosphere, as indicated by the instruments most in use?

And, secondly, what influence does its presence or absence exert on the health of animals or vegetables? or does its presence or absence give rise to disease?

1. What are the *barometric* indications?

The presence of ozone in the atmosphere is accompanied by a low reading of the barometer, which generally continues while the *ozonic* period lasts; this period is accompanied or terminated almost invariably by precipitation in the shape of rain or snow.

Therometer.—I have observed the presence of ozone at all temperatures when the therometer has indicated 20° (below zero), and as high as 80°, and in all the intermediate temperatures, and it is generally in larger quantities during a fall of snow than of rain. The *psychometer* is a certain indication of the presence of ozone, for it would appear that a moist state of the atmosphere was necessary for its production or development, for when the difference between the *dry* and *wet* bulb therometer is little, the presence of ozone in considerable quantity is invariably present; but when the difference between the two therometers is considerable, no ozone is appreciable by the *ozone-ometer.* This fact, and the only one which (as far as my observations here go) is in connection with the presence or absence of ozone, has led me to compare the presence of ozone with the presence of precipitation in the shape of snow or rain, which gives a remarkable coincidence. For in and during the past seven years there were 918 days on which rain or snow fell (this is regardless of the amount or duration), and during the like period there were 816 days on which ozone was present in a quantity of five tenths; any amount below that quantity in this estimation is not taken into consideration in the discussion. In the year

1850 there were 106 days of precipitation, and 110 days of ozone.
1851 " 123 " " 135 " "
1852 " 136 " " 152 " "
1853 " 136 " " 114 " "
1854 " 133 " " 73 " "
1855 " 140 " " 110 " "
1856 " 144 " " 126 " "

The small amount of ozone in 1854, which was the year of the last visitation of cholera, would tend to favor the opinion that there was a deficiency of ozone in the atmosphere during the prevalence of that epidemic. A deficiency was, however, observed in almost every month of that year, although the number of days on which snow or rain fell were almost equal with the other years; for which see the following table, which shows the amount for each year, and for each respective month:—

YEARS.	1851.		1852.		1853.		1854.		1855.		1856.	
	DAYS OF		DAYS OF		DAYS OF		DAYS OF		DAYS OF		DAYS OF	
MONTHS.	Precipitation.	Ozone.	Precipitation.	Ozone.	Precipitation.	Ozone.	Precipitation.	Ozone.	Precipitation.	Ozone.	Precipitation.	Ozone.
January, . . .	10	5	14	7	9	11	14	6	12	6	12	14
February, . .	6	7	11	8	11	9	16	6	8	9	9	16
March, . . .	11	6	17	11	9	21	17	7	9	6	10	17
April, . . .	12	8	10	11	7	12	10	6	14	8	11	20
May, ...	16	14	9	8	16	12	8	7	6	9	13	20
June, ...	12	12	17	18	15	11	10	8	15	12	10	17
July, ...	13	16	11	17	9	4	5	4	7	11	12	18
August, . . .	8	15	9	16	13	7	7	3	11	12	15	11
September, . .	11	15	10	16	11	9	11	3	12	11	14	12
October, . . .	12	11	17	18	14	5	11	8	18	11	10	8
November, . .	10	13	10	12	14	11	13	11	14	9	15	8
December, . .	11	13	18	14	8	8	12	7	14	6	11	5
Total,	123	135	136	152	136	110	133	73	140	110	144	126

Southerly and easterly *winds* being the point from which our rain or snow generally comes, are for the most part present, during the indications of ozone, while, on the contrary, northerly or westerly winds very rarely accompany its development.

In reviewing these observations, there is no condition of the atmosphere appreciable by our instruments, that indicates the presence of ozone, except the presence of vapor or humidity.

Schonbein has asserted that a high electrical state of the atmosphere was always present when ozone was developed, and that the amount depended essentially on the amount of atmospheric electricity. From the comparison of nearly 6,000 observations on the electrical state of the atmosphere, and the amount of ozone taken at the same hour, at this place, and carefully compared, I have not found that opinion sustained, neither have I found its amount or presence influenced by the appearance of the *aurora borealis,* which has also been said to be the case.

From these observations it would appear that a moist and humid atmosphere was necessary for the development of ozone, and this may account in some measure for its more constant presence and its greater quantity in proximity to the sea. So far as regards its effects on the production of disease in plants, especially the potatoe, and to which it has been more especially referred, it is almost certain that one of two causes must have given rise to the lamentable failure in this useful vegetable; either that the soil must have furnished the medium of disease, or the action of the atmosphere upon the leaves and stem of the plant;—the causes which act upon the stem and leaves involve the action alone of atmospheric influences, while those that act through the medium of the soil are more numerous.

In this neighborhood the disease showed itself after rain followed by a hot sun, the atmosphere being loaded with moisture or vapor,—just the condition essentially proper for indicating the presence of ozone,—and the disease was much more extensive on wet and clayey soils than on sandy or dry ones.

It cannot be doubted that an agent so active as ozone, if really present, must exert a great influence on the health of individuals, as well as animals and plants; the manner of its production, whether by chemical action or electricity or magnetism, demands from us further investigation, and these investigations should be carried out with uniformity for the sake of careful comparison,—one point should not be overlooked, that is, to mark carefully the amount of vapor present in the atmosphere, as the intimate connection between them is too prominent to escape observation.

I have, as you will perceive, offered no theoretical deductions. If, as our continental brethren assert, it does possess such powerful and wonderful properties, it must be evident that the American Association should at once take up the subject, in a way that we may arrive at important conclusions. I should not be justified in expressing a doubt on the labors of others in this department of physical science, neither do I think it fair to offer any conclusions until our observations are more extended; and it is with this intention that I have brought it before the Association, hoping that between now and our next meeting, we may be able to investigate and compare observations so as to give it a proper place in this department of physical investigation.

3. THE METEOROLOGY OF THE VICINITY OF MONTREAL. By Prof. CHARLES SMALLWOOD, of Montreal, C. E.

I AM well aware that many of you are here for the first time in this, our northern city, and have scanned, and I have no doubt admired, the numerous edifices,—those artificial structures erected by the human hand, guided by human skill, and well suited to our wants. I am also aware that many among you have bent your investigations beneath our alluvial and fruitful soil, to contemplate the geology of our rocky formations, and the deposits of by-gone ages, the work of that Divine Architect at whose command those bright and countless orbs that spangle in our firmament were brought into existence, and which form to the astronomer so many objects for his study; and I felt it might be interesting to you to know something of our climatology, and it is for this purpose I intend laying before the section some remarks in illustration, reduced from observations taken at St. Martins, nine miles due west of this place; and I shall for this purpose confine my observations to the means reduced from the last septennial period, although the observations on record extend over a much longer period of time.

The geographical coördinates of the place are 45° 32′ north latitude, and 73° 36′ longitude, west of Greenwich. The cisterns of the barometers are placed 118 feet above the level of the sea. The instruments used are standard instruments; the barometric observations are all reduced to the freezing point (32° F.), and the temperatures are all in Fahrenheit's scale. The hygrometric observations are reduced by the tables and formula adopted at the Greenwich Observatory in England. The receiver of the rain gauge is placed twenty feet above the soil. The direction and velocity of the wind are ascertained by a self-registering instrument, which indicates its velocity by dots on a paper register in miles linear. The electrical apparatus is provided with a collecting lantern, which is elevated eighty feet from the ground. The solar and terrestrial radiators are also read in terms of Fahrenheit's scale. The ozoneometer is of Schonbein's construction. The whole of the means are reduced from three daily observations, taken at 6 A. M., 2 P. M., and 10 P. M.; extra hours are also set apart for any unusual phenomena.

Barometer.—The mean height of the barometer for this period (seven years) was 29.676 inches; the mean reading for the same septennial period in January was 29.744 inches; February, 29.744 inches; March, 29.492 inches; April, 29.679 inches; May, 29.604 inches; June, 29.718 inches; July, 29.715 inches; August, 29.754 inches; September, 29.722 inches; October, 29.619 inches; November, 29.769 inches; December, 29.565 inches. The highest reading observed and on record here was on the 8th of January, 1855, and at 4 P. M. it attained the unusual height of 30.876 inches; the lowest reading on record was in December, also in 1855, and was 28.689 inches, giving an absolute range of 2.187 inches. The mean yearly range for the seven years was 1.032 inches, and for the months as follows:—

	Inches		Inches		Inches
January	1.550	May	0.800	September	0.815
February	1.131	June	0.752	October	0.951
March	1.145	July	0.616	November	1.295
April	1.090	August	0.701	December	1.538

There are two maxima and two minima variations occurring in the barometer in the 24 hours; the maxima variations occur at between 9 and 10 o'clock A. M., and between 9 and 10 P. M.; the minima variations occur at 3 A. M. and 3 P. M.

Thermometer.—The temperature of the air for the same period (7 years) exhibits a yearly mean of 41° 56′. The mean temperature of January was 13° 26′; February, 13° 31′; March, 25° 44′; April, 40° 12′; May, 55° 70′; June, 62° 11′; July, 74° 78′; August, 61° 21′; September, 58° 12′; October, 46° 04′; November, 31° 49′; December, 13° 80′. The absolute mean range for the same period has been from 90° 9′ + to 27° 4′ — (below zero). The absolute monthly range was, in

January	+ 40° 7 to 25° 1 —	July	+ 97° 1 to 47° 8 +
February	+ 41° 1 to 25° 2 —	August	+ 96° 7 to 40° 6 +
March	+ 56° 0 to 6° 7 —	September	+ 91° 2 to 30° 4 +
April	+ 75° 6 to 10° 1 +	October	+ 75° 7 to 23° 8 +
May	+ 86° 6 to 25° 7 +	November	+ 60° 4 to 5° 7 +
June	+ 94° 5 to 40° 5 +	December	+ 42° 1 to 26° 3 —

The highest temperature in the shade on record here was 100° 1′, and the lowest was 36° 2′ below zero; giving a climatic range of 136° 3′. The hottest month is July, and the coldest month is February. The warmest part of the day in summer is at 3 P. M., and in the winter season at 2 P. M. The coldest part of the day in winter is at a little before sunrise.

The mean yearly temperature of the *dew point* reduced for the same period, was 35° 6′, and for the different months as follows:—

January	9° 6	July	65° 0
February	7° 4	August	53° 1
March	20° 2	September	52° 2
April	34° 6	October	40° 8
May	47° 2	November	26° 1
June	54° 1	December	8° 1

The relative degree of humidity for that period, saturation being 1.000, was 814; and for the months:—

January	.869	July	.744
February	.808	August	.765
March	.835	September	.809
April	.812	October	.821
May	.774	November	.824
June	.770	December	.832

The *elastic force of vapor* exhibits a daily maximum at 3 A. M., and a minimum at between 3 and 4 P. M. The summer quarter, which embraces June, July, and August, is the dryest quarter; next is the spring quarter, which embraces March, April, and May. The autumnal and winter quarters are the most humid. Complete saturation does not often occur; it has nevertheless taken place about four or five times in each year.

The mean number of days on which *rain* fell for the same period is 73 per year, and the number of days on which *snow* fell is 43, making a sum of 116 days on which precipitation took place, leaving 249 fair days as a yearly mean for the 7 years. There is, on an average, about 110 nights suitable for astronomical purposes in each year.

The yearly mean amount of rain for the same period was 43.004 inches in depth on the surface; and the depth of snow also on the surface, shows a yearly mean of 95.76 inches. The monthly means for snow and rain are as follows:—

	Inches of Rain.	Inches of Snow.		Inches of Rain.	Inches of Snow.
January	0.600	22.38	July	3.003	
February	0.167	25.00	August	5.908	
March	0.380	18.79	September	5.831	
April	4.624	2.46	October	6.063	1.80
May	4.386		November	5.055	4.34
June	6.013		December	0.940	17.71

This gives a mean of 52.380 inches of rain and melted snow. This is reduced by the Smithsonian formula, which does not hold good or correct for low temperatures; and I think "1 to 8" would be more accurate. The greatest amount of rain, which fell in 24 hours, on record here, was in September, 1853, and amounted to 5.142 inches, but this is unusual. You will perceive that we are a little more than 5 months without snow.

The difficulty in this climate of measuring the amount of evaporation from the surface of the water, except for 7 months of the year, owing to frosty nights, has induced me to undertake the registration of the amount of evaporation from the surface of ice during the remainder of the year, (5 months,) so as to compensate in some measure for the defect in the observations on the amount of evaporation from the watery surface. These combined observations give a mean of more than 30 inches as the amount of water evaporated. The evaporator is shaded from the sun and rain, but is exposed to the currents of wind; so is also the icy surface in winter.

I am led to believe this amount is tolerably correct. The mean amount of evaporation from the surface of water alone for the 7 months is nearly 21 inches, the remaining amount being furnished by the evaporation which takes place from the surface of ice during the remaining 5 months.

Winds. — The most prevailing wind of the year is the westerly, and the mean direction for the 7 years in the different months is as follows: —

January	N. E. by E.	July	S. W. by W.
February	W. S. W.	August	W. N. W.
March	W.	September	W. N. W.
April	N. E. by E.	October	W. by W.
May	N. W. by N.	November	W. N. W.
June	S. W. by W.	December	N. E. by E.

The greatest velocity on record here exceeds somewhat 60 miles per hour linear. There seems a disposition for a change both in the direction and velocity at 3 P. M. and at 3 A. M., which corresponds precisely with the diurnal barometric fluctuations. The whole amount of miles linear of wind during the past year (1856) was 53,061.63 miles, which being resolved into the four cardinal points, gave N. 6,969.80 miles; S. 5,298.89 miles; E. 10,776.40 miles; and W. 30,016.56 miles. The maximum velocity during the past year was 44.40 miles per hour. There were 2,220 hours 15 minutes calm, and 6,546 hours during which the atmosphere was in motion. Below is a table of the anemometric observation during the year 1856, showing the direction and amount of miles from each quarter of the compass, and also the amount of miles run in each month; also the amount of calm in hours for each month.

Course.	Velocity in Miles.	Course.	Velocity in Miles.	Course.	Velocity in Miles.
N.	310.50	S. E. by E.	403.00	W. S. W.	4679.66
N. by E.	211.50	S. E.	297.00	W. by S.	4542.50
N. N. E.	412.00	S. E. by S.	690.20	W.	3111.80
N. E. by W.	661.70	S. S. E.	374.00	W. by N.	3103.00
N. E.	1325.00	S. by E.	578.50	W. N. W.	4790.00
N. E. by E.	8092.60	S.	714.70	N. W. by W.	2112.80
E. N. E.	892.70	S. by W.	238.30	N. W.	2728.00
E. by N.	237.10	S. S. W.	497.57	N. W. by N.	1269.00
E.	86.30	S. W. by S.	608.10	N. N. W.	687.00
E. by S.	156.00	S. W.	2375.70	N. by W.	77.00
E. S. E.	240.00	S. W. by W.	3845.60		

RESOLVED INTO THE FOUR CARDINAL POINTS.

Months.	Miles North.	Miles South.	Miles West.	Miles East.	Total Miles.	Hours and Minutes Calm.
January	395.40	95.77	4115.66	1744.10	6351.23	143.00
February	71.90	280.00	4854.80	277.20	5463.90	166.00
March	674.80	917.30	3706.60	567.70	5866.40	177.00
April	234.00	116.00	1644.00	2585.10	4579.10	247.00
May	1415.00	484.00	1323.00	1321.00	4540.00	179.10
June	350.00	768.00	1450.00	582.00	3130.00	168.40
July	776.00	345.00	1652.20	111.00	2884.00	174.20
August	621.00	242.30	1018.20	569.30	2450.00	269.20
September	471.00	589.50	1249.00	490.00	2799.50	243.14
October	843.00	371.00	2270.00	248.00	3752.10	226.45
November	653.00	650.00	2386.00	975.00	4644.00	149.00
December	464.70	458.00	4387.00	1310.00	6628.20	78.30

The yearly mean intensity of the sun's rays for the same septennial period, is 102° 6′, and for the months as follows: —

January	79° 4	July	121° 4
February	87° 5	August	118° 4
March	119° 4	September	103° 9
April	107° 1	October	99° 4
May	110° 5	November	89° 7
June	110° 2	December	84° 9

The yearly (septennial) mean of terrestrial radiation was 11° 6′, and for the months as follows: —

January	20° 9	July	46° 7
February	22° 6	August	38° 1
March	18° 2	September	34° 2
April	8° 0	October	18° 9
May	29° 6	November	11° 6
June	39° 1	December	25° 1

The amount of dew is very variable, but bears a proportion to the degree of terrestrial radiation.

The mean of cloudless days were 57 days perfectly cloudless; the prevailing clouds are the cumuli stratus and cirri stratus.

The song sparrow (*fringilla melodia*), the harbinger of the Canadian spring, generally makes its first appearance the first week of April. Frogs (*rana*) are first heard about the 23d of April. Shad (*alosa*) are caught the last week in May. Fire-flies (*lampyrus corusca*) are first seen about the 24th of June; and the snow bird (*plectrophanes nivalis*) generally makes its first appearance about the 20th of November. Swallows (*hirudo rufa*) about the 18th of April. Our winter generally sets in about the latter week of November, or the first week in December, and is ushered in by a fall of snow from the N. E. by E., and this is the point from which our winter storms come. Rain generally comes accompanied with a wind from the S. S. W., or S. E., and also from the N. E. by E.

We have generally a few days of that poetic season, the Indian summer, in November.

> "The year's last, loveliest smile,
> That comes to fill with hope the human heart;
> And strengthen it to bear the storms awhile,
> Till winter's days depart."

Our snow storms of winter are from the N. E. by E.; and for some hours before they form, the eastern horizon becomes gradually covered with heavy *strata* clouds of a deep leaden hue. The upper strata of clouds are generally a mixture of *cirri cumulus* and *stratus*, moving from the south; but the surface wind is from the point I have stated, N. E. by E. The wind during these storms often attains a velocity of some 30 or 40 miles per hour; the barometer is falling, and the thermometer somewhere about zero, the psychometer indicates an increasing amount of moisture, the electrometers indicate a very high tension of *negative* electricity, often an amount of 300° in terms of Volta's No. 1 electrometer, and sparks are constantly passing between the receiver and discharger for hours.

Minute but perfect crystalline forms of snow commence to fall, and may continue for some 48 hours; and I have seen some 12 or more inches of snow fall during this time. Precipitation then ceases; the wind veers *always* by the N. to the W., or W. N. W., with a velocity of some 30 miles per hour, (this is our cold term); and the wind carries the loose, finely crystallized snow in clouds before it; this is, in Canadian parlance, a "poudrerie." The wind is intensely cold; the thermometer, during this period, attains a minimum of some 30° below zero. The sky is partly covered by *cumuli* clouds, with a few *strati;* the electrometers still, indicate a high tension, but of an opposite or *positive* character. This westerly wind may last some 48 hours or more, and lulls down at sunset; may be, of the second day, into a calm. The blue tint of the sky is very deep, and the rays of the setting sun throw a red or orange shade on the snowy scene, and the atmosphere attains a greater dryness, and the electrical action gradually ceases with the wind.

Our thunderstorms of summer, which give a yearly mean of 14 (for the same period of 7 years) are of short duration, forming generally in the W. or N. W., and the electricity varies in kind.

The months of April, May, and June bring returning summer; the nights of July and part of August are generally oppressive; the temperature often remains at 70° during the night; but the Canadian autumn is very pleasant. The woods, with its leaves of a thousand varied tints, and the blue and cloudless sky, with frosty nights, remind us that the good times of the merry sleigh-bells are near.

Notwithstanding these vicissitudes and extremes of temperature, the soil is very productive, and vegetation prolific and rapid; and it has again pleased an all-wise Providence, during the present year, to crown the labors of the Canadian husbandman with a bountiful and abundant harvest.

PART II.

B. NATURAL HISTORY.

I. GEOLOGY AND GEOGRAPHY.

1. EXPLANATIONS OF THE GEOLOGICAL MAP OF MISSOURI, AND A SECTION OF ITS ROCKS. By PROFESSOR G. C. SWALLOW, State Geologist of Missouri.

IN calling your attention to the map and section of the rocks of Missouri, it has been deemed advisable to give a brief description of the formations, and the useful minerals, which they contain.

So far as observed, the stratified rocks of Missouri belong to the following divisions: —

SYSTEM I. — QUATERNARY.
SYSTEM II. — TERTIARY.
SYSTEM III. — CRETACEOUS.
SYSTEM IV. — CARBONIFEROUS.
SYSTEM V. — DEVONIAN.
SYSTEM VI. — SILURIAN.

The rocks of these divisions will be examined in their order, from the top, down.

SYSTEM I. — QUATERNARY.

As the formations of this system have been described in another paper, I shall simply give their names here, in their order, as follows: —

ALLUVIUM.
BOTTOM PRAIRIE.
BLUFF.
DRIFT.

SYSTEM II.—TERTIARY.

There is a formation made up of clays and sands, extending along the bluffs, and skirting the bottoms, from Commerce, in Scott county, westward to Stoddard, and thence south to the Chalk Bluffs in Arkansas.

The following section, obtained in the neighborhood of Commerce, will give a good idea of the character of these beds. The strata are numbered in their natural order, from the top, down.

No. 1. 9 feet. Pebbles, sand and clay, intermingled.
No. 2. 2 feet. Sand and iron ore, brown haematite.
No. 3. 10 feet. Brown and buff sand interstratified.
No. 4. 12 feet. Buff and white sand interstratified, containing rounded masses of sandstone of the same character and color as the sand forming the strata.
No. 5. 5 feet. Clay and gravel, of a bright chrome yellow.
No. 6. 1 foot. Clay and haematite ore, nearly all iron.
No. 7. 47 feet. Blue shale, which separates, on exposure, into rhomboidal masses.
No. 8. 2 feet. Carbonate of iron in septaria and nodular masses, or in regular strata, which break into rhomboidal masses.
No. 9. 6 feet. Blue shale, like No. 7.
No. 10. 1 foot. Iron ore, like No. 8.
No. 11. 11 feet. Blue shale, like No. 7.
No. 12. 1½ feet. Carbonate of iron, like No. 8.
No. 13. 31 feet. Blue shale, like No. 7, with some thin bands and nodules of iron ore.
No. 14. 7 feet. Sandy clay, with thin strata, and globular masses of haematite ore.
No. 15. 18 feet. White sand, interstratified with thin brown strata, containing some rounded masses of sandstone.
No. 16. 5 feet. Sand, of a light peach-blossom color, interstratified with brown beds.
No. 17. 12 feet. White sandstone, in thick beds. The upper part is hard and vitreous; but the lower is soft and friable. This rock very much resembles the Saccharoidal sandstone of the

Calciferous series, and appears to have been much worn by running water.

No. 18. $\frac{1}{12}$ feet. Very hard, compact, oxide of iron. It is strong, and rings like pot-metal.

No. 19. 20 feet. Salmon-colored, white, purple, and yellow sands, interstratified with clays of the same colors.

No. 20. 1 foot. Spathic iron ore.

No. 21. 13 feet. Blue potter's clay. — 214 feet, total thickness.

I have observed no fossils in these beds, except the impression of a leaf on the sandstone of No. 17.

In the absence of any positive proof of the age of these interesting strata, I have marked them Tertiary, until future discoveries shall show their true position.

The *iron ore* of these beds is very abundant, and exceedingly valuable. The Spathic ore has been found in no other locality in southeastern Missouri, so that the large quantity and excellent quality of these beds will render them very valuable for the various purposes to which this ore is peculiarly adapted.

The *white sand* of these beds will be very valuable for glass-making, and for the composition of mortars and cements. The *clays* are well adapted to the manufacture of pottery and stoneware.

SYSTEM III. — CRETACEOUS.

Beneath the Tertiary beds above described, in the bluffs of the Mississippi above Commerce, the following strata were observed: —

No. 1. 13 feet. Argillaceous sandstone, variegated with gray, brown, and white.

No. 2. 20 feet. Soft bluish-brown sandy slate, containing large quantities of iron pyrites.

No. 3. 25 feet. Whitish-brown impure sandstone, banded with purple and pink.

No. 4. 45 feet. Slate, like No. 2.

No. 5. 45 feet. Fine white silicious clay, interstratified with white flint more or less spotted, and banded with pink and purple.

No. 6. 10 feet. Purple, red, and blue clays. — 158 feet, the entire thickness.

These beds are very much disturbed, fractured, upheaved, and tilted, so as to form various faults and axes, anticlinal and synclinal; while the strata, above described as Tertiary, are in their natural position, and rest nonformably upon these beds.

These facts show the occurrence of great disturbances, subsequent to the deposition of these beds, and anterior to the formation of the strata above.

We have no clue to the age of these rocks, save that they are older than the Tertiary beds above, and newer than the Trenton limestone below. They somewhat resemble some Cretaceous beds found in several places on this part of the continent; and these facts have led me to the inquiry, whether they are Cretaceous. Our future investigations may show their true position.

We have observed no fossils in these rocks.

SYSTEM IV. — CARBONIFEROUS.

This system presents two important divisions:—

UPPER CARBONIFEROUS, OR COAL-MEASURES.
LOWER CARBONIFEROUS, OR MOUNTAIN LIMESTONE.

The COAL-MEASURES are made up of numerous strata of sandstones, limestones, shales, clays, marls, spathic iron ores, and coals. We have observed about 1,500 feet of these coal-measures, containing numerous beds of iron ore, and at least eight or ten beds of good workable coal.

These rocks, with the accompanying beds of coal and iron, cover an area of more than 27,000 square miles in Missouri.* If a line be drawn from the north-eastern corner of the State, through Clark, Lewis, Shelby, Monroe, Andrain, Callaway, Boone, Cooper, Saline, Henry, St. Clair, Cedar, and Dade counties, to the middle of the western boundary of Jasper, this irregular boundary will separate the great body of the coal-measures, on the north-west, from the older rocks, on the south-east. Besides the large body of coal-measures on the north-west side of this line, there are extensive beds in Cole, Moniteau, St. Charles, St. Louis, and Callaway counties. The common bituminous and cannel coals are the only varieties of this mineral observed. These exist in vast quantities — one might almost say, inexhaustible.

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* The Missouri coal basin is one of the largest in the known world. Besides the 27,000 square miles in Missouri, there are in Nebraska at least 10,000 square miles, in Kansas, 23,000, in Iowa, according to Dr. Owen, 20,000, in Illinois, 20,000 (?), making, in all, at least 100,000 square miles. And we may expect the explorations of Maj. Hawn in Kansas, and others in Nebraska, will add much more.

The fossils are numerous and interesting. So far as our observations extend in Missouri, the *Fusulina cylindrica*, *Spirifer Meusebachanus*, *S. planoconvexa*, *S. hemiplicata*, *S. Kentuckensis*, *Productus splendens*, *P. aequicostatus*, *P. Nebrascencis*, *P. Wabashensis*, *Chonetes mesoloba*, *C. parva*, *C. Smithi*, *Myalina subquadrata*, *Allorisma regularis*, *A. terminalis*, *Leda arata*, *Pleurotomaria sphaerulata*, *Campophyllum torquium*, and *Chaetetes milleporaceus*, are confined to, and very characteristic of, the coal-measures.* The discovery of the fact that these fossils are confined to the coal-measures, has enabled us to point out the existence of the coal-measures, and the coal beds contained in them, over an area of many thousand miles, where geologists had supposed no coal-measures and no coal existed.

Of the Lower Carboniferous rock, we have observed the following formations: —

CHESTER SANDSTONE	75 feet.
UPPER ARCHIMEDES LIMESTONE	250 "
FERRUGINOUS SANDSTONE	195 "
ST. LOUIS LIMESTONE	225 "
ARCHIMEDES LIMESTONE	350 "
ENCRINITAL LIMESTONE	550 "

It will be observed, that the *Chester Sandstone* and the *Upper Archimedes Limestone* have been added since the publication of our Second Annual Report, in 1854. After making a careful comparison of a large collection of fossils from Chester, with those from the northern part of the State and Iowa, I am fully satisfied that the beds at Chester, in Illinois, and St. Mary's, in Missouri, are distinct from the Archimedes beds in the northern part of the State, and at Keokuk.

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* So far as I know, the Second Annual Report of the Missouri Survey made known the very striking and important difference between the fossils of the coal-measures and the Lower Carboniferous rocks of the Mississippi valley. It was also shown by the same report that some, at least, of the different beds of limestone in the coal-measures could be distinguished by their fossils. This is another application of the use of fossils, of vast importance in tracing out the position of the various coal beds in these rocks.

I had had some doubts as to the true position of these beds in the series; but the fact that a large number of the fossils are so similar to some found in the more northern beds, and at the same time so different from any thing found in any of the other Carboniferous rocks, and also the fact, that soon eight or ten of the Chester species appear to be identical with those found in the northern beds, led me to place these rocks in the same formation with the Lower Archimedes beds.

But, on the authority of Prof. Hall and Dr. Norwood, who have had a better opportunity of examining these rocks and their fossils, I now place them above the Ferruginous sandstone, as in his section. The sandstone above these beds is also placed in this series; for, so far as my observations extend, it has much stronger affinities with the sandstones in the Lower Carboniferous series than with those in the coal-measures. The following is the best section I have been able to obtain of these rocks.

No. 1. 65 feet of a heavy bedded, irregularly stratified, brownish sandstone. It is made up of round and angular pellucid particles, having the interstices filled with a fine, opaque, brown substance, which is often replaced by oxides of iron and manganese.

No. 2. 90 feet of limestone, with thin, shaly strata between the beds. The upper beds are usually thin, light gray, coarsely crystalline, and brittle; but the lower beds are thick, more compact, and of a bluish gray color.

No. 3. 58 feet of dark blue shales, with gray and purple beds intercalated.

No. 4. 9 feet. Bluish gray, coarse grained, and thin bedded limestone, interstratified with blue shale.

No. 5. 20 feet. Sandstone. The upper is in thin beds, which are ripple-marked and fossiliferous. The lower part often exhibits well marked, irregular lines of deposition, but no distinct lines of stratification. This sandstone is sometimes replaced by blue and sandy shales.

No. 6. 80 feet. Bluish gray, crystalline limestone, interstratified with blue shales.
No. 7. 10 feet. Light gray, fine ground limestone.

These beds contain a species of *Fenestella* (Archimedes), with a strong axis like one found in northern and central Missouri, similar to *F. Worthenii*, of Hall.

In this formation, we have observed the following fossils: *Productus cora*, *P. elegans*, *Spirifer Leidyi*, *S. incrassatus* (?), *S. spinosus*, *S. lineatus* (?), *Spirigera hirsuta*, *Athyris subtilita*, *Atrypa serpentina*, *Orthis umbraculum* (?), *Fenestella lyra*, *F. Swallvana*, *F. Meekana*, *Pentremites pyriformis*, *P. sulcatus*, *Agassizocrinus dactyliformis*, and *Poteriocrinus occidentalis.*

The *Ferruginous Sandstone* is variable in its lithological characters. In some places it is very white and Saccharoidal; in others, fine, impure particles are disseminated through the mass, and the color becomes a dirty brown; and in a few localities, as near Fulton, Callaway county, it is a coarse conglomerate. But generally, where well developed, it is a coarse-grained, heavy-bedded, friable sandstone, colored with various shades of brown, red, and purple, as it appears in the bluffs near Salt Creek Sulphur Springs, some two miles west of Osceola; or clouded with yellow and red, as on Turkey creek, in Cedar county. The upper part is more regularly stratified and finer grained, contains more argillocalcareous matter, and has a light brown, yellowish gray, or cream color. It is very soft when quarried, and may then be dressed for building purposes; but exposure renders it much harder and more durable.

This sandstone contains large quantities of oxides of iron, brown and red hematites, which, in many places, form extensive beds of excellent ore. In Cooper county, this sandstone contains a good bed of ore, three feet thick. The same bed again shows itself in several places in Sec. 33, Town. 48, R. 19, and in various other places in the county. It was also observed, in large masses, on Grand river, in Henry county; in Sec. 28, Town. 39, R. 24, in St. Clair county; and in Bates and Hickory, and in still greater abundance, in the western part of Greene county.

The large quantities of iron in this sandstone have led me to give it the provisional name, *Ferruginous Sandstone.* It is found skirting the eastern border of the coal-measures, from the mouth of the Des Moines to McDonald county.

The *St. Louis Limestone* is made up of hard crystalline, and compact, gray and blue, somewhat cherty limestones, interstratified with thin partings of blue shale. Its stratigraphical position is between the Ferruginous sandstone and the Archimedes limestone, as seen near the Des Moines, and near the first tunnel on the Pacific railroad. It is found in Clark and Lewis counties, but attains its greatest development in St. Louis, from which the name is derived.

The most characteristic fossils yet described, are *Palaechinus multipora*, *Lithostrotion Canadense*, *Echinocrinus Nerci*, *Poteriocrinus longidactylus*, and *Atrypa lingulata*.

The *Archimedes Limestone.* In this formation are included the "*Arenaceous bed*," the "*Warsaw* on *Second Archimedes Limestone*," the "*Magnesian Limestone*," "*Geode bed*," and the "*Keokuk* or *Lower Archimedes Limestone*" of Prof. Hall's section, and the lead-bearing rocks of south-western Missouri, which, though different from any of the above beds, are more nearly allied to them than to the Encrinital limestone below. All of the above beds are easily recognized in Missouri, save, perhaps, the Warsaw limestone, which is but imperfectly represented in our north-eastern counties, where the "Keokuk limestone," the "Geode beds," and the "Magnesian limestone," are well developed.

The most characteristic fossils described, are *Fenestella Worthenii* (?), *F. Owenanæ*, *Agaricocrinus tuberosus*, *Actinocrinus Humboldtii*, *Spirifer incrassatus* (?).

This formation extends from the north-eastern part of the State, to the south-west, in an irregular zone, skirting the eastern border of Ferruginous sandstone. The extensive and rich lead deposits of south-western Missouri are in this formation. These mines occupy an area of more than one hundred square miles, in the counties of Jasper and Newton.

The *Encrinital Limestone* is at once the most extensive and best characterized of the divisions of the Carboniferous limestone. It is made up of brown, buff, gray and white, coarse, crystalline, heavy-bedded limestones. The darker colored, impure varieties, prevail near the base, while the lighter, and more purely calcareous, strata abound in the upper part. It everywhere contains globular, ovoid, and lenticular masses of chert, disseminated or arranged in beds, parallel to the lines of stratification. These masses of chert are more abundant in the upper beds; in fact, the upper beds are made up almost exclusively of this mineral. The strata of this formation are frequently intersected by joints, resembling the sutures of the cranium. The remains of corals and mollusks are very abundant; some of the strata are made up, almost entirely, of their exuviæ, especially of the joints and plates of *Crinoideans*. In the south-west, these strata rest upon some seventy or eighty feet of hard, porous, and thick-bedded silicious rock, which are included in this formation, as they have more affinities with it than with the Chemung below. There are nine divisions of this formation in Missouri, which are quite well marked by their fossils and lithological characters.

The Encrinital limestone extends from Marion county to Greene, forming an irregular zone on the east of the Archimedes beds.

Chemung.[*]

This group presents three formations, very distinct in lithological characters and fossil remains. They have received the following provisional names:—

CHOUTEAU LIMESTONE	85 feet.
VERMICULAR SANDSTONE AND SHALES . . . . ,	75 "
LITHOGRAPHIC LIMESTONE	125 "

The *Chouteau Limestone*, when fully developed, is made up of two very distinct divisions.

1. At the top, immediately under the Encrinital limestone, we find some forty or fifty feet of brownish gray, earthy, silico-magnesian limestone, in thick beds, which contain disseminated masses of white or limpid calcareous spar. This rock is very uniform in character, and contains but few fossils. Reticulated corals, and Fucoidal markings, like the *Cauda-galli*, are most abundant. In the quarry, it is quite soft, but becomes very hard on exposure, and forms a very firm and durable building rock. It is also hydraulic, and forms a good cement.

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[*] There is some difference of opinion respecting the system to which this group belongs; but if we make a division of the Missouri rocks into Devonian and Carboniferous, the line of separation, the most distinctly marked, is between the Encrinital and Chouteau limestones.

2. The upper division passes down into a fine, compact blue, or drab, thin-bedded limestone, whose strata are quite irregular and broken. Its fracture is conchoidal, and its structure, somewhat concretionary. Some of the beds are filled with a great profusion of most beautiful fossils. In many, the organic substance has been replaced by a calcareous spar. The most characteristic are *Spirifer Marionensis*, *Productus Murchisonianus*, *Chonetes ornata*, *Atrypagregaria*, *A. occidentalis*, *A. obscuroplicata*, *Leptaena depressa*, *Avicula Cooperenensis*, *Mytilus elongatus*, and several new species of *Trilobites*.

Chouteau Limestone has been applied to these rocks, as they were well developed at the Chouteau springs, in Cooper, where I first found large quantities of its new, beautiful, and characteristic fossils.

In the north-eastern part of the State, the Chouteau limestone is represented by a few feet of coarse, earthy, crystalline, calcareous rock, like the lower division of the Encrinital limestone, as there developed. There is, indeed, in that part of the State, no change of lithological characters as you pass from the Encrinital limestone to this formation; but the change in the organic remains is both sudden and great.

The Vermicular Sandstone and Shales. The upper part of this formation is usually a buff, or yellowish brown, fine-grained, pulverulent, argillo-calcareous sandstone. It is usually perforated in all directions with pores, filled with the same materials more highly colored, and less indurated. This portion, when exposed to atmospheric agencies, often disintegrates, and leaves the rock full of winding passages, as if it were worm-eaten. In the south-west, the harder part is much more silicious and indurated. The middle portion is a bluish-brown and gray silico-calcareous, magnesian shale. It has a conchoidal fracture, — the peculiar markings of the upper part, — together with those of a curious undescribed *Fucoid*. The lower part is usually a blue, sometimes brown, argillaceous shale, or fire-clay, in regular thin strata.

This formation contains but few fossils, and those are in the upper portions. *Spirifer Marionensis*, *Productus Murchisonianus*, *Chonetes ornata*, *Avicula circula*, the *Fucoids*, above named, and the *cauda-galli*, are most numerous. These beds can always be detected by the lithological characters and its peculiar *Fucoids*.

The *Lithographic Limestone* is a pure, fine, compact, even-textured silicious limestone, breaking rather easily, with a conchoidal fracture, into sharp, angular fragments. Its color varies from a light drab to the lighter shades of buff and blue. It gives a sharp, ringing sound under the hammer, from which it is called "pot-metal," in some parts of the State. It is regularly stratified in beds varying from two to sixteen inches in thickness, often presenting, in mural bluffs, all the regularity of masonry, as at Louisiana, on the Mississippi. The beds are intersected by numerous fractures, leaving surfaces covered with beautiful dendritic markings of oxide of iron. The strata are much thinner towards the top, where they often become silicious, and sometimes pass into an impure, thin-bedded, oolitic limestone, as in the bluffs, south-east of Elk Spring, in Pike county.

It has but few fossils. The most abundant are *Spirifer Marionensis*, *S. cuspidatus*, *Productus Murchisonianus*, *P. minutus*, *Proteus Missouriensis*, *Filictes gracilis*, a *Conularia*, *Fucoides cauda-galli* (?), and several large-chambered shells.

The Chemung rocks extend from Marion county to Greene, along the eastern border of the Carboniferous strata.

SYSTEM V. — DEVONIAN.

The Devonian system of Missouri contains the three following formations: — HAMILTON GROUP, ONONDAGA LIMESTONE, ORISKANY SANDSTONE.

The *Hamilton Group* is made up of some forty feet of blue shales, and 130 feet semi-crystalline limestone, containing *Dalmania calliteles*, *Phacops bufo*, *Spirifer mucronatus*, *S. sculptilis*, *S. congesta*, *Chonetes carinata*, *Favosites Cosaltica.*

Onondaga Limestone. This formation is usually a coarse gray, or buff, crystalline, thick-bedded, and cherty limestone, abounding in *Terebratula reticularis*, *Orthis resupinata*, *Chonetes nana*, *Productus subaculeatus*, *Spirifer euruteines* (Owen), *Phacops bufo*, *Cyathophyllum rugosum*, *Emmorisia hemispherica*, and a *Pentamerus*, like *galeatus.*

No formation in Missouri presents such variable and widely different lithological characters as the Onondaga. It is, generally, a coarse, gray, crystalline limestone; often, a somewhat compact, bluish concretionary limestone, containing cavities filled with green matter, or calcspar; in a few places, a white Saccharoidal sandstone; in two or three localities, a soft, brown sandstone, and, at Louisiana, a pure white oolite. Will those who would have us follow lithological characters exclusively tell us how we are to identify this formation, without its fossils, at these various localities?

The *Oriskany Sandstone* of Missouri is a light-gray *limestone,* which contains the *Spirifer arenosa, Leptæna depressa,* and several new species of *Spirifer, Chonetes, Illaenus, and Lichas.*

The Devonian rocks occupy a small area in Marion, Ralls, Pike, Callaway, Saline, and Perry counties.

SYSTEM VI. — SILURIAN.

Of the *Upper Silurian* series, we have the following formations: —

LOWER HELDERBERG	350 feet.
NIAGARA GROUP	75 "
CAPE GIRARDEAU LIMESTONE	60 "

The *Lower Helderberg Group* is made up of buff, gray, and reddish cherty, and argillaceous limestones, blue shales, and dark graptolite slates, *Dalmania tridentifera, Cheirurus Missouriensis, Encrinurus punctatus* (?), *Calymene rugosa, Orthis hybrida, O. elegantula,* and several species of *Platyostoma,* are the prevailing fossils.

*Niagara Group.** The upper part of this formation consists of red, yellow, and ash-colored shales, with compact limestones, variegated with bands and nodules of chert.

Halysites catenularia, Columnaria inequalis, Calymene Blumenbachii, and *Caryocrinus ornatus,* are the most characteristic fossils.

Cape Girardeau Limestone. I am also indebted to Dr. Shumard for a description of this formation.

"It is, according to him, a compact, bluish-gray, brittle limestone, with a smooth fracture, in layers from two to six inches in thickness, with thin argillaceous partings.

"These strata contain a great many fossils, principally Trilobites and Crinoids. In a small slab, not more than three by three inches, I have counted four genera of Trilobites, namely, *Cyphaspis Girardeauensis, Acidaspis Halli, Proteus depressus, Asaphus, Nov. sp.* None of the Trilobites have been before mentioned in this country, and, so far as I can ascertain, the species are distinct from European forms. According to Barande, the first three genera occur in the greatest number in the Upper Silurian period, and are very sparingly represented in the Lower Silurian groups. The Crinoids belong mostly to the genera Glyptocrinus, Homocrinus, and Tentaculites, and Palaeaster and the shells to Orthis, Leptaena, and Turbo, — all being of undescribed species."

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* I am indebted to Dr. Shumard for the information possessed respecting the Niagara and *Lower Helderberg Groups,* and the *Oriskany Sandstone,* as I have not examined those formations.

"These strata occur on the Mississippi river, about one mile and a half above Cape Girardeau. Thickness, forty to fifty feet."

LOWER SILURIAN.

We have thus far observed ten formations belonging to this series.

Hudson River Group	220 feet.
Trenton Limestone	360 "
Black River, and Birds-eye Limestone	75 "
1st Magnesian Limestone	200 "
Saccharoidal Sandstone	125 "
2d Magnesian Limestone	230 "
2d Sandstone	115 "
3d Magnesian Limestone	350 "
3d Sandstone	60 "
4th Magnesian Limestone	300 "

Hudson River Group.

There are three formations, which we have referred to this group.

1st. Immediately below the Oolite, of the Onondaga limestone, in the bluffs both above and below Louisiana, we find some forty feet of blue, gray and brown, argillaceous, magnesian limestone. The upper part of these shales is in thick beds, presenting a dull conchoidal fracture, and containing *Asaphus megistos*, and *Calymene senaria*.

The lower part of this division becomes more argillaceous, and has several thin beds of bluish-gray, crystalline limestone, intercalated, which contain many fossils of the following species: — *Leptaena sericea*, *L. alternata*, *L. planumbona*, *Orthis jugosa*, *O. subquadrata*, and *Rhynconella capax*.

There are, also, strata of calcareo-arenaceous slate, in the same position, filled with remains, which I am unable to distinguish from Prof.

Hall's *Palaeophycus virgatus*, and another contorted species, smaller than No. 2, pl. 70, of Prof. Hall's Report. There are, also, beds of slate, similar to those above mentioned, at the base of these shales, whose surfaces are covered with great numbers of the *Lingula ancyloidea.*

2d. On the Grassy, three and a half miles north-west of Louisiana, about sixty feet of blue and purple shales are exposed, below the beds above described. They contain three species of *Lingula*: *Lingula quadrata*, *L. fragilis*, and still another, not named.

The first resembles the *L. quadrata* of Hall, but is destitute of the "radiating striæ" of that species, and is larger; it is more like the variety from the Trenton limestone, than that from the Hudson river group.

3d. Under the 2d division are some twenty feet of argillo-magnesian limestone, similar to that in the 1st division, interstratified with blue shales. *Orthis subquadrata*, *O. jugosa*, *Leptaena alternata*, *Rhynconella capax*, and *Asaphus megistos*, are abundant.

These rocks crop out in Ralls, Pike, and Cape Girardeau counties. On the Grassy, a thickness of 120 feet is exposed; and they extend below the surface to an unknown depth.

TRENTON LIMESTONE.

The upper part of this formation is made up of thick beds of hard, compact, bluish gray and drab limestone, variegated with irregular cavities, filled with greenish materials; while the beds below are filled with irregular cylindrical portions, which readily decompose on exposure, and leave the rock perforated with numerous irregular passages, that somewhat resemble those made in timber by the *Teredo navalis.* The appearance of the rock, when thus decomposed, is very singular, and is a well-marked character of this part of the formation. The decomposed, honey-combed portions are most admirably adapted to ornamental rockwork, in gardens and yards. These beds are exposed on the plank road, from Hannibal to New London, north of Salt river, and are seventy-five feet thick. Below them are thick strata of impure, coarse, gray and buff, crystalline, magnesian limestone, with many brown earthy portions, which rapidly disintegrate on exposure to atmospheric influences. This part may be seen in the bluff of Salt river, near the plank road, 150 feet thick. The lower part is made up of hard blue and bluish gray, semi-compact silico-magnesian limestone, interstratified with light buff and drab, soft and earthy magnesian beds. Fifty feet of these strata crop out at the quarries south of the plank road bridge over Salt river, and on Spencer's creek, in Ralls county. The middle beds sometimes pass into a pure white crystalline marble of great beauty, as at Cape Girardeau.

Organic Remains. Fossils are abundant in all parts of the formation. *Leptaena deltoidea*, *L. sericea*, *L. alternata*, *Orthis pectinella*, *O. testudinaria*, *O. tricenaria*, *Rynconella capax*, *Murchisonia gracilis*, *M. bellicincta*, *Receptaculites sulcata*, and *Chaetetes lycoperdon*, are most common.

BLACK RIVER AND BIRDS-EYE LIMESTONE.

"They are bluish gray or dove-colored, compact, brittle limestones, with a smooth conchoidal fracture. The beds vary in thickness from a few inches to several feet." "Near the base, the rock is frequently traversed in all directions by vermicular cavities and cells."

Gonioceras anceps, *Ormoceras tenuifolium*, *Cythere sublevis*, are the most abundant fossils.

1ST MAGNESIAN LIMESTONE.

This formation is developed in many parts of the State. It is usually a gray or buff, crystalline, cherty, silico-magnesian limestone, filled with small, irregular masses of a soft white or greenish yellow, silicious substance, which rapidly decomposes when exposed, and leaves the rock full of irregular cavities, and covered with rough, projecting points. These rugged, weather-worn strata crop out in the prairies, and cap the picturesque bluffs of the Osage in Benton, and the neighboring counties.

These beds often pass into a homogeneous buff or gray crystalline magnesian limestone, which is frequently clouded with blue or pink, and would make a good fire-rock and building stone. At other places, the strata become compact, hard, and clouded, as above, forming a still more beautiful and durable marble.

Some of the upper beds are silicious, presenting a porous, semi-transparent, vitreous mass, in which are disseminated numerous small, globular, white, enamelled oolitic particles. They are sometimes in regular and continuous strata, at others in irregular mass, presenting mammillated and botryoidal and drusy forms of this beautiful mineral. In some parts of Benton and the neighboring counties, these masses, left by the denuded strata, literally cover the surface, and render the soil almost valueless for ordinary cultivation. Other strata abound in concretions, or organic forms, which resemble wooden button-moulds, with a central aperture and one convex surface. Masses of calcareous spar are quite abundant in the upper beds. But the lower part of this formation is made up of thin, regular strata, of a soft earthy, light drab or cream-colored silico-argillaceous magnesian limestone.*

Above the beds already described, we find, in several places in the State, a succession of hard, silicious, dark bluish gray semi-crystalline limestones, interstratified with grayish-drab earthy magnesian varieties, all in regular layers destitute of chert.' These strata have been joined to the 1st Magnesian limestone, with the expectation that they may prove distinct from the Calciferous sandrock and the 1st Magnesian limestone, and be identified with the Chazy limestone, or some other formation. *Straparollus laevata*, a small variety of *Cythere sublevis*, and a large *Orthoceras*, have been observed.

SACCHAROIDAL SANDSTONE.

This formation is usually a bed of white friable sandstone, slightly tinged with red and brown, which is made up of globular concretions and angular fragments of limpid quartz. It presents very imperfect strata, but somewhat more distinct lines of deposition, variously inclined to the planes of stratification.

This interesting formation has a wide range over the State. I have seen it in Ralls, Boone, Saline, Cooper, Moniteau, Pettis, Benton, Morgan, Hickory, St. Clair, Cedar, Polk, and Dallas; and Drs. Shumard and Litton observed it in Perry, St. Francois, Franklin, Ste. Genevieve, and other counties.

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* This variety of Magnesian limestone is generally called "Cotton Rock" in many parts of the State.

Its thickness is very variable, from one to 125 feet. At times it thickens very rapidly, so much so, as to increase thirty or forty feet in a few hundred yards. In a bluff about two miles north-west of Warsaw, is a very striking illustration of this change of thickness. This sandstone crops out along the bluff, between the 1st and 2d Magnesian limestone, and in a few yards decreases in thickness from twenty feet to one. Where thinnest, it is semi-vitreous, and the line of demarcation between it and the limestones is very distinct.

Near the same place is a locality where the sandstone thickens so rapidly as to present the appearance of a dyke, cutting off the strata of limestone above and below that formation. I have had specimens broken from the junction of this dyke-like mass with the wall of the adjacent limestone, which are half sandstone and half limestone, showing the two rocks firmly cemented together. On Bear creek, near Warsaw, as shown in the Second Annual Report of the Missouri Survey, at Hermann, and in many other places, are very striking instances of the dyke-like development of this rock; but I must admit that such a freak among sedimentary rocks I have never observed in any other formation. One might give a satisfactory reason for its penetrating the strata above; but by what process of nature it was made to cut off the beds below, is not so obvious. There is, perhaps, a possibility that, after the deposition of the 2d Magnesian limestone, the waters, which deposited the silicious matter of the sandstone, first cut a channel in the upper strata of the limestone. But future investigations may enable us to solve the difficulty more satisfactorily.

A very large *Orthoceras* is found in this sandstone.

2D MAGNESIAN LIMESTONE.

The lithological characters of this formation are very much like those of the 1st Magnesian limestone, above described. The following section from the bluffs of the Osage, above Warsaw, will give an idea of its general character:—

No. 1. 12 feet. 1st Magnesian limestone.
No. 2. 4 feet. Saccharoidal sandstone.
No. 3. 15 feet of soft, earthy, fine-grained, yellowish-white or drab

silico-magnesian limestone, with a conchoidal earthy fracture, in beds from half of an inch to one foot thick, interstratified with thin layers of bluish silico-argillaceous magnesian limestone. It is called "*Cotton Rock.*"

No. 4. 1 foot of coarse-grained crystalline greenish brown limestone, in thin laminæ. The crystals are as large as buck-shot, and give the rock a brecciated appearance.

No. 5. 8 feet of limestone, like No. 3, interstratified with chert.

No. 6. 10 feet of compact buff silicious limestone, filled with heavy spar and iron pyrites, some parts so variegated with flesh-colored spots as to present the appearance of a breccia—a beautiful and durable marble.

No. 7. 3 feet, coarse gray brown and buff crystalline magnesian limestone, filled with masses and veins of calcareous spar.

No. 8. 1 foot, like No. 3.

No. 9. 5 feet, like No. 7.

No. 10. 5 feet of hard, compact, gray silicious limestone, interstratified with chert and "Cotton Rock."

No. 11. 1 foot of yellowish-gray Saccharoidal sandstone.

No. 12. 4 feet, like No. 10.

No. 13. 10 feet, like No. 3.

No. 14. 5 feet, semi-oolitic sub-crystalline hard gray silicious limestone, interstratified with compact flesh-colored silicious beds.

No. 15. 6 feet of soft buff fine-grained magnesian limestone, interstratified with compact flesh-colored silicious limestone.

No. 16. 25 feet of coarse gray and buff silico-magnesian limestone, variegated by cavities filled with a white or yellowish pulverulent silicious substance, which decomposes on exposure, and leaves the rock porous. It is an excellent fire-rock.

No. 17. 4 feet, like No. 14.

No. 18. 10 feet, like No. 15. Strata undulating.

No. 19. 2 feet of fine, compact, flesh-colored silicious limestone.

No. 20. 8 feet of hard, gray crystalline semi-vitreous calcareous sandstone, with chert interspersed.

No. 21. 20 feet slope to water.

2D SANDSTONE.

This is usually a brown or yellowish-brown fine-grained sandstone, distinctly stratified in regular beds, varying from two to eighteen inches in thickness. The surfaces are often ripple-marked and micaceous. It is sometimes quite friable, though generally sufficiently indurated for building purposes. The upper part is often made up of thin strata of light, soft and porous, semi-pulverulent sandy chert or hornstone, whose cavities are usually lined with limpid crystals of quartz. Fragments of these strata are very abundant in the soil and on the ridges, where this sandstone forms the surface of the rock. It sometimes becomes a pure white fine-grained soft sandstone, as on Cedar creek, in Washington county, in Franklin, and other localities.

3D MAGNESIAN LIMESTONE.

This limestone is exposed in the high and picturesque bluffs of the Niangua, in the neighborhood of Bryce's spring, where the following strata were observed:—

No 1. 50 feet of the 2d Sandstone.
No. 2. 80 feet of gray and crystalline silico-magnesian limestone, somewhat clouded with flesh-colored spots and bluish bands. It is regularly stratified in thick beds, some of which have many cells filled with a white pulverulent silicious substance, while others are ferruginous and semi-oolitic. It contains very little chert.
No. 3. 50 feet of blue and white ferruginous chert, interstratified with hard, compact, and flesh-colored silicious limestone.
No. 4. 190 feet, like No. 2, save some beds are hard, compact, buff, or flesh-colored silicious limestone.
No. 5. 20 feet of light-drab fine-grained crystalline silico-magnesian limestone, often slightly tinged with peach-blossom, and beautifully clouded with darker spots and bands of the same hue or flesh color. It is distinctly stratified in beds of medium thickness.
No. 6. 50 feet, like No. 2.
No. 7. 30 feet of the *3d Sandstone* crops out lower down.

3D SANDSTONE.

This is a white Saccharoidal sandstone, made up of slightly cohering, transparent, globular, and angular particles of silex. It shows but little appearance of stratification, yet the well-marked lines of deposition, like those of a Missouri sand-bar, indicate its formation in moving water.

4TH MAGNESIAN LIMESTONE.

This presents more permanent and uniform lithological characters than any of the other Magnesian limestone. It is usually a grayish-buff, coarse-grained, crystalline magnesian limestone, containing a few cavities filled with less indurated silicious matter. Its thick uniform beds contain but little chert. The best exposures of this formation are on the Neangua and Osage rivers.

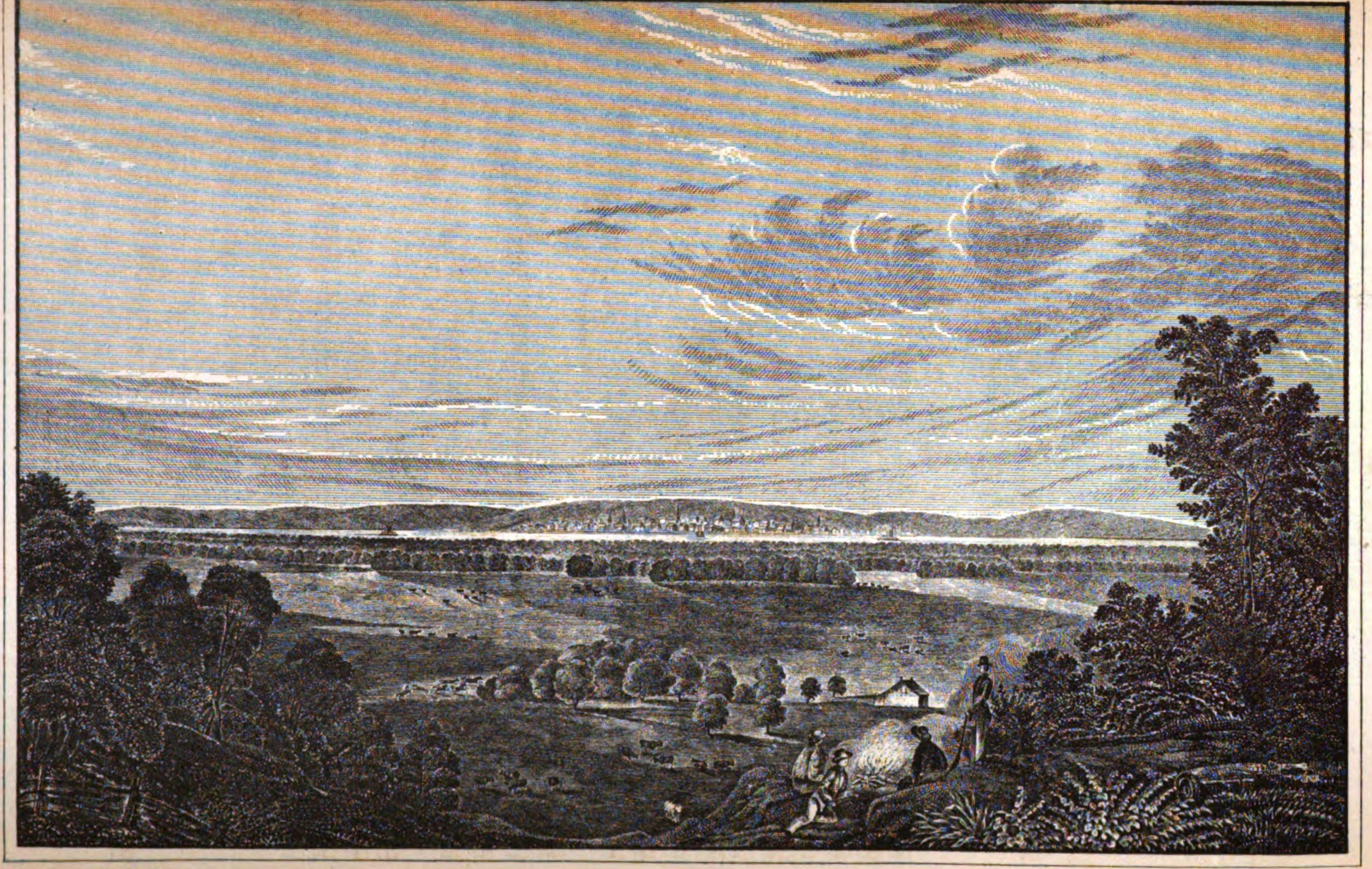
This *Magnesian Limestone Series* is very interesting, both in its scientific and economical relations. It covers a large portion of southern and south-eastern Missouri, is remarkable for its extensive caves and springs, and contains all the vast deposits of *lead, zinc, copper, cobalt, haematite, ores of iron,* and nearly all the marble beds of the State. They indeed contain a large part of all our mineral wealth.

The lower part of the 1st Magnesian limestone, the Saccharoidal sandstone, the 2d Magnesian limestone, the 2d Sandstone, and the upper part of the 3d Magnesian limestone belong, without doubt, to the age of the Calciferous sand-rock; but the remainder of the series may prove to be Potsdam sandstone.

Igneous Rocks.

There are a series of rounded knobs and hills in St. Francois, Iron, Dent, and the neighboring counties, which are principally made up of *granite, porphyry,* and *greenstone.* These igneous rocks contain those wonderful beds of *Specular Iron,* of which *Iron Mountain* and *Pilot Knob* are samples.

These mountains of iron and igneous rocks are older than the oldest of the stratified rocks above described; as the beds of the latter rest against the sides of the former, without exhibiting signs of any considerable disturbance.



Drawn by R. B. Price

View of Quincy and the Mississippi bottom, NOTE. I am indebted to Maj. Hawn, of the Linear Survey of Kansas, for all the geological facts on the accompanying map of Kansas, save what was derived from my own observations along the eastern border. The scientific world will be greatly indebted to Maj. Hawn for his diligence in collecting and recording geological data, while engaged in his survey.

2. QUATERNARY DEPOSITS OF MISSOURI. By Prof. G. C. SWALLOW, State Geologist of Missouri.

PREVIOUS to the commencement of the Geological Survey of Missouri, but little effort had been made to trace out and classify the various deposits of the Quaternary system of the Mississippi valley. This fact, and the vast importance of these formations, both in our scientific and economical geology, have led us to undertake a careful investigation of this system, as developed in our State. The results of our early investigations were given in the second Annual Report of the Missouri Survey, in 1854. This paper will include the facts there recorded, and those observed in our subsequent examinations, that it may present a full view of the present state of our knowledge upon this subject.

When it is remembered that these formations contain the entire geological record of all the cycles from the end of the Tertiary period to the present time, and that their economical value is greater than that of all the other formations combined, I shall need no apology for entering somewhat into details in recording the phenomena they present.

The *Quaternary System* comprises the drift and all the deposits above it — all the strata included in the Alluvium and Diluvium of former authors. There are, within this period, four distinct and well-marked formations in this State, which we have thus named in the order of their stratigraphical position: —[*]

ALLUVIUM. BLUFF.
BOTTOM PRAIRIE. DRIFT.

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[*] See the general section of the rocks of Missouri accompanying my paper on that subject.

All of the latest deposits — all that have been formed since the present order of things commenced upon our continent — are included in the

ALLUVIUM.

All the deposits observed in the State, belonging to this formation, are: —

1st. *Soils.*
2d. *Pebbles and Sand.*
3d. *Clays.*
4th. *Vegetable Mould, or Humus.*
5th. *Bog Iron Ore.*
6th. *Calcareous Tufa.*
7th. *Stalactites and Stalagmites.*

1st. *Soils* are a well-known mixture of various comminuted and decomposed mineral substances, combined and mingled with decayed vegetable and animal remains, all comprising those ingredients peculiarly adapted to the nourishment of the vegetable kingdom. They are formed by the action of water, particularly in the form of rain and dews, cold, heat, and other atmospheric influences, together with the co-operation of the vegetable and animal kingdoms.

The process by which soils are formed is one of the most beautiful and wonderful in nature. By a careful examination of what is transpiring in this great laboratory of nature, we may easily detect that process. If a rock, fresh from the quarry, be exposed, its surface will soon present a dull, earthy appearance, which is caused by a disintegration of its surface by atmospheric influences. Fine particles have been separated from the mass, and this meagre coating of decomposing mineral matter will soon become the resting-place of numerous microscopic germs, which will be developed into a minute growth of lichens. These, in turn, will decay, and add their remains to the pulverized particles, and prepare them to sustain a more vigorous growth of herbs, and to become the abode of the small insects and worms, which will burrow in their recesses, feed upon the increasing vegetation, and swell the mass, both by their mechanical agency, and by adding their exuviæ to the accumulating soil. Larger plants and animals will accelerate the process by their more powerful agencies, and by the greater amount contributed by their decaying remains. Thus, by almost imperceptible increments, our rich, deep soils have been accumulated.

But the soils of Missouri are made up by the mingling of organic matter with the comminuted marls, clays, and sands of the Quaternary deposits, which cover all parts of the State with a vast abundance of the very best materials for their rapid formation. Hence the soils of the State are very deep and wonderfully productive, save in those limited localities where the materials of the Quaternary strata are unusually coarse, or entirely wanting. But I shall speak more particularly of the soils while treating of the formations on which they are formed.

2d. *Pebbles and Sand.* Many of our streams abound in water-worn pebbles, which constitute their beds, and form bars along their margins and across their channels. These pebbles were derived from the drift and the harder portions of the adjacent rocks. They vary in size according to the transporting power of the streams in which they are found.

The economical value of these pebbles for roads and streets, and the obstruction they often present to navigation, as in the Osage, give them unusual importance in our geology. The Osage, Gasconade, Niangua, Marais des Cygnes, Sac, and Spring rivers, of the south; and the Salt, South, North, Fabins, and Charitan, of the north, all furnish good and abundant examples of these deposits, which have been formed by the action of those streams.

Sand is the most abundant material in the alluvial bottoms of the great rivers in the State. Vast quantities of it are constantly borne along by the irresistible current of the Missouri. Its whirling, rolling, turbulent waters form of it extensive bars in incredibly short periods, which they again wear away, often still more rapidly than they were formed.

These sand-bars, so common in this stream, frequently extend along its bed several miles, with a breadth varying from one to five or six furlongs, and limited in thickness only by the depth of the water. A slight fall in the river leaves these vast sand-beds dry, when their surfaces are soon covered by a growth of weeds, interspersed with young willows and cotton-wood. The fickle stream, however, seldom leaves these sand-beds to a long repose, but returns to its old channel by a rapid removal of their loose materials.

A disaster of the ill-fated steamer, *Timour No.* 2, presents a good illustration of the rapidity with which the Missouri forms and destroys these extensive deposits of sand. In the fall of 1853, this steamer ran upon a sand-bar, and was soon left high and dry, some seventy-five or one hundred yards from the water, with a fair prospect of leaving her timbers to decay in the young forest of willows and cotton-wood, which would soon spring up around her.* But the current changed, and cut its way through the sandy stratum upon which the boat rested, and floated her away, uninjured, to the great city of the West. As these sand-bars are cut away, their perpendicular faces present beautiful illustrations of their stratification, which is usually very irregular and complicated, as might be expected from the changeable character of the current.

Fig. 2, sketched from a sand-bar in the Missouri river, two miles above Wayne City, presents a good example of their stratification. But water is not the only agent engaged in producing the irregular stratification of the sand-bars of the Missouri and Mississippi rivers. When these sand-bars become dry by exposure, the winds easily transport and rearrange their light and fine materials. Such quantities are moved by high winds, that the entire channels of the rivers are obscured by the dense clouds of moving sand. The stratification of the sand-beds thus formed is very interesting and complicated, and aids us in explaining some examples of stratification observed in the older rocks.

At high stages of water, both the Missouri and Mississippi overflow their low bottoms, and leave deposits of a grayish-brown, or a grayish-yellow sand, similar to that in the sand-bars mentioned above. The thickness of these beds depends upon the height and continuance of the overflowing waters, varying from a mere perceptible stratum to several feet.

That from the flood of 1844 is very conspicuous, throughout the length of the Missouri bottom. It is sometimes six or eight feet thick, particularly in low bottoms, so heavily timbered as to obstruct the current.

At the lower end of the Waconda prairie, this deposit is very evenly distributed over its surface; but it increases in thickness as the prairie descends to the lower timbered bottoms, lower down the stream, where

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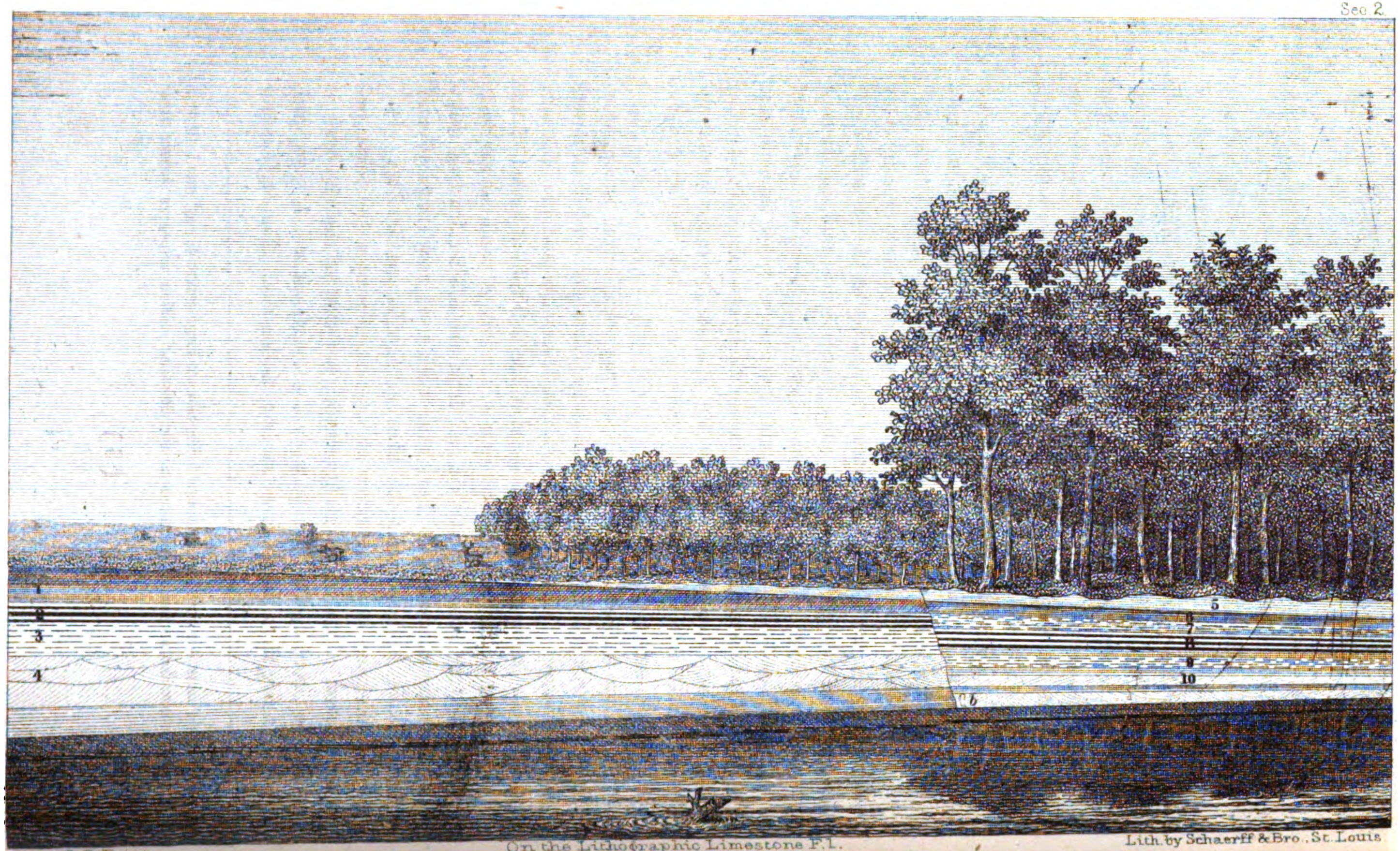
* The sand of the Missouri, usually grayish-brown and fine-grained, contains a considerable quantity of lime and clay and vegetable matter, which render it very productive.

Sec. 2.



Dr. by R. B. Price

On the Lithographic Limestone F. I.

Lith. by Schaerff & Bro., St. Louis

it is six or seven feet, and its surface becomes very irregular, like the surface of a lake, when disturbed by a high wind, or a *chopped sea.*

Sect. 2, showing the lower extremity of Waconda prairie and the cotton-wood bottom below, finely illustrates these phenomena. Nos. 6, 7, 8, 9, and 10 represent older strata, and No. 5, that left by the flood of 1844. The small timber in the middle is a young growth of cotton-wood, which has sprung up subsequent to that event, and the larger trees just below are the older growth, which obstructed the waters flowing through the bottom, and thus caused the more abundant and irregular deposit there observed.

Similar phenomena are exhibited in the bottoms opposite St. Charles and Jefferson City, and at many other places on the Missouri, and several localities in New Madrid and Pemiscot counties, on the Mississippi.

These sands were doubtless derived from those extensive sand-stone formations on the Platte,* and other tributaries of the Missouri. It is nearly all silex, but contains enough calcareous and argillaceous matter to render it fertile, as is abundantly proved by the growth of weeds and willows, cotton-wood and sycamores, which immediately spring up on these sand-bars whenever they are exposed above the water. There are many points on the Missouri, as in the bottom opposite St. Charles, where a thrifty growth of young timber may be seen on the sand deposits of 1844.

3d. *Clays.* These are dark, bluish-gray, argillaceous strata, rendered more or less impure by fine silicious, calcareous, and decomposed organic matter. When the floods of the Mississippi and the Missouri subside, the lagoons, sloughs, and lakes are left full of turbid water. The coarser materials soon settle into a stratum of sand, but the finer particles more gradually subside, and form the silico-calcareous clays of their alluvial bottoms. Thus, after each flood, new strata of sand and clay are deposited, until the lakes and sloughs are silted up.

The thickness of each stratum of sand depends upon the height and continuation of the flood which forms it; but that of the clay-beds is governed more by the time between the overflows, and is very variable, ranging from the tenth of an inch to ten feet. The argillaceous materials which formed them were doubtless derived from the Cretaceous and Tertiary clays of the upper Missouri and its tributaries, whence, as from the *Mauvaises Terres*, such vast quantities of a simlar material have been removed by denudation.

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* The Platte is a rapid stream, and brings down large quantities of sand, though its waters are not so turbid as those of the Missouri, either above or below their junction.

4th. *Vegetable Mould or Humus* is dark-brown or black deposit of decayed vegetable matter, containing variable, though small, quantities of fine silicious and argillaceous particles. When wet, it is very soft and plastic, and quite black; but when dry, it separates into angular cuboidal fragments, which readily crumble into a dark, very light, impalpable powder. In these beds of almost homogeneous humus, leaves and stems of trees are sometimes found in a tolerable state of preservation.* The process by which these strata of humus are deposited is very obvious. When the lakes and sloughs of these bottoms are so far filled up as to sustain vegetable life, the decay of the annual growth, and of the foreign matter which falls or floats into these waters, forms a stratum of humus at the bottom, over the beds of clay and sand, previously deposited by the floods and still waters. Another overflow gives another succession of sand and clay; and the succeeding annual crop of vegetable matter, another stratum of humus.

These changes have often continued until several series of these deposits were formed; but when the bottoms of those bodies of water had been thus raised so high above the river, that the floods less frequently flowed into them, the deposits of sand diminished, and the long, quiet intervals favored the deposition of clay and humus. In time, these shallow waters became mere marshes, where a rank vegetation rapidly formed thick beds of vegetable mould, for the support of the magnificent forests which now occupy the sites of those ancient lakes and sloughs.

Such is the process by which the succession of sands, clays, and humus, in those alluvial bottoms, has been deposited. Whence it is easy to see why the sands are most abundant at the bottom, when the waters from the river floods would more frequently overflow them; the clays in the middle, when the waters would be rarely disturbed by overflows; and the humus or vegetable mould at the top, when a rank vegetation prevailed and inundations were rare.

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* Do these preserved leaves and stems prove that these beds of humus were not derived from decayed vegetables?

Sections 3 and 4 of our second annual report are good illustrations of the manner in which these strata of sand, clay, and vegetable mould succeed each other in the alluvial bottoms of our two great rivers. Such is the structure of the vast alluvial plains bordering the Missouri and Mississippi rivers.

The bottom of the Missouri, extending from the Iowa line to its mouth, is about seven hundred miles long, and five broad, presenting an area of 3,500 square miles; more than one half of this, say 2,000 square miles, may be set down as alluvium, while the river, "bottom-prairies," and lakes, occupy the remainder.

On the Missouri side of the Mississippi bottom, there are about 4,300 square miles of alluvial bottom of a similar character. Thus the alluvial bottoms of our two great rivers alone, give more than 4,000,000 acres of land based upon these strata of sands, clays, marls, and humus. And beside, the quantity is constantly increasing by the silting up of the sloughs and lakes, as above described.

The soil formed upon these alluvial beds is deep, light, and rich, almost beyond comparison, as is abundantly proved by the immense burden of timber * growing upon it, and by the unparalleled crops of hemp, tobacco, and corn, harvested from its cultivated fields.

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* In the fall of 1856, our surveying party measured several trees in south-eastern Missouri. The following measurements were obtained from some of the largest trees of those species: —

In Stoddard Co., a Beech, *Fagus ferruginea*, 18 feet circumference, and 100 high.

In Stoddard Co., a Tupelo Gum, *Nyssa grandidentata*, 30 feet circumference, and 120 high.

In Dunklin Co., a Catalpa, *Catalpa bignonioides*, Gray, 10 feet circumference, and 90 high.

In Pemiscat Co., an Elm, *Ulmus Americana*, 22 feet circumference, and 100 high.

In New Madrid Co., a Cypress, *Taxodium distichum*, 29 feet circumference, and 125 high.

In Cape Girardeau Co., a Sweet Gum, *Liquidambar Styraciflua*, 15 feet circumference, and 130 high.

In Cape Girardeau Co., a White Ash, *Fraxinus Americana*, 18 feet circumference, and 110 high.

In Mississippi Co., a Spanish Oak, *Quercus falcata*, 28 feet circumference, and 100 high.

In Mississippi Co., a Sycamore, *Platanus occidentalis*, 43 feet in circumference. This sycamore was hollow, and the cavity measured 15½ feet in one diameter, and 13 feet in the other.

5th. *Bog-Iron Ore* is deposited from several chalybeate springs. Large quantities of the hydrated oxide have accumulated near a fine spring, some two miles west of Oseola, and a small amount from another near Sharpsburg, in Marion county. But the most important deposits of bog-iron are situated in the cypress-swamps, and other low wet portions of south-eastern Missouri. Extensive beds were traced out in lake St. John, extending from the "Stark Glades," in Scott county, to the "Iron-Ore Ford," in N. Madrid. Several large beds were observed in the south-western part of Dunklin county, and two in the western part of Mississippi. These beds vary in thickness from one or two inches to two feet, while some of them are several miles in length. The quality of the ore is good, and the quantity very great,—almost fabulous.

6th. *Calcareous Tufa* has been found in several places in the State. In a ravine south of Parkville is a mass in which many specimens of moss are well preserved; and another similar deposit was observed under the bluffs near Bryce's spring, on the Niangua.

7th. *Stalactites and Stalagmites* are abundant in some parts of the State. Many beautiful specimens were observed in the extensive caves of Boone, Camden, and Greene counties.

Such are the alluvial deposits, so far as observed, in Missouri. Future investigations may bring to light others belonging to this formation.

Range and Thickness. Our alluvium is, as a matter of course, diffused throughout the entire State, as it comprises all the soils and other deposits now forming. It is, however, much more abundant in the valleys of our great streams. The thickness is often thirty or forty feet, though usually much less.

BOTTOM PRAIRIE.

This important formation, in many respects, resembles that of the *alluvial bottoms* above described, with which it has usually been confounded by geologists; though agriculturalists have made a distinction.

There are, however, important differences:—

1st. The stratification in the prairie is much more uniform, and more regularly extended over wide areas.

2d. In the prairie formation, the strata are not so distinct, nor are they so purely silicious or argillaceous.

3d. It was evidently formed by agencies operating over the entire bottoms, whose action was more uniform and quiet, and continued uninterrupted through longer periods, than those now forming the alluvial deposits in the same bottoms.

4th. Where these two formations meet, one can usually trace out the line of demarcation. Either the strata of the prairie pass under those of the alluvium, or are cut off and replaced by them. Instances of both these changes may be observed at the lower end of Waconda prairie, as shown in sect. 2. The upper stratum of the prairie, No. 1, passes under No. 5, of the alluvial bottom, and continues in that position several hundred yards; while Nos. 1, 2, 3, and 4 of the former are cut off at *b*, and are replaced by Nos. 6, 7, 8, 9, and 10 of the latter.

5th. The alluvial bottom is continually increased at the expense of the prairie, through the action of the rivers. The current is constantly cutting away the prairie, forming new channels, and filling up the old ones with drift and silt. This explains the fact that the strata of the prairie are frequently cut off, and others quite different set in, as we pass from it to the timbered bottoms, as illustrated in sect. 2. The part of that section under the large timber was once the channel of the river, and has been filled up by the process explained above, in describing the last formation. At high stages of water the lower portions of the prairie are overflowed, and deposits of sand are left on its surface, which are soon covered with willows, sycamores, or cottonwood, as is shown by the growth which has sprung up on the deposits from the flood of 1844.

6th. No causes now in operation could, at the present level of the country, produce a formation of such extent and uniform structure as the bottom prairies.*

Such are some of the facts which have convinced me that this is an older formation, and one entirely distinct from the alluvial bottoms. Several facts show it to be distinct from, and newer than, the bluffs or loess. Its composition, structure, and position are entirely different, and in many places the bottom prairie rests non-conformably upon the bluff, as at St. Joseph, and the mouth of the Big Nemaha.

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* Some of the bottom prairies of the Missouri are, at least, thirty miles long, and from ten to twenty feet broad, as the Huppan-Kuty of Nicolet, above the mouth of the Sioux river, and the Waconda, in Carrol county. And these are probably only fragments of one which was once continuous from the former to the mouth of the Missouri.

This formation, like the last, is made up of sands, clays, vegetable mould, variously interstratified.

The sand in the upper part is fine and yellowish-brown, like that of the Missouri sand-bars; but the lower beds are more purely silicious.

The clays are usually dark, bluish-brown, and marly, with more or less sand and humus intermingled.

The humus or vegetable mould has a brownish or black color; when wet, it is somewhat plastic, and slightly tenacious; when dry, it is brittle, and breaks into angular fragments, and can be easily reduced to an impalpable powder. These beds of humus were evidently formed by the growth and decay of plants in the localities where they are found.

Range and Thickness. This formation is confined to the bottoms of the Missouri and Mississippi rivers, and is more abundant and better characterized on the former. The bottom prairie is about half as extensive as the alluvial bottoms above described, and sustains a soil of equal fertility. This estimate will give us about 2,000,000 acres of these vastly rich savannas, all prepared by nature for the plough. Their agricultural capacities are scarcely inferior to any lands in the world, as is abundantly demonstrated by the mineral contents of the strata, and the products of the numerous flourishing farms located upon them.

The Organic Remains of the bottom prairie, are numerous and well preserved. All the shells of the bluff, save the *Helicina occulta*, have been found in it; but no remains of the elephant or mastodon have yet been detected. We have collected many species of trees, shrubs, and vines from these beds.

The character and position of the strata forming the bottom prairie show most conclusively, that the level of the country where they were deposited was somewhat different from its present condition, that bodies of almost still water covered the present valleys of our great rivers, and that the formation was coextensive with these river bottoms; and that subsequently such a change of level occurred, as gave the present rapid current to the waters passing through these valleys. The rapid waters cut channels in their soft beds, and left broad level areas dry, and subject to vegetable life. These "Bottom Prairies" extended from the mouth of the Sioux to the Mississippi, and probably from the St. Peter's to the Arkansas. Since that period, the rivers have been ever busy wearing away the bottom prairie and depositing the alluvial bottoms above described, until we have but few remnants, such as the Waconda and Hupan-Kuty, of the vast bottom prairie which once occupied these great valleys.

These beautiful savannas are almost universally called "*Bottom Prairie*," and I have proposed that as the geological name of the interesting formation on which they rest. The scenery of the alluvial bottom and the bottom prairie is well represented in Sections 2 and Plate 12.

BLUFF.

This formation rests upon the drift, as is obvious whenever the two formations are well developed. In many places, as at St. Joseph and at the mouth of the Big Nemeha, it is seen dipping beneath the beds of the bottom prairie. The bluff formation rests upon the highest ridges and river bluffs, and descends along their slopes to the lowest valleys. The bottom prairie is confined to the river bottoms, and was deposited in horizontal beds between the bluffs. Thus, while the bottom prairie occupies a higher geological horizon, the bluff is usually several hundred feet above it in the topographical.

This formation, when well developed, usually presents a fine, pulverulent, obsoletely stratified mass of light grayish buff, silicious and slightly indurated marl. Its color is usually variegated with deeper brown stains of oxide of iron. The bluff above St. Joseph exhibits an exposure of it 140 feet thick, presenting its usual characteristic features. When but sparingly developed, it generally becomes more argillaceous, and assumes a deeper brown or red color, as on the railroad south of Palmyra, where it is a dark brick-red, tinged with purple. In some places, the ferruginous and calcareous matter increases, and we find concretions of marl and iron-stone, either disseminated through or arranged in horizontal belts. At other places, it has more arenaceous matter, and is much more decidedly stratified, as at a point one mile above Wellington, and in the bluff at St. Joseph.

These are the only places seen where the stratification assumed the irregular appearance so often presented by sand-bars. It is barely possible that this stratified sand is a portion of *altered drift;* but the beds between it and the drift, having the usual appearance of the bluff, militate against such a supposition.

The bluff formation is often penetrated by numerous tubes or cylinders, about the size or thickness of pipe stems, some larger and others smaller. They are composed of clay, carbonate of lime, and oxide of iron, being argillo-calcareous oxide of iron, or calcareous clay-ironstone. But it is not so easy to say how they were formed. Several facts may aid us in determining this matter. These tubes penetrate the formation in all directions, and are most abundant near the surface, though some extend to the depth of twenty feet. The space for some half inch around each tube, more or less according to its size, is of a much lighter color, as if the coloring matter (oxide of iron) had been extracted.

The same appearances were observed around the green and dry roots of the white oak (Quercus alba), which had penetrated the same formation. Qualitative analysis proved these same roots to contain a large portion of oxide of iron. And besides, oak-wood always contains a large portion of that metal and manganese. An analysis of its ashes by Saussure gave 2.25 per cent. of the oxides of these metals, while the analysis of "oak-wood mould," or the decayed wood, by the same chemist, gave 14 per cent. of the same oxides.

It is thus made manifest that oak-wood contains iron, which must have been absorbed through the roots from the earth. This fact readily explains the loss of the iron from the marl around the roots, and around the tubes, provided they were once oak-roots. But the question naturally arises, how these roots became tubular. They were seen in the various stages of decay, and the woolly fibres of some had disappeared and left the bark in the form of a tube, still retaining its organic structure, though strongly impregnated with the oxide of iron and aluminum and carbonate of lime.

It may also be objected, that the roots of the oak do not penetrate to such depths; but, in the language of a poet and botanist,

> "Aesculus in primis; quae quantum vertice ad auras,
> Aetheras, tantum radice in Tartara tendit."

These facts have led to the conclusion, that these tubes of calcareous clay-ironstone are decayed roots of oak or other plants. In some localities, small holes, also, without any tubes of different material, penetrate this formation in great numbers, and are probably caused by similar agencies.

These phenomena have been thus minutely investigated, not merely as interesting scientific facts, but, also, as one of the most useful agricultural features of this preëminently valuable formation; for upon it, and sustained by its absolutely inexhaustible fertilizing resources, rest the very best farms of the Mississippi and Missouri valleys. These tubes and holes, also, constitute the *most thorough system of drainage* imaginable.

Range and Thickness. So far as my own observations extend, this formation caps all the bluffs of the Missouri, from Council Bluffs to its mouth, and those of the Mississippi, from the mouth of the Des Moines to that of the Ohio, and forms the upper stratum beneath the soil of all the highlands, both timber and prairie, of all the counties north of the Osage and Missouri, and also St. Louis, and the other Mississippi counties on the south.

According to Mr. Meek, its western or north-western limit is probably a few miles below Fort Pierre; Lyell traces a similar formation up the Ohio and further down the Mississippi; Dr. Owen mentions its existence on the Wabash river; and Dr. G. G. Shumard saw similar deposits on Red river.* The identity of the deposits at Council Bluffs, St. Joseph, Lexington, Booneville, Hannibal, St. Louis, and at Cape Girardeau, is placed beyond all doubt by the following facts:—

1st. They occupy the same geological position.

2d. They have the same topographical position on the tops of the bluffs.

3d. They present the same lithological and chemical characters.

4th. Nearly all the fossils are found at all those places, save, perhaps, the last.

5th. These localities are connected by an unbroken continuity of the same deposit.

Its greatest development in this State is in the counties on the Missouri, from the Iowa line to Boonville; but thence to St. Louis, it is not so thick. In some places, it is two hundred feet thick. At St. Joseph, it is one hundred and forty; at Boonville, one hundred; and at St. Louis, in St. George's quarry, and the Big Mound, it is about fifty feet; while its greatest thickness observed in Marion county was only thirty.

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* Capt. Marcy's Report on Red river of Louisiana, p. 182.

Organic Remains. The fossils of the Bluff are very numerous and interesting. Those which I have had time to determine are in the following catalogue: —

Catalogue of Bluff Fossils.*

No. 1.	*Cyclas*, Species undt.	Near mouth of Wolf river.
No. 2.	*Amnicola lapidaria*, Say,	Near St. Louis.
No. 3.	*Helix rufa*, De Kay,	St. Joseph Landing.
No. 4.	*Helix albolabris*, Say,	Half mile below Great Nemaha.
No. 5.	*Helix alternata*, Say,	Bellevue, Bluff City Landing, mouth Wolf river, and Lexington.
No. 6.	*Helix concava*, Say,	Bluff City Landing, and near St. Louis.
No. 7.	*Helix thyroidus*, Say,	Bluff City Landing.
No. 8.	*Helix profunda*, Say,	Lexington, mouth Little Nemaha, Bluff City Landing, etc.
No. 9.	*Helix multilineata*, Say,	Near mouth Big Nemaha, Little Nemaha, and Platte river.
No. 10.	*Helix clausa ?* Say,	
No. 11.	*Helix striatella*, Anthony,	St. Louis, Boonville, below mouth Platte river and Big Nemaha.
No. 12.	*Helix monodon*, Rackett,	St. Louis.
No. 13.	*Helix electrina*, Gould,	St. Louis and Boonville.
No. 14.	*Helix arborea*, Say,	St. Louis.
No. 15.	*Helix indentata*, Say,	Below mouth Platte river.
No. 16.	*Helix hirsuta*, Say,	Bluff near St. Louis.
No. 17.	*Helix lineata*, Say,	Bluff City Landing, below mouth Platte, near mouth Big Nemaha.
No. 18.	*Helix minuta*, Say,	Bluff City Landing, below mouth Platte, Boonville, etc.
No. 19.	*Helix labyrinthica*, Say,	St. Louis.
No. 20.	*Helicina occulata*, Say,	Boonville and near St. Louis.
No. 21.	*Limnea fragilis*, Lin,	Bluff City Landing, below mouth Platte, Bellevue and Lexington.
No. 22.	*Limnea reflexa*, Say,	Bluff City Landing.
No. 23.	*Limnea umbrosa ?* Say,	Near mouth Great Nemaha.
No. 24.	*Limnea* — 5 or 6 sp.,	Bluff City Landing, mouth Wolf river, below mouth Platte, etc.

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* I am indebted to Mr. Meek for the arrangement of this catalogue.

No. 25.	*Physa plicata*, De Kay,	Bluff City Landing, near mouth Wolf river.
No. 26.	*Physa heterostropha*, Say,	Below mouth Platte and mouth Little Nemaha.
No. 27.	*Physa elongata*, Say,	Below mouth of Platte river.
No. 28.	*Physa gyrina*, Say,	Bluff City Landing and below mouth Platte.
No. 29.	*Physa* — Several undt. sp.,	Most of the above localities.
No. 30.	*Planorbis trivolvis*, Say,	Bluff City Landing, below mouth Platte, and at Big Nemaha, etc.
No. 31.	*Planorbis trivolvis*, (var.,)	Same as above.
No. 32.	*Planorbis armigerus*, Say,	Mouth Wolf river, and below mouth Platte.
No. 33.	*Planorbis* (undet.),	Below mouth of Platte river.
No. 34.	*Pupa armifera*, Say,	Bluff near St. Louis, Bluff City Landing, mouth Platte, etc.
No. 35.	*Pupa* (undet.),	Near St. Louis.
No. 36.	*Succinea obliqua*, Say,	Bluff City Landing, and below mouth of Platte river.
No. 37.	*Succinea campestris*, Say,	Bluff City Landing.
No. 38.	*Succinea ovalis?* Say,	Below mouth of Platte, Bluff City Landing, etc.
No. 39.	*Succinea* — 3 or 4 undet. sp.,	Same localities as above.
No. 40.	*Valvata tricarinata*, Say,	Bluff City Landing.
		PLANTS.
No. 41.	*Seeds of Lithospermum*,	Nine miles below Bethlehem.
		MAMMALIA.
No. 42.	*Castor Fiber-Americana*,	Near mouth Big Nemaha.
No. 43.	*Elephas primigenius*,	Bonne Femme Creek, Boone county.
No. 44.	*Mastodon giganteus*,	St. Louis.
No. 45.	*Molar of Ruminant*,	Near mouth Big Nemaha.
No. 46.	*Incisors of small Rodent*,	Near mouth Big Nemaha and mouth Wolf river.

I have collected from it, of the Mammalia, two teeth of the *Elephas primigenius*, the jawbone of the *Castor fiber Americana*, the molar of a *Ruminant*, and the incisor of a *Rodent;* of the Mollusca, seventeen species of the genus *Helix*, eight *Limnea*, eight *Physa*, three *Pupa*, four *Planorbis*, six *Succinea*, and one each of the genera *Valvata*, *Amnicola*, *Helicina*, and *Cyclas*, besides some others not determined.

These *lacustrine*, *fluviatile*, *amphibious*, and *land* species, indicate a deposit formed in a fresh-water lake, surrounded by land and fed by rivers. These facts carry back the mind to a time when a large portion of this great valley was covered by a vast lake, into which, from the surrounding land, flowed various rivers and smaller streams. We see the waters peopled with numerous mollusks, the industrious beaver building his habitation, the nimble squirrel, the fleet deer, the sedate elephant and huge mastodon, lords of the soil. There must have been land to sustain the elephant and mastodon and helices; fresh water and land for the beaver, and fresh water for the cyclas and limnea.

Some have supposed this formation was deposited by the rivers when their waters were at a higher stage. If it was deposited by the rivers, their waters were high enough to cover nearly all this and a large part of the adjoining States and territories, and quiet enough to be the abode of linneas, and to be called a lake.

I have proposed the title *Bluff* formation for this deposit, as it forms a large portion of, and gives the peculiar characters to, the bluffs so conspicuous and unique in the scenery about Council Bluffs and other portions of the Missouri valley, and as it forms the tops of the bluffs wherever it is developed.

Loess, the name of a similar formation on the Rhine, has been given to this by some geologists. But this would imply that these two formations are identical, when they may or may not be, so far as any proof has been given. It is true they are both fresh-water deposits, both have recent shells of the same genera, and in lithological and chemical characters, they are somewhat similar. But there are other deposits whose fauna and lithological and chemical properties are quite as similar to the bluff, and some of them more so, and yet they are more recent.

There is just as much evidence of the identity of the loess and the bottom prairie, as there is of the loess and the bluff; and still we know the bluff was formed long before the bottom prairie, and under a very different condition of this part of the continent. It may, also, be stated, that there is just as much evidence of the identity of the bluff and bottom prairie, as of the bluff and loess, and yet the bluff and bottom prairie are not identical. The fossils of all three formations only prove them to be quaternary.

There is, indeed, but little probability that two such vast fresh-water lakes existed at the same time on the two continents, with the ocean rolling between.

But it would seem impossible to identify formations so recent on separate continents, whose recent faunas are so widely different; as the deposits on these continents, though contemporaneous, would of necessity present faunas very distinct. Hence, if we make fossils our only guide in identifying them, it will be impossible to distinguish deposits formed since the present genera of animals and plants came into existence, and we shall be compelled to omit all distinctions between formations of the recent period, and to make all of our recent deposits identical with each other, and with all belonging to the same system in Europe and Asia; and this would deprive us of distinctions recognized in scientific, and almost indispensable in economical, geology.

I have been thus minute in my examinations of the bluff, the bottom prairie, and the alluvial formations, both on account of their vast importance to our agricultural interests, and the comparatively little attention geologists have given to them. It is to this formation that the central Mississippi and southern Missouri valleys owe their preëminence in agriculture. The most desirable lands of Iowa, Missouri, western Illinois, Kansas, and Nebraska, all rest upon the fine silicious marls of the bluff formation. Where it is best developed, in western Missouri, the soil is inferior to none in the country.

The scenery presented by the bluff formation is at once unique and beautiful, and gives character to nearly all the best landscapes on the lower Missouri. Plates I. and II. of the Missouri Reports give characteristic views of the scenery where this formation is well developed.

DRIFT.

This formation lies directly beneath the bluff, and rests upon the various members of the Palæozoic series, as they successively come to the surface of that system. In this formation there appear to be three distinct deposits: —

1st. What might be called an *Altered Drift* frequently appears in the banks of the Missouri river, as at the mouth of the Kansas, and in the bottom prairie below Brunswick, and at Waconda prairie, sect. 2, No. 4. These strata of sand and pebbles appear to be the finer materials of the drift, removed and rearranged by aqueous agencies subsequent to the Drift period, and prior to the formation of the bluff. The pebbles are from all the varieties of rocks found in the true drift, but are comparatively small.

2d. The Boulder formation, as it was left distributed by those powerful and widely extended agencies, which formed that deposit of the northern hemisphere. It is a heterogeneous stratum of sand, gravel, and boulders, all water-worn fragments of the older rocks. The larger part are from the Igneous and Metamorphic rocks, in place at the north, and the remainder from the Palæozoic strata, upon which they rest. The Metamorphic and Igneous rocks must have come from the northern localities of those strata, the nearest of which, according to Dr. Owen's report, is on the St. Peter's river, about three hundred miles north of St. Joseph. But the Palaeozoic fragments are usually from localities near where they rest, as shown by the fossils they contain, and are as *completely rounded* as those from the more distant points.

The largest boulders observed in Missouri are five or six feet in diameter. They are usually granite and Metamorphic sandstone.

3d. *Boulder Clay.* In northern Missouri, the Boulder formation just described often rests upon a bed of bluish or brown sandy clay, through which pebbles of various sizes are disseminated in greater or less abundance. In some localities this deposit becomes a pure white pipe clay.

Range and Thickness. The *Altered Drift* has been observed more frequently in the north-western part of the State, and is often twenty-five or thirty feet thick. The Boulder formation abounds in all parts of the State north of the Missouri, and exists in small quantities as far south as the Osage and Meramec. Its thickness is very variable, from one to forty-five feet. Its development is greater, the boulders larger, and those of a foreign origin more numerous, towards the north.

Its thickness varies from one to fifty feet. The Boulder clay is also most abundant in the northern part of the State, and is, in some places, more than one hundred feet thick.

Organic Remains. I have seen no fossils in this deposit, save a few logs in the Altered drift of the Missouri. Some of these are still sound, and burn quite well when dry, as we have proved by building our camp fires with them on several occasions.

There are other deposits, particularly in the middle and southern parts of this State, which are not genuine drift; and yet they bear a greater resemblance to that than any other formation, and occupy precisely the same stratigraphical position.

Beneath the alluvium of the bottoms, we often find deposits of pebbles similar to the genuine or Altered drift of the Missouri, but all the materials come from the neighboring rocks, and appear like the beds of ancient streams.

On the high lands there are, in the same position, numerous beds of angular fragments of the adjacent rocks, somewhat worn, and indiscriminately commingled with sands and clays.

Whether these deposits were formed by the same agencies which produced the drift, or by a part of them only, or by other causes, has scarcely been determined.

3. ON THREE COMPARATIVE SECTIONS OF THE COAL-MEASURES IN KENTUCKY, AND IN EASTERN AND WESTERN PENNSYLVANIA. By J. P. LESLEY, of Philadelphia.

MY object in presenting these sections is twofold. While we note at this meeting such remarkable advancing steps of our science as the identification of the sub-palæozoic systems in Europe and America, and the finding of Keysport Potsdam fossils as a link between the Lower Silurian of Boston bay and Wisconsin, we should place on record also as part of the successes of the last year, and a very eminent part, the identification of the coal-beds of Pennsylvania, Kentucky, and Ohio by my friend M. Leo Lesquereux, whose unpublished results I take upon myself the responsibility of alluding to, simply to record the fact, and to communicate to those who are working with me in the same field the happiness I have myself experienced in seeing the evident final success of a task which, from a merely lithological and structural point of view, seemed hitherto hopeless. In 1841, '42, and '43, when the mass of materials collected by the gentlemen geologists of the Pennsylvania Survey fell into my hands, by direction of Professor Rogers, the chief of the survey, to be mapped, and thrown into vertical and horizontal sections, I spent many weeks in discussing hundreds of fragmentary sections of the coal-measures, both anthracite and bituminous, many of which I had made or verified myself, to realize what I supposed would be a very simple and easy consequence. In vain, however, I adjusted and readjusted these sections to each other in every variety of grouping. Although I felt instinctively and with increasing certainty as I went on, that an identity of the bituminous and anthracite beds existed; that the Monongahela and Alleghany systems, imperfectly made out as they were, would certainly be carried over their eastern outcrops, some day, when corrected and perfected, into the anthracite basins, and westward through Ohio into Kentucky; still no such scheme could be stated on paper by any method of exhibition at my command. The intervals were so variable over such small areas, and the beds themselves varied so much in appearance, that every extensive exhibition of them in series of vertical sections which I made, became a mass of confusion to the eye. The attempt was abandoned, and the sections were engrossed, in Mr. Rogers' Final Manuscript Report, separately, on the pages of the text in which they were severally described. The difficulty lay in the want of a palæontological guide. This has been furnished by the distinguished fossil botanist of Columbus, whom I feel honored to know and name, who has succeeded, after some years of the closest field-work, in laying the foundation for a perfectly systematic identification of our coals over our largest carboniferous areas. I need only mention his positive identification of the Lingula Umbonata and Lepidostrobus bed B everywhere, the identification of the Pomeroy coal-bed on the Ohio river, with the Gate vein at Pottsville, on the Schuylkill river, and the identification of the highest Curlew and Airdrie coal-bed of western Kentucky, with the great Pittsburgh bed of Pennsylvania, as typical examples.

My second object is to correct an important error in my Manual on Coal, suggesting the identity of the Mahoning sandstone with the Shamoken conglomerate. A glance at the three sections here compared shows that if the conglomerate of the twelfth Shamoken coal-bed extends west, it is the great Anvil rock of Kentucky, which in the Somerset section assumes its proper place above the Pittsburgh coals, while the Mahoning sandstone is several hundred feet below. There is no good reason why the area of these great sand-rocks should not be coextensive with that of the Bottom Conglomerate, or any other silicious palæozoic formation, the Oriskany or the Potsdam, for example. In this fact, and in the outspread of the limestones and distinguished clays, we have the ground of that pure lithological identification before alluded to, which began naturally to be studied before the palæontological identification could be started, and then had to wait for it, has been outstripped by it, but is following after it and will catch up with it by favor of its guidance. These fossil determinations have indeed a finer faculty; but, after all, they cannot replace structural determinations, and, in fact, depend upon these for their origin, and for their last certainties.

It also appears from these three sections, that our notions of the thickness of the coal formation must be modified. The extraordinary comparative depth of the anthracite coal-measures is all a mistake. This notion originated with the idea of a profound fault running through Pottsville, along the base of the Sharp mountain, leaving a certain number of vertical beds on its south side, and throwing down an enormous additional number on its north side. The sinking of the Salem Vein slopes has shown all this to be false. There is no dislocation. Consequently the number of beds cannot be materially greater on one side of the synclinal than on the other. Mr. Peter W. Shaeffer, the geologist, thinks he has proven by structure that the Gate, Salem, Lewis, and Spohn are the same bed. Mr. Lesquereux asserts the same fact from their fossils, and cannot yet find the Pittsburgh bed at Pottsville. The Dauphin section presented with this paper is collated from three old sections by Taylor, and shows three great beds just in the proper place for the Pittsburgh series, in Somerset county, and in Kentucky. In the Broad Top, Frostburg, and Shamoken basins the measures are equally deep. The inference is indubitable. There is no impoverishment of the carboniferous westward. The First Report of the Kentucky Survey has doubled the column, but there still remain in Kentucky the same thousand feet of strata, containing the same coal-beds as in the anthracite basins of the east. The number of beds — the intervals — are substantially the same; and, what is of more importance, the relative values of the beds among themselves seem to be maintained. In the Somerset section there is almost as fine an exhibition of coal in the upper group as any in the anthracite field, and a finer one than in the Dauphin section. The limestones disappear from the anthracite beds, but occur everywhere else.

Instead of letting any longer our geological glances wander uneasily over these vast areas, to see nothing but a confused intercalation of lenticular deposits, each one limited to a small territory, or so irregular as to defy detection under its many disguises, we are destined to see a fair panorama of harmonized sections, showing the essential regularity and persistency of the few beds of coal and lime, iron, clay, and sand, which constitute a nearly equal thickness of the formation throughout, and producing upon us an effect analogous to that produced upon us by the palæozoic series of formations as a whole, — excepting in the one point of total thickness, which in the palæozoic series, as a whole, undoubtedly declines rapidly westward, but in the coal-measures, taken as a separate formation, does not.

4. ON THE ANCIENT MINING OPERATIONS OF LAKE SUPERIOR.

By Col. CHARLES WHITTLESEY, of Cleveland, Ohio.

(Abstract.)

A REMARKABLE object in the geography of Lake Superior is Point Kenenaw, extending out into the lake some seventy miles, in form like the beak of an eagle. The spine of this peninsula is elevated some five or six hundred feet, composed of copper-bearing rock, which extends to an aggregate length of 160 miles. In very ancient periods of time, mining was carried on extensively along this range, and excavations have been found two miles long, and from twenty to thirty feet deep. This mining was by open cuts, like quarries, for the miners had no means of penetrating deeply into the earth, nor of raising large masses had they so penetrated. The copper found there is well known to exist in its native form, and in masses from minute specimens to those of five hundred tons. The implements and tools made by them of this metal were constructed without melting it, by simply beating it into form in its cold state, and there are no signs whatever that they had any idea of melting copper. Their mode of mining was to build fires to soften the rock, and then break out the pieces of metal by means of large stone hammers. Masses of five or six tons are found, from which pieces have been beaten, and the rest left in place.

At the Minesota mine, those miners had gone down in one place twenty-five feet, and recently, upon opening their excavation, a mass was found, which had been raised upon skids by means of wedges, weighing six tons. The opening had been filled up by rubbish as the miners advanced, and thus, with the accumulation of centuries in addition, it was not easily perceptible. Trees of full growth covered it when again found, and the proof that this was a real excavation for mining purposes was afforded by burnt wood, stone hammers, and the like. This is a sample of those ancient works scattered along a line of 150 miles. Some few cavities have been found, like a room, in the faces of rocky bluffs — places where a dozen men could easily work.

The miners' tools were hammers of hard stone, five to twenty-five pounds in weight; wooden shovels of cedar, used for scraping dirt, as shown by the form to which they are worn; copper implements, like the quarryman's gad, to be used as wedges, driven by the stone mauls; spear heads, copper knives, chisels, and the like. Upon blocks of timbers are still to be seen marks of these axes or chisels.

These tools, and the marks they have left, give a hint toward proving the connection of the people who mined copper here with the ancient Mexicans; for the copper tools, as depicted in Squier and Davis's work, correspond to those of Lake Superior. When I first saw the cuts upon the timber at the lake, I saw at once that they must have been made by an instrument like the copper axe of the Ohio mounds. Another fact was noted, that in the copper tools found in Ohio are seen spots of native silver — a well-known fact in relation to lake copper — and this proves that the Ohio people of the Mound epoch did not possess the art of smelting copper — else the silver would have been melted — and that they did derive their copper from the lake. Now Squier's work seems to prove the Mexican and Ohio race of people the same; the forms of their mounds, pyramids, etc., being the same, lead to this inference; and these facts equally tend to show the identity of the latter with the miners of Lake Superior.

In regard to the period when the mining operations were in progress, it is said that the timber growing in the old excavations is of full size and age. I have counted 290 rings of annual growth. But this does not carry us back so far as we can go with safety. There is plenty of evidence that these ancient trees are of the second growth, at least, since the mines were deserted. This carries us back at least to six hundred years. It must, however, not be forgotten, that the same species of tree does not immediately succeed, but others take its place. The miners, of course, must have cut the timber, and thus we may with confidence carry back our date to a period at least a thousand or twelve hundred years. Again, judging from the amount of work done, and their want of facilities, their labors must have extended through a period of five hundred years. From the fact that no remains of houses in that severe climate are found, no roads nor other improvements made by permanent inhabitants, the conclusion seems inevitable that the mines were wrought only in summer, and then by some people who came thither for the purpose, and departed with the approach of winter. This people, I am of opinion, dwelt in Ohio. The mounds and the mines are of the same age. The Indians of our era could not have been the miners, nor have they any traditions whatever relating to the mines.

Another gentleman has described an ancient pit on Portage lake, twelve to fifteen feet deep, where chisels, many stone hammers, stones used for sharpening gads, a skid charred by fire, etc., were found. In this pit, a tree with four hundred annual circles was growing. He concurred fully in my conclusions.

5. On the Division of the Azoic Rocks of Canada into Huronian and Lawrentian. By Sir William E. Logan, of Montreal, Canada.

The sub-Silurian Azoic rocks of Canada occupy an area of nearly a quarter of a million of square miles. Independent of their stratification, the parallelism that can be shown to exist, between their lithological character and that of metamorphic rocks of a later age, leaves no doubt on my mind that they are a series of very ancient sedimentary deposits, in an altered condition. The further they are investigated, the greater is the evidence that they must be of very great thickness, and the more strongly is the conviction forced upon me that they are capable of division into stratigraphical groups, the superposition of which will be ultimately demonstrated, while the volume each will be found to possess, and the importance of the economic materials by which some of them are characterized, will render it proper and convenient that they should be recognized by distinct names, and represented by different colors on the geological map.

So early as the year 1845, as will be found by reference to my Report on the Ottawa district (presented to the Canadian government the subsequent year), a division was drawn between that portion which consists of gneiss and its subordinate masses, and that portion consisting of gneiss interstratified with important bands of crystalline limestone. I was then disposed to place the lime-bearing series above the uncalcareous, and although no reason has since been found to contradict this arrangement, nothing has been discovered especially to confirm it; and the complication which subsequent experience has shown to exist in the folds of the whole (apparent dips being from frequent overturns of little value), would induce me to suspend any very positive assertion in respect to their relative superposition, until more extended examination has furnished better evidence.

In the same Report is mentioned, among the Azoic rocks, a formation occurring on Lake Temiscaming, and consisting of silicious slates and slate conglomerates, overlaid by pale sea-green or slightly greenish-white sandstone, with quartzose conglomerates. The slate conglomerates are described as holding pebbles and boulders (sometimes a foot in diameter) derived from the subjacent gneiss, the boulders displaying red feldspar, translucent quartz, green hornblende, and black mica, arranged in parallel layers, which present directions according with the attitude in which the boulders were accidentally inclosed. From this it is evident that the slate conglomerate was not deposited until the subjacent formation had been converted into gneiss, and very probably greatly disturbed; for while the dip of the gneiss, up to the immediate vicinity of the slate conglomerate, was usually at high angles, that of the latter did not exceed nine degrees, and the sandstone above it was nearly horizontal.

In the Report transmitted to the Canadian government in 1848, on the north shore of Lake Huron, similar rocks are described as constituting the group which is rendered of such economic importance, from its association with copper lodes. This group consists of the same silicious slates and slate conglomerates, holding pebbles of syenite instead of gneiss, similar sandstones, sometimes showing ripple-marks, some of the sandstones pale-red green, and similar quartzose conglomerates, in which blood-red jasper pebbles become largely mingled with those of white quartz, and in great mountain masses predominate over them. But the series is here much intersected and interstratified with greenstone trap, which was not observed on Lake Temiscaming.

These rocks were traced along the north shore of Lake Huron, from the vicinity of Sault Ste. Mary for 120 miles; and Mr. Murray ascertained that their limit on the lake shore occurred near Shibahahnahning, where they were succeeded by the underlying gneiss.

The position in which the group was met with on Lake Temiscaming is 130 miles to the north-east of Shibahahnahning, and last year Mr. Murray, in exploring the White-fish river, was enabled to trace the outcrop of the group, characterized by its slates, sandstones, conglomerates, greenstones, and copper lodes, for sixty-five miles from Shibahahnahning to the junction of the Maskinongé and Sturgeon rivers, tributary to Lake Nipissing. The general bearing of the outcrop is north-east, and an equal additional distance in the same direction would strike the exposure on Lake Temiscaming. In the portion which Mr. Murray examined last year, the dip appears to be about north-west, often at a high angle, while that of the subjacent gneiss is more generally south-east, sometimes at a low angle, and in some places nearly horizontal.

To the eastward of this outcrop, Canada has an area of 200,000 square miles. This has yet been but imperfectly examined, but in so far as investigation has proceeded, no similar series of rocks has been met with in it; and it may safely be asserted that none exists between the basset edge of the Lower Silurian, and the gneiss from Shibahahnahning to the Mingan islands, a distance of more than one thousand miles, and probably still further to Labrador.

The group on Lake Huron we have computed to be about 10,000 feet thick, and from its volume, its distinct lithological character, its clearly marked date posterior to the gneiss, and its economic importance as a copper-bearing formation, it appears to me to require a distinct appellation, and a separate color on the map. Indeed, the investigation of Canadian geology could not be conveniently carried on without it. We have, in consequence, given to the series the title of Huronian.

A distinctive name being given to this portion of the Azoic rock, renders it necessary to apply one to the remaining portion. The only local one that would be appropriate in Canada is that derived from the Laurentide range of mountains, which are composed of it from Lake Huron to Labrador. We have, therefore, designated it as the Laurentian series.

These local names are, of course, only provisional, devised for the purpose of avoiding paraphrastic or descriptive titles, the use of which had been found inconvenient, and they can be changed when more important developments, proved to be the equivalents of the series, are met with elsewhere.

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6. On the Probable Subdivision of the Laurentian Series of Rocks of Canada. By Sir William E. Logan, of Montreal, Canada.

I have already indicated the probable separation of the Laurentian rocks of Canada into two great groups, that characterized by the presence of much lime and that without; but from recent investigation, the result of which has just been reported to the Canadian government, it appears to me almost certain that the former of these two great groups will be capable of subdivision, and that some of its bands of limestone, and their associated strata, are of sufficient importance to be represented separately on the map.

Having followed out one of those bands of limestone through all its windings for a distance of eighty miles, the object of the present paper is to exhibit to this section of the Association its geographical distribution, and the forms it presents in the physical structure of the region which it characterizes.

What at first appear to be two bands of these limestones emerge from beneath the Lower Silurian series in the township of Grenville on the Ottawa, and run into the interior parallel to one another, striking N. N. E. They are about two miles separated from one another, and both, with the gneiss between, dip in one direction, that is, N. N. W., at angles varying from about fifty to seventy degrees. Attaining the rear of the township, a distance of about ten miles, the two bands unite, and are found really to constitute but one; the thickness of which, as far as I can make it out, is from five hundred to one thousand feet.

It is plain from this distribution, that the limestone is part of the outcrop of an undulating sheet, the ridges of which have been worn down. But in the horizontal section of an undulating surface, similar forms in the distribution of the rim may be derived from the anticlinal or synclinal part of the undulation, and as the dips on the opposite sides are both one way, it is a question to which part the area belongs.

Within a short distance of the eastern side of the limestone, in fact touching it in one place, an intrusive syenite makes its appearance, belonging to a mass which occupies about thirty square miles in the township of Grenville and Chatham, and runs to a point in Wentworth.

The intrusion of such a mass of igneous rock as this can scarcely fail to have had a considerable effect in modifying the attitude of the strata which surround it. The crystalline condition of the syenite shows that it was slowly cooled under great pressure, and we cannot now say whether it was a deep-seated part of an outburst which reached the surface, as it was then constituted, or whether it was originally overlaid by masses of gneiss and limestone which have since been worn away. In either case the probability is, that it would give to the strata now surrounding it an anticlinal form. It seems probable, therefore, that the western dip, belonging to the eastern band of limestone, where it approaches the syenite, is a true one, and that the form between the bands is synclinal. This appears to be corroborated by the fact that where transverse valleys occur between them, the wearing down of the intermediate gneiss widens the calcareous bands, particularly the east one, and narrows the interval.

The calcareous sheet having thus the form of a trough, the western dip of the western outcrop must be an overturn; and two spurs of the rock which point to one another, the one turning south from the western belt, and the other north from the eastern, must constitute a subordinate anticlinal. Without reference to minor corrugations, the general form of the area would be that of two troughs joined together, each about a mile and a half wide, with an overturn dip on the west side, the one trough running north and south, and the other, as far as unconcealed by the superior fossiliferous strata, south-south-west and north-north-east.

The opposite sides of this calcareous trough run in two valleys, which unite at its northern extremity. But though the limestone then crops out, the valley continues northward into Harrington, and after a short interval shows an isolated patch of limestone of about a mile and a half in length by a mile in breadth, possessing, of course, a synclinal form.

Beyond this the valley splits into two, and while one branch runs rather north of N. E., the other turns north of E. Each of these valleys is paved with limestone, the distribution of which shows a continuation of the synclinal form with a bend more to the eastward than before. The calcareous band on the western side has been traced to the north boundary of the township of Harrington, whence it crosses into Montcalm. It there appears to turn to the westward, but it has not yet been further accurately examined.

The eastern branch has been followed for between six and seven miles into Wentworth, when it appears to turn upon an anticlinal axis, and, proceeding in a bearing S. S. W. for seven miles, it attains the southern boundary of the township, close upon the east side of the northern prolongation of the intrusive syenite. It runs in the same bearing for about three miles along the eastern side of this into Chatham, and becomes deflected to the south-east by the main body of the syenite, to which it runs parallel for about three miles.

It then folds upon the axis of a synclinal, and, running N. N. E. for upwards of five miles, returns into Wentworth, where it gradually bends round more to the eastward, and in about five miles reaches a position in the Gore of Chatham. It here folds over upon the axis of an anticlinal, and turning S. S. E. it maintains this course for about eight miles, in which it crosses into the Seigniory of Argenteuil and reaches the vicinity of Lachute, where it once more bends upon a synclinal axis, and proceeding eastward for about a mile, plunges under the Potsdam sandstone and is lost.

In the winding course derived from the plications of the strata, the limestone usually presents a valley on the geographical surface; but to the west of all the folds that have been described, a bold ridge of gneiss runs from the front of Grenville to the rear of Harrington, the distance being about twenty miles, and the bearing N. N. E. On the west side of this ridge, about midway of its length, there are two areas about five miles long and three quarters of a mile broad, presenting the forms of valleys, which are underlaid by limestone so distributed as to render it probable that they are two outlying parallel troughs joined together, belonging to the same calcareous sheet as the one described.

There would thus be four main synclinals and three main anticlinals; and the breadth they occupy altogether is about eighteen miles, giving about four and a half miles for the breadth of each undulation.

Bands of dolomite sometimes accompany the limestone, which is often interstratified with bands of quartzite. The quartzites appear to be heaviest near the junction of the limestone and the gneiss, becoming thinner and less frequent as you recede from the calcareous rock. The greatest mass of quartzite met with had a vertical measure of four hundred feet, and it was in stratigraphical position beneath the limestone. The quartzite and the gneiss on each side of the limestone are often very thickly studded with garnets, and in some cases the aggregation of these is so close as to constitute a granular garnet rock.

In the Gore of Chatham a band of limestone, about three quarters of a mile to the north-west of the one described, has been traced running parallel with it for seven miles. If the form which has been attributed to the first band be correct, the second would overlie it with a great mass of gneiss between.

A third band of limestone occurs about six miles north of the second; this has been traced for about four miles, running east, which would be nearly parallel with the bearing of the second. In this bearing it has not yet been followed further than to within a short distance from the line between the Seigniory of Argenteuil and the township of Abercrombie, towards the rear of both. Continuous exposures of limestone have been met with on the west side of the Rivière du Nord, at St. Jerome. They have been followed for two miles in a north bearing, and the strike of the stratification, between St. Jerome and the rear of Abercrombie, is such as to make it probable that the St. Jerome rock will ultimately prove to be a part of the third band.

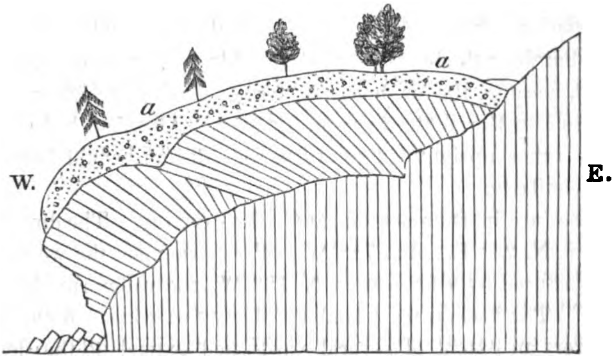
A feature common to both localities is the occurrence immediately near the limestone of immense masses of lime feldspar. North of the Argenteuil band, eight miles, examined across the stratification, consist almost entirely of it, in the form of labradorite, of which masses of the opalescent variety are in some parts inclosed in a paste of the mineral without any play of colors, and they are accompanied by hypersthene and ilmenite. Lime feldspar is abundant at St. Jerome, and its stratified character is conspicuously displayed, the beds running parallel with the limestone.

Mr. Hunt has traced a band of crystalline limestone for eleven miles, running diagonally across the township of Rawdon, in a north bearing. On the west side of this, lime feldspar forms the great bulk of the rock exposures for twelve miles across the measures, and shows a well-marked stratification. It appears probable that the Rawdon calcareous band is the same as the St. Jerome band, and that a synclinal axis exists between the two, the turn of the calcareous band on which is covered up by the fossiliferous rocks to the south.

In Chateau Richer, below Quebec, a band of limestone occurs about a mile from the fossiliferous deposits, and to the north-west of it, lime feldspars present a breadth of eight miles. On an island near Parry's Sound, on Lake Huron, Dr. Bigsby observed the occurrence *in situ* of the opalescent variety of labradorite, and the name of the mineral reminds us of the existence of the rock beyond the eastern end of the province. It thus appears probable that a range of the rock will be found, winding irregularly from one end of the province to the other, of sufficient importance to authorize its representation by a distinct color on the map, and a distinct designation in geological nomenclature.

7. ON FRACTURED LEDGES OF SLATE IN VÈRMONT. By CHARLES H. HITCHCOCK, of Amherst, Mass.

WHILE engaged in the geological survey of Vermont, the past summer, several cases of fractured ledges of slate came under my observation. I will describe them. The slate is argillaceous. Like most of the Vermont rocks, the strike of the slate is about N. 10° E.; the dip varies from 76° E. to 76° W., being in every case nearly perpendicular. The fractures are upon the tops and sides of hills. As the hills trend in the same direction with the strike, these fractures, being made by a force at right angles to the strike, are either upon the east or west side of the hills. Imagine, then, a valley running about north and south, with one of its sides precipitous. This precipitous side may be from twenty to one hundred feet high. The slope of the hill, when examined, as when preparations are made for quarrying roofing slate, is found to be made up of fragments of slate symmetrically arranged, possessing a constant strike, with a dip varying from horizontal to perpendicular. Beneath these fragments, the slate is found in place, undisturbed, in perpendicular strata. The figure will illustrate. A vertical section from east to west, crossing the fragments, is represented.



There are twelve cases of these fractures which have been seen in Vermont. They are in the towns of Guilford, Brattleboro', Dummerston, Northfield, and Newbury. The fracture at Bruce's quarry, in Guilford, has acquired some celebrity from the figures and descriptions of Dr. Jackson and Prof. Hitchcock. The fractured portion of the slate is upon the west side of a hill, the force having come from the east. The mass of broken fragments, so far as exposed, is about one hundred feet, north and south, in length; and its width, from east to west, about forty feet. There has been some alteration in the appearance of these fragments since their first notice, as part have been removed by workmen, and new additions have been made to the pile. Hence the earlier figures do not represent the mass as it appears now.

In the south-east part of Brattleboro', the strike of the slate is N. 20° E. The strata are perpendicular. The force came from the east, as before. The dislocation extends only three feet downwards.

In Northfield, as I was passing on the Vermont Central Railroad, I could see the relics of a similar fracture, the pieces having been mostly removed in the construction of the road. The strike of the slate is N. 20° E.; the dip, 76° W. The force in this example came from the west; and as the rock dips to the west, the breaking was performed more easily than in the previous examples mentioned.

At the slate quarries in another part of Northfield, the strata are simply bent fifteen to twenty degrees from their original position, and this differs altogether from the other examples. The valley runs east and west; the strata cross at nearly right angles, and the bent strata lie in the valley. The force came *down* the valley, much in the manner of a glacier. Some of the cases in Wales are said to be situated like this; but there the breaking was caused by true glaciers. Not so in Northfield, because the evidences of former glaciers, which we find in other valleys in Vermont, are wanting here. Also, had a glacier existed here, other upright ledges, a few rods distant, would have been fractured similarly.

Two cases are in Dummerston, at Clarke's quarry. The direction of the strata is N. 23° E.; dip, 78° W. They are upon the west side of the hill, fractured by a force from N. 15° W., the valley running north and south. The broken mass is about thirty feet wide. A third case nearly covers an extent of several yards, and extends to the depth of two feet.

But the finest example is at Newbury. The track of the Connecticut and Passumpsic Rivers Railroad passes through it, and so great is the mass of broken rock, that for a long time workmen have almost uselessly attempted to diminish the amount, that the trains may pass without inconvenience, else the jar of a heavy train would set in motion a terrific slide. The Connecticut river flows at the base of the mass, two miles above Newbury Centre. The strike is N. 40° E.; dip 76° W.; and the force came from the north-west. The north and south extent of the broken mass is 170 feet. Above the track of the railroad, it extends seventy-five feet, and probably twenty feet more beneath. The width east and west, without actual measurement, I should judge to be forty feet. The rock receives a little hornblende into its composition, thus making tough strata, while at a similar fracture, half a mile south, the rock is soft, and is a talcose slate. Probably for a mile or two along this hill these fractures occur, all of them upon the eastern slope, extending to the apex of the hill.

Having described the appearances, it remains to notice the theories already presented, to account for the facts.

In the First Report on the Geology of New Hampshire, Dr. C. T. Jackson figures and describes the example at Bruce's quarry, in Guilford, and accounts for it by supposing an overlapping of a portion of the strata, succeeded by a lateral thrust occasioned by original formation. His figure was very erroneous. Prof. Hitchcock having shown that the strata beneath had not been disturbed, and that there had been no crossing or overlapping, and that evidence of a lateral thrust was entirely wanting, Dr. Jackson, upon a reëxamination, abandoned the figure and the theory, and gave a correct figure with a new explanation. He proposed the agency of water, percolating between the strata of slate, as the cause of the fracture. Frost enters deeper every year, causing the layers to expand, diverging upwards. A continuation of this process, Dr. Jackson argues, would at length bend over strata enough to equal in size the masses described.

But this theory, correct as far as it goes, does not account for the *breaking off* of the strata; and it would be difficult for expansion simply to throw the strata down loosely into a horizontal position near the top of the hill, before the slate was quarried.

Another case of slate, bent like the letter Z, by the same force, as described by Prof. Adams, shows that there must have been a force *besides* the expansion, directed downwards.

Another theory supposes that the drift agency produced these fractures. There are cases in other parts of the country where drift has without doubt performed the work, as in eastern Massachusetts, New Jersey, and especially the case near Niagara Falls, described by Prof. Hall. But many of the forces in the examples in Vermont came from the east, a direction different from the drift current all over the State. And the fractures, where the force came from the west, are universally situated on the eastern slope, below the top of the hill, thus being precisely on the lee side, where the drift agency has not operated. I can say this with confidence, as I have not found a single case in the State where the south side of a hill or mountain has been worn or scratched by drift. Nor can we suppose, with Darwin, that when the continent was submerged, the slate was fractured by icebergs lifted up and down by the tides; or that huge icebergs, rising and falling by the undulations of a heavy sea, struck these hills with a downward force. The objection rises from the direction. The current of the ocean in the drift period was about north and south. The onward southern movement of the iceberg, then, would crush the strata so obliquely, that its mark — which is wanting — might be seen at the present day. And it would be very curious, also, to know how an iceberg, in these twelve cases, always happened to strike upon the slope of the hill, either eastern or western, and never upon the top of the hill.

To suppose that these effects are attributable to glaciers, would be inconsistent with the facts; since the glaciers would have followed the slopes of the valleys, a course at right angles to that taken by these forces. Upon visiting one of these fractures recently, the true explanation flashed over the mind. We stood upon the top of the crushed portion, and saw that we were upon the debris of a veritable slide, which formed during the past winter. The water percolating into the seams of the rock had frozen, separating the strata. Gradually the strata got upon their edges, perhaps requiring a hundred years, when they were ready to be broken by any force pushing laterally. In the winter the hill is covered with snow, which, in a severe season like the last, accumulates in enormous masses. This, with the loose soil and timber resting upon the weakened strata, together with the weight of the strata themselves, upon a slight impetus would begin to slide down the hill, when the strata previously made perpendicular by the freezing water will be broken off, the top of the severed mass will be dislocated the most, even to horizontal, and rarely back to perpendicular (see figure, in which *aa* represents the slide). Thus the work of fracturing is going on at present under favorable circumstances. The case alluded to occurred only during the past winter, and we were so fortunate as to visit the quarry before the workmen had removed the fragments.

This, we trust, presents the true cause of the phenomena. It seems much better to seek for forces now in operation to explain this and all phenomena, than to attribute causes which may or may not have operated in any particular district.

8. ON A FEATURE IN THE RANGE OF THE WESTERN MOUNTAIN SYSTEM OF NORTH AMERICA. By Major W. H. EMORY, U. S. A., of Washington, D. C.

AT the last meeting of the American Association for the Advancement of Science, the undersigned read a paper giving a general view of that portion of the American continent traversed by the United States and Mexican Boundary line, and of the lines previously explored by him. Since then the projection of the local surveys has developed what seems to be a law in regard to the orography of that portion of the continent, which it is thought may be general throughout the western portion of the continent, when the surveys in the interior are made with the same minuteness as those on the boundary.

Most of the surveys and explorations across the continent have been made for the purpose of finding the best route for a railway or wagon road, or for the purpose of ascertaining the sources, direction, and capacity of rivers for navigation, and they have usually followed the valleys and cañons of the mountain system. Hence, what was seen and projected by the persons conducting these surveys, were either valleys surrounded by mountains or mountain passes. The mountain system itself was not traversed transversely by these surveys, and the topography connecting the different points, where the mountains were crossed, were represented hypothetically. Hence the variety of forms the system of mountains assume with every new map issued of the American continent.

The line of the Boundary Survey was, however, in a direction very generally at right angles to the meridian, and in a straight line across the mountains, without regard to valleys and passes. The projection of these surveys develops the singular fact, that the various chains of mountains crossed, although presenting to the traveller and casual observer apparently a continuous line, are in fact composed of a variety of isolated chains disposed *en echelon.*

To illustrate the subject more clearly, I present two proof sheets of maps yet unfinished. A glance at them will show better than written description the orographic law, which I have attempted briefly to explain.

The discovery of this form of the mountains will go far to explain, what has heretofore appeared so extraordinary to casual observers, that the great rivers of the western portion of the American continent should almost universally strike the great chains of mountains nearly at right angles to their general direction. It now appears, that, with regard at least to those mountains examined, there are valleys between the links forming the chain, and these valleys are sometimes lower than the plains between the different ranges of mountains.

9. ON THE CRYSTALLINE ROCKS OF THE NORTH HIGHLANDS OF SCOTLAND. (In the form of a letter addressed to SIR WILLIAM LOGAN.)

LONDON, July 27, 1857.

MY DEAR SIR WILLIAM,—Being unable, to my great regret, to attend the Montreal meeting of the American Association for the Advancement of Science, where my distinguished friend, Professor Ramsay, will represent British geologists and our survey, I beg to communicate to you, and any geological contemporaries who may be present, the final determination of a question which has been much agitated in this country, and which has just been settled by a comparison with North American typical fossils of Lower Silurian age. This question is:—What is the true place in the geological series, of those great masses of crystalline or subcrystalline stratified rocks, in the North Highlands of Scotland, in some of which organic remains were discovered by Mr. Charles Peach, in 1855?

That discovery induced me, in the same year, to revisit the localities in the north-west part of Sutherlandshire, to the east of Cape Wrath (Durness), in which the fossils had been detected; my chief object being to ascertain if the views of former explorers of that region, including Sedgwick and myself in 1827, were correct; namely, that these quartz rocks and limestones, associated with mica schist and a sort of gneiss, are of a more ancient date than the great series of Old Red Sandstone, or Devonian deposits, that occupy so large a portion of the north-east of Scotland, and are particularly developed in Caithness and the Orkney islands.

The results arrived at in that excursion, in which I was accompanied by Prof. James Nicol, were communicated at the meeting of the British Association, at Glasgow, in September, 1855, and published in the volume of that year — (See Transactions of the Sections, 1855, p. 85). I then reaffirmed the opinions I had formed in the year 1827, in company with Professor Sedgwick, as to the anteriority of all these quartz rocks, with intercalated limestones, to the Old Red Sandstone, or Devonian System; and judging from the facts that such crystalline and subcrystalline strata reposed unconformably upon an ancient granitoidal gneiss, and were flanked and surmounted by the ichthyolitic deposits of Caithness, I expressed my belief, that, although very imperfect and difficult of absolute determination, the fossils there found by Mr. Peach were of Lower Silurian age.

At that time, my eminent and lamented friend, the late Hugh Miller, had suggested, theoretically, that the quartzites and limestones of the north-western Highlands might prove to be the metamorphosed equivalents of the Old Red series of the Eastern Coast; and, subsequently, Prof. Nicol has even endeavored to show that these rocks may be the metamorphosed representatives of the carboniferous series of the south of Scotland! Both these suggestions were, of course, opposed to my own belief, and as they have been put forth by distinguished contemporaries, I have now to show how my own views have been sustained.

Within these few weeks, Mr. C. Peach has found, in the same locality, (Durness,) other and better preserved fossils, which have, I rejoice to say, set the *questio vexata* at rest, as will be seen by the annexed note of Mr. Salter, who unhesitatingly compares these remains with those known to Mr. James Hall, yourself, and other North American geologists, as occupying the true Silurian position of the calciferous sandrock and base of the Trenton limestones.

It is of course most gratifying to me to find that the general views of succession of the rocks of my native Highlands, indicated so far back as the years 1826–7, — opinions then formed irrespectively of zoölogical evidences, and simply from the physical relations of the rock masses, — should have been thus supported by fossil discoveries.

North American geologists will, of course, have no difficulty in understanding and admitting the conversion of Lower Silurian sediments into quartz rocks, crystalline limestone, mica-schists, chloritic slates, etc.; since their own eastern coast ranges exhibit such phenomena, some of which have been described and mapped by yourself.

To the geologists of the old country this determination is of the deepest interest; for it gives them a key to unravel the real age of large masses of the quartzites, limestones, chloritic and clay slates, mica-schists and quasi-gneissic rocks, (sometimes more, sometimes less metamorphosed,) which occupy vast wild tracts of the Highlands of Scotland.

The general order of the Scottish rocks is, therefore, pretty well ascertained. The lowest known rocks are masses of granitoid gneiss, on the upturned edges of which repose certain hard gritty beds and conglomerates, often of a red color, which, in the early days of our science, were confounded with the Old Red Sandstone. Now, however, that the existence of conglomerates at different levels in the Lower Silurian rocks of the south of Scotland has been demonstrated, (See Siluria, pp. 156–60,) the old views dependent on the mineral characters only, have been swept away. The lowest, indeed, of the conglomerates on the north-west coast of the Highlands may pass for the Cambrian rocks of the Geological survey. Then follows in an ascending order, the series of quartzites, mica and chloritic schists, etc., with included limestones, representing in a metamorphic condition the Lower Silurian sediments.

It is highly probable that the Upper Silurian rocks, which exist partially in the south of Scotland, have no real equivalent in the Highland; since the metamorphic rocks above adverted to are unconformably overlapped by those conglomerates and sandstones which form the very base of the Devonian rocks or Old Red Sandstone.

That great series is clearly exhibited on the north-east coast of the Highlands, and is made up of three subdivisions; namely, (a) Lower Conglomerates and Sandstone, (b) Middle Flagstones and Schists, with abundance of the well-known ichthyolites, and (c) Overlying Sandstones, — the latter constituting the northern headlands of Caithness, and the chief hills of the Orkney islands.

I feel confident that this triple series represents in full, as I have endeavored to show in my work (Siluria), the Devonian rocks of Devonshire, as well as the slaty rocks of the Rhenish Provinces (including the Terrain Rhénan of Dumont).

The *experimentum crucis* as respects Russia was in fact settled by the discoveries of my colleagues, De Verneuil and Keyserling, and myself, when we found the fossil shells of Devonshire and of the gorges of the Rhine in the same beds with the ichthyolites of the Scottish Old Red; many species being identical.

In turning to Ireland we have there obtained evidences illustrative of the conversion of Lower Silurian rocks, as shown by sections across the Connemarra mountains, where a great succession of crystalline limestone and quartzites, including the green Connemarra marble, having been observed to lie directly beneath strata with fossils of the Llandovery rocks (Middle Silurian), I have had no hesitation in considering these altered masses to be representatives of the Lower Silurian of other tracts. (See Siluria, p. 108.) *

Again adverting to Ireland, the survey under our friend Mr. Beete Jukes has ascertained, that in the Dingle Promontory true Upper Silurian rocks, with both Wenlock and Ludlow fossils, are conformably surmounted by many thousand feet of hard chloritic and silicious grits and schists (Glengariff grits), which represent, in my opinion, the great mass of the Devonian rocks. The peculiarity, however, of the Irish section is, that between these Glengariff Grits, and that which has hitherto been exclusively called the Old Red Sandstone of Ireland, there is a great hiatus; for the latter reposes on the edges of the former, and passes conformably under the carboniferous deposits.

This phenomenon, however, simply shows that a great break or local change in the sediments took place in the south-west of Ireland which had no existence in the north-east of Scotland, where the Old Red or Devonian series is continuous.

I cannot on this occasion enter into questions of detail concerning the localities where the Upper Silurian strata pass upwards with perfect conformity into the Old Red or Devonian rocks, or indicate other tracts in Europe, (notably in France and Spain,) where on the contrary the Upper Silurian is entirely omitted. In regard to local dislocations, I particularly refer you to my comparison of the Old Rocks of the Thuringerwald and the Hartz.† I will simply conclude this letter by calling your attention to what is now seen to be the true

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* I examined this tract last year in company with Mr. Jukes, Mr. Griffith, and Mr. Salter. Mr. Du Noyer has ably mapped and delineated the country.

† Quarterly Journal, Geological Society, November, 1855.

method of comparing the Older Palæozoic or Silurian rocks of distant regions.

When that skilful and profound geologist, M. Barrande, published in the course of last year his most instructive essay, entitled "Parallèle entre les dépôts siluriens de Bohême et de Scandinavie," he showed how with an agreement in generic characters of the fossils of each Silurian zone, thus indicating a general harmony, there was a great contrast in the species of marine animals in each of the countries compared. By applying this method in a different sense, I may now say that when the Silurian rocks are viewed in their extension through the same latitudes, a remarkable specific agreement is clearly traceable. On the other hand, the Silurian fossils of Bohemia are in accordance with those of France and Spain, or along another and distinct broad southern zone of the same age.

The Silurians of Scandinavia are of the British and American type. In making known the description of the Silurian rocks of Norway by Mr. Kejereslf,* I have recently shown how remarkable is the persistence of the Lower Silurian types (even in species) when these rocks are followed from Scandinavia into the British isles, and to how great an extent this resemblance of type is preserved, even when the Atlantic is traversed, and that the same strata in the crust of the globe are again met with in North America. The occurrence in the south of Scotland of the *Maclurea magna* of Hall, of the *Isotelus gigas* in Ireland, and of the fossils of your calciferous sand-rock in our Scottish Highlands, are all most satisfactory proofs that the order in Canada and the country of our kinsmen is, with certain modifications, the same as in the ancient realm of Caractacus.

Excuse this hurried letter, and wishing you as successful a meeting as your labors and those of my other eminent friends in the United States deserve,

Believe me to be,
Yours very sincerely,
RODERICK I. MURCHISON.

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* Journal Geological Society, about to be published.

PALEONTOLOGY.

1. Note on the Fossils in the Crystalline Rocks of the North Highlands of Scotland. By J. W. Salter, F. G. S.

The specimens previously sent from Durness were far from satisfactory, and, though clearly Palæozoic, could not be appealed to as settling their true place. They might, indeed, have been either Carboniferous or Devonian, although Sir R. I. Murchison had offered strong geological reasons to lead us to suppose them to be Lower Silurian forms. One cast in particular, which was at first doubtfully regarded as a *Maclurea*, though it had a right-handed curvature of the whorls, is now more properly referred to *Raphistoma* or *Ophileta*. And an *Orthoceras* present in the same beds could not decide the ease. But those lately collected by Mr. Peach leave no doubt as to the true age of the beds. The principal fossil will be particularly interesting to Canadian geologists, being the same as one from the "Calciferous Sand-rock" of Beauharnois, and which, being undescribed, has received the MSS. name of *Ophileta compacta*. The genus is doubtful, and the fossil is probably only a sub-genus of *Raphistoma* (Hall), the species of which have a wide umbilicus (bounded by a very prominent ridge) and straight-sided whorls. This species in Canada grew full an inch and a half wide, and had as many as six or seven whorls, flat above, and with a sunk apex, and a very broad and wide umbilicus, so that the entire shell is much attenuated, and the inner whorls would easily break out, as in Mr. Hall's figure of *O. levata*, Pal. 4, 7; Pl. 3, vol. 1, fig. 4. The whorls of that species are much less carinate below, and the umbilicus not nearly so wide. *O. compacta* will be fully figured and described in a decade of the Canadian fossils; it is unnecessary to say more of it here. It is curious that the *Euomphalus* (*Maclurea matutina*) which accompanies the Beauharnois fossil in Canada, is found also in the Highland beds, with another thick whorled species. Again, a species of *Pleurotomaria*, known in America in the Trenton limestone — the *P. subconica* (Hall) — comes so very near to one of our fossils, that it might well be only a variety of the species. The Highland fossil has rather more numerous whorls, and perhaps a broader band. The genus *Oncoceras*, so characteristic of the Trenton limestone, also occurs, but of a larger species, with more numerous septa than the *O. constrictum*. As the calcareous beds in Canada frequently contain the fossils of more than one subdivision of the New York series, it is not more than we should expect, to find the above fossils associated in a single thick band of limestone. It is most satisfactory to find, in the northernmost part of Scotland, the representatives of the Calciferous sand-rock and the Trenton limestones, as in the south of Scotland, that of the Chazy limestone.* And as the former repose upon a quartz rock with abundance of fucoidal impressions, the suggestion is obvious that such rock may, perhaps, occupy the place of the Potsdam sandstone.

NOTE. Mr. C. Peach is now proceeding, at my special request, to endeavor to collect more fossils, not only at Durness, but throughout the Assynt and other tracts into which the same limestones and quartzites extend.

2. ON GRAPTOPORA, A NEW GENUS OF POLYZOA, ALLIED TO THE GRAPTOLITES. By J. W. SALTER, F. G. S.

IT has long been doubtful to what class and order the *Graptolite* belongs. McCoy stands almost alone in referring them to the Sertulariadæ, on account of their being built up, as he supposes, of separate cell, cut off by a diaphragm from the common tube, a structure which Barrande's discoveries have disproved. The generality of naturalists have followed Dr. Beck, in classing *Graptolites* with the Alcyonarian polypes (*Virgularia* being the form with which they are most allied), and the figures of Geinitz and of Richter, showing what appear to have been soft and branched forms, probably belonging to the Graptolitidæ, have tended to confirm such a conclusion.

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* The great *Maclurea* of Grivan, in Ayrshire, has been identified with the *M. magna* (Hall) by Prof. McCoy.

Mr. Huxley is the only naturalist, so far as I am aware, who has suggested the Polyzoa as a group to which these bodies might belong. Symmetrically branched forms, connected by a membraneous expansion at the common base, found by Sir W. E. Logan in the Hudson group of Canada, seemed to him to offer analogy with Defrancia and allied genera. But there was no link to connect the horny tubes and long-projecting cells of the Graptolites with any of the forms of Polyzoa, so abundant in the old palæozoic sea. This link, or one of the links, has at length been supplied, and it is in a direction where we should perhaps not have looked for it. The Fenestellidæ have been recognized as an extinct group of Polyzoa, allied to Retepora, and differing from it by the presence of a vertical fibrous layer on the reverse side.

But all the known species are calcareous, furnished with a layer of fibrous structure, above noted, in which are set short cells, quite distinct from each other; so that to convert a Fenestella into a Graptolite, even supposing we have the least branched species of the former and the most complex of the latter to compare, it would be necessary to get rid of nearly all the calcareous material, reduce the fibrous layer to a mere thread (the axis), and lengthen out the cells enormously, while they were made fully to communicate with each other at their bases. The connecting processes, too, or dissepiments, must be removed, as nothing of the kind has yet been observed in the Graptolithnia, unless some obscure traces of lateral processes observed by my friend, Dr. Wyrille Thomson, in the *Grapt. Sedgwicki*, may have relation to them.

The species about to be described (Gorgonia flabelliformis Eichwald) appears to fulfil most of the conditions required to connect the two groups, since it has only a corneous structure, with no calcareous layers, and has the cells elongated on the inner surface of the cup, so that but for the connecting processes, in which it resembles Fenestella, it would be a branched form of Graptolite, not greatly more compound than the species discovered in Canada, and before noticed. I had found it in plenty in North Wales (1853), and named it *Fenestella socialis* before I was aware of its structure or its identity with Eichwald's species.

In a letter from Count Keyserling to Sir R. I. Murchison (1857), he mentions this *Gorgonia flabelliformis*, Eich., as abundant in the lower schists of Russia, as well as Sweden, and that Angelin has proposed the name *Phyllograpsus* for it. It is evident, therefore, the Swedish Paleontologist has observed the affinities above noted; and I would willingly adopt his expressive unpublished name, were it not that we are sure of the affinity with Fenestella, while that with the Graptolites, though strongly suggested by the structure, is not certain enough to warrant our including it in a symmetrical nomenclature with the Graptolitidæ. The term *Graptopora* will be sufficiently in unison with other genera of the Fenestellidæ, and at the same time indicate what I believe a true relation with the Graptolite group.*

GRAPTOPORA–*n–g.*

General form, rooted, net-like, cup-shaped, oblong, parallel, dicliotomous branches, connected by very irregularly placed processes (dissepiments). The branches of corneous texture (without a fibrous layer), of a double row of cells, which have projecting angular mouths, on the inner sides of the cup.

G. socialis, n. sp. [*Gorgonia flabelliformis*, Eichw. Urwelt Russlands, Heft 2, p. 45, tab. 1, f. 6 ?]

G. *triuncialis, calycibus tubulosis congregatis* [*ex eadem radice* 3–4 ?]: *ramulis parallelis approximatis; dissepimentes crebris irregularibus.*

Lest the English species should prove distinct from the larger Scandinavian form (*Gorgonia flabelliformis*, Eichwald), a specific character is given above; but they appear to me identical; and, in a collection lately sent by M. Kjerulf from Xtiania, the Swedish species appears with the name (*Fenestella socialis*) which has been for some years in manuscript, applied to the English specimens in our cabinets. I had not, however, discovered, till lately, its generic character.

The English and Welsh specimens differ a little in the width of the meshes; but in no important particular. The cups are frequently three inches long and one and a half broad at the top, and pretty regularly conical. They are very generally grouped in threes or fours at the base, and must have grown obliquely, if not radiating horizontally, as they appear now arranged so upon the slabs. The attachment is a short radicle, which soon branches into three or four dichotomising stems, and these again branch three or four times in the lower and middle part of the cup, till the full number (thirty to thirty-five) of parallel stems (or interstices, as they used to be called) are obtained, which end rather abruptly and regularly at the upper margin of the cup.

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* There is another reason why I should be glad to see the name adopted. The genus was described, and the name printed, in the second edition of Sir R. I. Murchison's Siluria (now in press), before I was aware that Angelin had noticed it. And I believe his name is only in MSS.; if otherwise, *Graptopora* should of course yield to *Phyllograpsus.*

The branches themselves are minutely flexuous throughout, and throw off on either side numerous thin processes (or dissepiments) very irregular in direction and position (Eichwald especially mentions this irregularity in his *G. flabelliformis*), sometimes oblique upwards, sometimes downwards, and not unfrequently two close together, or partially coalescing. The cells show themselves in a double row in the branches, projecting a little on each side of them, and rather closely set at about the diameter of the branches, apart. (The mouths of the cells only show on parts of the English specimens, but are clearly seen on those from Scandinavia, along the whole length of the branches, which are laterally compressed.) They are projecting angular processes, exactly like the teeth of *Graptolites*, the lower edge oblique and produced upwards into a short spine, the upper edge nearly straight and horizontal, or a very little concave. They are not unlike the cells and mouths of *Didymograpsus* (*Graptolites*) *geminus*, His., a species which occurs, with that above described, in the lowest alum slates of Scandinavia.

The irregularity of the dissepiments is a character, so far as I know, not met with in any of the true *Fenestellidæ*. However, the corneous texture and projecting cells will, I think, separate *Graptopora* from that group, and constitute it a new family, leading off to the Graptolitidæ.

3. ON THE VARIETIES AND MODE OF PRESERVATION OF THE FOSSILS KNOWN AS STERNBERGIÆ. By J. W. DAWSON, F. G. S., of Montreal.

THE fossils which have been named Sternbergiæ and sometimes Artisiæ, are usually mere casts in clay or sand, having a transversely wrinkled surface, and sometimes an external coaly coating and traces of internal coaly partitions. They are found in the coal formation rocks of most countries, and very abundantly in those of Nova Scotia. Until the recent discoveries of Corda and Williamson, they were objects of curious and varied conjecture to geologists and botanists, and were supposed to indicate some very extraordinary and anomalous vegetable structure. They are now known to be casts of the piths or internal medullary cavities of trees, and the genera to which some of them belong have been pointed out. Many interesting truths with respect to them, both in their geological and botanical relations, still, however, remain to be developed; and in the present paper I propose to offer some further contributions towards their history, and the geological inferences deducible from it.

In a paper communicated to the Geological Society of London, in 1846, to which Professor Williamson, in his able memoir in the Manchester Transactions,* assigns the credit of first suggesting that connection between these curious fossils and the conifers, which he has so successfully worked out, I stated my belief that those specimens of Sternbergiæ which occur with only thin smooth coatings of coal, might have belonged to rush-like endogens; while those to which fragments of fossil wood were attached, presented structures resembling those of conifers. These last were not, however, so well preserved as to justify me in speaking very positively as to their coniferous affinities. They were also comparatively rare; and I was unable to understand how casts of the pith of conifers could assume the appearance of the naked or thinly coated Sternbergiæ. Additional specimens, affording well-preserved coniferous tissue, have removed these doubts, and in connection with others in a less perfect state of preservation, have en-

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* Vol. ix., 1851.

ILLUSTRATIONS OF STERNBERGIA.

Fig. 1.

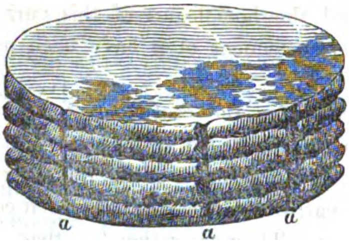

Fig. 2.

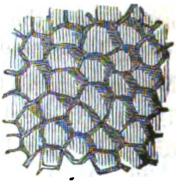

FIG. 1.—Portion of Sternbergia, (nat. size,) (*a*) Remains of Woody Fibre.
FIG. 2.—Transverse Section of one of the Diaphragms of Fig. 1, (magnified.)

Fig. 3.

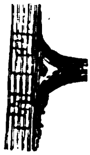

Fig. 4.

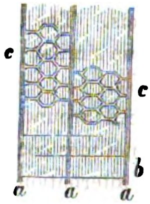

FIG. 3.—Junction of Diaphragm and Wood of Fig. 1, (magnified).
FIG. 4.—Woody Tissue of Fig. 1, (highly magnified). (*a*) Cell Walls. (*b*) Medullary Rays. (*c*) Hexagonal Discs.

Fig. 5.

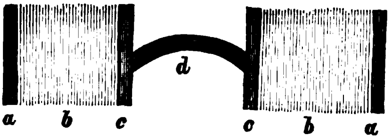

Fig. 5, *A*.

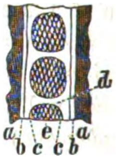

FIG. 5.—Longitudinal Section of Recent Cecropia peltata, (nat. size). (*a*) Bark. (*b*) Wood. (*c*) Pith lining Medullary Cavity. (*d*) Diaphragm of Pith.
FIG. 5, *A*.—Section of young branch of a species of Ficus, showing the outer pith tissue and partitions, and the spaces between the latter still filled with the ordinary or inner pith. Reference letters same as Fig. 5. (*e*) Ordinary Pith.

Fig. 6.

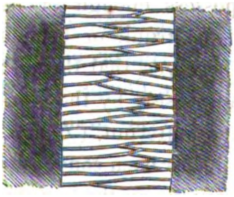

Fig. 7.

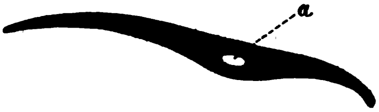

FIG. 6.—Flattened Sternbergia with compressed Bark, (nat. size).
FIG. 7.—Flattened Trunk, one foot in diameter, with Sternbergia. (*a*) Portion of Sternbergia Cast.

abled me more fully to comprehend the homologies of this curious structure, and the manner in which specimens of it have been preserved independently of the wood.

My most perfect specimen is one from the coal field of Pictou, (Fig. 1). It is cylindrical but somewhat flattened, being one inch two tenths in its least diameter, and one inch and seven tenths in its greatest. The diaphragms or transverse partitions appear to have been continuous, though now somewhat broken. They are rather less than one tenth of an inch apart, and are more regular than is usual in these fossils. The outer surface of the pith, except where covered by the remains of the wood, is marked by strong wrinkles, corresponding to the diaphragms. The little transverse ridges are in part coated with a smooth tissue similar to that of the diaphragms, and of nearly the same thickness.

When traced around the circumference or toward the centre, the partitions sometimes coalesce and become double, and there is a tendency to the alternation of wider and narrower wrinkles on the surface. In these characters and in its general external aspect, the specimen perfectly resembles many of the ordinary naked Sternbergiæ.

On microscopic examination the partitions are found to consist of condensed pith, which, from the compression of the cells, must have been of a firm bark-like texture in the recent plant, (Figs. 2 and 3). The wood attached to the surface, which consists of merely a few small splinters, is distinctly coniferous, with two and three rows of discs on the cell walls, (Fig. 4). It is not distinguishable from that of *Pinites*, (*Dadoxylon*,) *Brandlingi*, of Witham, or from that of the specimens figured by Professor Williamson. The wood and transverse partitions are perfectly silicified, and of a dark-brown color. The partitions are coated with small colorless crystals of quartz and a little iron pyrites, and the remaining spaces are filled with crystalline laminæ of sulphate of barytes.

Unfortunately this fine specimen does not possess enough of its woody tissue to show the dimensions or age of the trunk or branch which contained this enormous pith. It proves, however, that the pith itself has not been merely dried and cracked transversely by the elongation of the stem, as appears to be the case in the butternut, (*Juglans Cinerea*,) and some other modern trees; but that it has been condensed into a firm epidermis-like coating and partitions, apparently less destructible than the woody tissue which invested them. In this specimen the process of condensation has been carried much further than in that described by Professor Williamson, in which a portion of the unaltered pith remained between the Sternbergia-cast and the wood. It thus more fully explains the possibility of the preservation of such hollow chambered piths, after the disappearance of the wood. It also shows that the coaly coating investing such detached pith casts is not the medullary sheath, properly so called, but the outer part of the condensed pith itself.

The examination of this specimen having convinced me that the structure of Sternbergiæ implies something more than the transverse cracking observed in Juglandaceæ, I proceeded to compare it with other piths, and especially with that of Cecropia Peltata, a West Indian tree, of the natural family Artocarpaceæ, a specimen of which was kindly presented to me by Professor Balfour, of Edinburgh, and which I believe has been noticed by Dr. Fleming, in a paper to which I have not had access. This recent stem is two inches in diameter. Its medullary cylinder is three quarters of an inch in diameter, and is lined throughout by a coating of dense whitish pith tissue, one twentieth of an inch in thickness. This condensed pith is of a firm corky texture, and forms a sort of internal bark lining the medullary cavity. Within this the stem is hollow, but is crossed by arched partitions, convex upward, and distant from each other from three quarters to one and a quarter inch. These partitions are of the same white corky tissue with the pith lining the cavity; and on their surfaces, as well as on that of the latter, are small patches of brownish large-celled pith, being the remains of that which has disappeared from the intervening spaces. Each partition corresponds with the upper margin of one of the large triangular leaf scars, arranged in quincuncial order on the surface of the stem. (Fig. 5.)

Inferring from these appearances that this plant contains two distinct kinds of pith tissue, differing in duration and probably in function, I obtained, for comparison, specimens of living plants of this and allied families. In some of these, and especially in a species labelled "Ficus Imperialis," from Jamaica, I found the same structure; and in the young branches, before the central part of the pith was broken up, it was evident that the tissue was of two distinct kinds, — one forming the outer coating and transverse partitions opposite the insertions of the leaves, and retaining its vitality for several years at least; the other occupying the intervening spaces or internodes, of looser texture, speedily drying up, and ultimately disappearing, (Fig. 5, *A*).

Another variety of the Sternbergia-like pith structure appears in a rapidly growing exogeneous tree with opposite leaves, cultivated here, and I believe a species of Paullinia. In this trunk there are thick nodal partitions, and the intervening spaces are hollow, and lined with firm corky pith, with its superficial portion condensed into a sort of epidermis, and marked with transverse wrinkles; a cast of which would resemble those Sternbergiæ which have merely wrinkles without diaphragms.

The trunks above-noticed are of rapid growth, and have large leaves; and it is probable that the more permanent pith tissue of the medullary lining and partitions serves to equalize the distribution of the juices of the stem, which might otherwise be endangered by the tearing of the ordinary pith in the rapid elongation of the internodes. A similar structure has evidently existed in the coal-formation conifers of the genus Dadoxylon, and possibly they also were of rapid growth, and furnished with very large or abundant leaves.

I have no means of ascertaining to what extent this structure may characterize certain botanical families, nor what gradations it may present, between the mere transverse cracking observed in the trunks of the Butternut and other Juglandaceæ, and the perfect partitions developed in Cecropia. Prof. Gray states that the transverse pith structure is characteristic of the North American trees of the genus Juglans, but wanting in the closely allied genus Carya—a parallel case with its apparent restriction to one genus, or perhaps species, of extinct conifers. It is quite possible that some of the more rapidly growing and thicker-branched species of southern conifers still present similar structures. The axes of cones also deserve study in this respect, since I have observed that the pith of the cone of Pinus Strobus shows, though obscurely, a tendency to the formation of transverse dissepiments.

Applying the facts above stated to the different varieties or species of Sternbergia, we must in the first place connect with these fossils such plants as the Pinites Medullaris of Witham. I have not seen a longitudinal section of this fossil, but should expect it to present a transverse structure of the Sternbergia type. The first specimen described by Prof. Williamson represents a second variety, in which the transverse structure is developed in the central part of the pith, but not at the sides. In my Pictou specimen the pith has wholly disappeared, with the exception of the denser outer coating and transverse plates. All these are distinctly coniferous, and the differences that appear may be due merely to age, or more or less rapid growth.

Other specimens of Sternbergia want the internal partitions, which may, however, have been removed by decay; and these often retain very imperfect traces, or none, of the investing wood. In the case of those which retain any portion of the wood, sufficient to render probable their coniferous affinities, the surface-markings are similar in character to those of my Pictou specimen, but often vary greatly in their dimensions, some having fine transverse wrinkles, others having these wide and coarse. Of those specimens which retain no wood, but only a thin coaly investment representing the outer pith, many cannot be distinguished by their superficial markings from those that are known to be coniferous, and they occasionally afford evidence that we must not attach too much importance to the character of their markings. A very instructive specimen of this kind from Ohio, with which I have been favored by Prof. Newberry, has in a portion of its thicker end very fine transverse wrinkles, and in the remainder of the specimen much coarser wrinkles. This difference marks, perhaps, the various rates of growth in successive seasons, or the change of the character of the pith in older portions of the stem.

I have not been so fortunate as to find any of the Sternbergia or Artisia casts associated with the wood of plants allied to Lepidodendron, as observed by M. Corda. There are, however, in the collection of Prof. Newberry, as well as in my own, specimens which present very considerable differences in their external characters from those of the varieties known to have been coniferous, and which may be the axes of such plants.

The state of preservation of the Sternbergia casts in reference to the woody matter which surrounded them, presents, in a geological point of view, many interesting features. Prof. Williamson's specimen I suppose to be unique in its showing all the tissues of the branch or trunk in a good state of preservation. More frequently, only fragments of the wood remain, in such a condition as to evidence an advanced state of decay; while the bark-like medullary lining remains

In other specimens, the coaly coating investing the cast sends forth flat expansions on either side, as if the Sternbergia had been the mid-rib of a long, thick leaf. This appearance, at one time very perplexing to me, I suppose to result from the entire removal of the wood by decay, and the flattening of the bark, so that a perfectly flattened specimen, like that in fig. 6, may be all that remains of a coniferous branch nearly two inches in diameter. A still greater amount of decay of woody tissue is evidenced by those Sternbergia casts which are thinly coated with structureless coal. These must, in many cases, represent trunks and branches which have lost their bark and wood by decay; while the tough, cork-like, chambered pith drifted away to be imbedded in a separate state. This might readily happen with the pith of Cecropia; and perhaps that of these coniferous trees may have been more durable; while the wood, like the sap-wood of many modern pines, may have been susceptible of rapid decay, and liable, when exposed to alternate moisture and dryness, to break up into those rectangular blocks, which are seen in the decaying trunks of modern conifers, and are so abundantly scattered over the surfaces of coal and its associated beds in the form of mineral charcoal.

Some specimens of Sternbergia appear to show that they have existed in the interior of trunks of trees of considerable size. The best instance of this that I have found is that represented in fig. 7, from the South Joggins, and which appears to show the remains of a tree a foot in diameter, now flattened and converted into coal, but retaining a distinct cast of a wrinkled Sternbergia pith.

Are we to infer from these facts that the wood of the trees of the genus Dadoxylon was necessarily of a lax and perishable texture? Its structure, and the occurrence of the heart wood of huge trunks of similar character in a perfectly mineralized condition, would lead to a different conclusion; and I suspect that we should rather regard the mode of occurrence of Sternbergia as a caution against the too general inference from the state of preservation of trees of the coal formation, that their tissues were very destructible, and that the beds of coal must consist of such perishable materials. The coniferous character of the Sternbergiæ, in connection with their state of preservation, seems to strengthen a conclusion at which I have been arriving from microscopic and field examinations of the coal and carbonaceous shales, that the thickest beds of coal, at least in eastern America, consist in great part of the flattened bark of successive generations of coniferous, sigillaroid and lepidodendroid trees, the wood of which has perished by slow decay, or appears only in the state of fragments and films of mineral charcoal. This is a view, however, on which I do not now wish to insist, until I have further opportunities of confirming it by observation.

The most abundant locality of Sternbergia with which I am acquainted, occurs in the neighborhood of the town of Pictou, immediately below the bed of erect calamites described in the Journal of the Geological Society (Vol. 7, p. 194). The fossils are found in interrupted beds of very coarse sandstone, with calcareous concretions, imbedded in a thick reddish brown sandstone. These gray patches are full of well preserved calamites, which have either grown upon them, or have been drifted in clumps with their roots entire. The appearances suggest the idea of patches of gray sand rising from a bottom of red mud, with clumps of growing calamites which arrested quantities of drift plants, consisting principally of Sternbergia and fragments of much decayed wood and bark, now in the state of coaly matter too much penetrated by iron pyrites to show its structure distinctly. We thus, probably, have the fresh growing calamites entombed along with the debris of the old decaying conifers of some neighboring shore; furnishing an illustration of the truth that the most ephemeral and perishable forms may be fossilized and preserved, contemporaneously with the decay of the most durable tissues. The rush of a single summer may be preserved with its minutest striae unharmed, when the giant pine of centuries has crumbled into mould. It is so now, and it was so equally in the carboniferous period.

4. On the Newer Pliocene Fossils of the St. Lawrence Valley. By Professor Dawson, of Montreal.

The object of this paper was in the first place to notice several fossil shells recently found by the author and others in these deposits, and which did not appear to have been previously observed. The species mentioned were:—

Natica Heros, Say	Beauport.
Natica Grœnlandica, Beck	"
Fusus tornatus, Gould	Montreal.
Fusus harpularius, Couthoy	
Rissoa minuta	Montreal.
Turritella (like *erosa*)	Beauport.
Bulla oryza, Tott	Montreal.
Spirorbis sinistrorsa, Montagu	"
Univalve (perhaps *Menestho albula*)	

Most of these are shells now living on the Atlantic coast of America, north of Cape Cod, and some of them ranging very far north. The paper then referred to the distribution of the various kinds of drift in the vicinity of Montreal, and to the conditions of the sea areas, in which the shells and other marine animals of the Newer Pliocene period existed in the St. Lawrence valley. "Good evidence exists of a sea beach on Montreal mountain, at an elevation of 470 feet above the sea. The sea area corresponding to this beach must have extended to the Laurentide hills and the escarpment of Niagara, and communicated freely with the ocean on the east. On the other hand, there are lower shores of the same period only one hundred feet above the St. Lawrence. These must have belonged to a very narrow prolongation of the present Gulf of St. Lawrence. The conditions of climate, ice, drift, etc., corresponding to these different shores, must have been very diverse."

"Again, in the stratified drift, it is possible to recognize, within a few inches of each other a bed, containing deep sea shells, and others containing species that are littoral, these sea bottoms corresponding to different levels of the land. It is evident that any conclusions with reference to the climate indicated by the marine fauna of these successive beds of marine detritus, must take into account these fluctuations of the sea level, and the changes in animal life consequent on them. Taking these into account, positive and reliable results may be attained, and the study of such districts as the St. Lawrence valley may be made to contribute toward the elucidation of the conditions of life in older formations."

5. Fossils of the Sandstones and Slates of North Carolina. By E. Emmons, of Albany.

The inferences which may be derived from the fossils of the sandstones will be better appreciated by reference to the arrangement and relation of the rocks in which they are found.

In the ascending order, the masses and beds hold the following positions: —

1. Conglomerate, both coarse and fine, the coarser materials of which are derived from the broken-down veins of quartz of the auriferous slates of the Taconic system.
2. Red and gray sandstones, which are generally even grained and firm, but which are often separated by soft, disintegrating strata. The red color is replaced by gray, towards the bituminous slate which contains the coal seams. These masses are three thousand feet thick.
3. Bituminous slate, containing three coal seams, two distinct belts of black band, and one of argillaceous iron ore, fine clays, a few beds of sandstone, of which the most important lies between the upper and lower seams of coal, and is about sixteen feet thick. Towards the top of this mass, the bituminous slates alternate many times with magnesio-calcareous strata, which are mainly destitute of the cypridæ and posidonia, which are so common in the bituminous beds.

The whole mass is about six hundred feet thick. Beds of sandstone succeed the latter mass. The thickness above the slates is not well determined. To the latter succeed three or four beds of conglomerate, alternating with beds of fine blue, unbituminized slate, rich in Cycads and ferns, but which contain no animal remains whatever. Their whole thickness is about forty feet. For several hundred feet, however, the sandstones contain plants closely related to those of the unbituminized slates. Finally, the rock becomes a red sandstone, with soft, meshy, and calcareous layers; the whole, however, terminates in masses of very coarse brecciated conglomerates and sandstones.

The red sandstones predominate, and are very clearly divisible into lower and upper, each of which has its slate. In the upper, the slate is at the bottom and unbituminous, but in the lower, it is mostly bituminous, and is bounded below and above by gray sandstones, and is situated near the top of the mass. These rocks lie in a trough, which runs obliquely across the State, terminating in an apex in Granville county, near Oxford, and on the south, traversing a corner of Union county, and finally passing into South Carolina. From every point or side, the land descends to the outer rim of the sandstones. Their surface, together with a narrow border of the adjacent rocks, is overspread with rounded, loose pebbles, whose beds resemble a modern sea beach. These beds of pebbles may probably belong to the Eocene period. The formation is exceedingly variable in thickness. At Egypt, the Gulf, and Carbonton, or for about twenty-five miles measured along a crooked outcrop, or from Martin Dye's to Tooschec's, all the beds are present. But towards the north-east and south-west from these extreme points the lower masses thin out, so that at Lockville, on Deep river, both the lower sandstone and the bituminous slates, with their coal-beds, have disappeared; the lowest masses consist of the conglomerates and slates of the upper sandstone. These had been mistaken for the conglomerates of the lower sandstone, from which much confusion and perplexity had arisen. The most important fossils belonging to the foregoing series occupy two distinct horizons; namely, the bituminous slates of the lower standstone, and the non-bituminous slate at the base of the upper sandstone. The former contains remarkable fauna, which include both fish and reptiles, and a mammal. The fish are mostly Ganoids, with rhombic scales; but there is a large and important one furnished with circular scales and large plates, and which, from the structure of the circular scale, I have named Rabdiolepis, or *rodded scale.* Another Ganoid I have referred to the genus Amblypterus; it resembles very closely the Gyrolepis, Agassiz, of the Muschelkalk. There are already known six genera, one of which is a Tetragonilepis with large and highly polished scales.

The reptiles belong in part to the Thecodons, and are closely related to those of the Bristol conglomerate, England. Their teeth are not only set in distinct sockets, but the spinal cord had a moniliform structure, the canal through which it passed being distinctly enlarged immediately over the body of each vertebra, in which respect these Saurians differ from all others. One genus which has been discovered belongs probably to another family, inasmuch as it was furnished with osseous dermal plates, unlike those of the Teleosaurus. Besides the foregoing, there is also a Labyrinthodon, which has been named, by Prof. Leidy, Dictyocephalus elegans.

One of the foregoing genera has been referred to the Clepsisaurus of Lea; the other is evidently new, and has been named Rutiodon Carolinensis. The most important of the genera belonging to this formation has been referred to the genus Palaeosaurus of Riley and Scutchbury. Some of the teeth appear to be identical with the one figured by the authors referred to, but there are two or more varieties of form which are yet unknown in the Bristol conglomerate; hence there remains some uncertainty with respect to the correctness of the reference, because the standards of comparison are so few in the English formation.

The most interesting of all the fossils of these beds is the jaw of an insectivorous mammal. It differs, generally, from all mammals hitherto discovered. It is, however, remotely related to the Phascolotherium. It differs in one important particular from the latter, and also from the Amphitherium, in its having no incurvation at base. Hence it is supposed to belong to the pescented insectivora. It might at first be inferred, that this mammal would decide the age of the slates, and lead us to regard them as parallel with the stones field slate of the oolite. But it is evident that such an inference is unwarranted, as it is not even generically related to the foregoing mammals; it really determines nothing, as to age or period, of these slates. We must regard it, however, as the oldest mammal yet discovered, inasmuch as it is associated with the Thecodon Saurians.

The second important horizon of fossils is at least two thousand feet above the former, and furnishes only plants. The plant beds appear to indicate short periods of repose, interrupted by intervals of disturbance, during which the beds of conglomerates were deposited. The direction of the materials does not appear to be changed, for the kind of sediment does not differ materially from that which had been previously accumulating; yet there is probably a slight unconformity between the upper and lower sandstones; for the change from a fine to a coarse sediment indicates a change of level which must result in an unconformity; besides, it is apparent, there is an over extension of the beds which were deposited at this horizon.

The majority of the plants are Cycads, though the most common plant is a Calamites, supposed to be the C. arenaceus. The Equisetum columnare, which is common in the Richmond coal-field, is rare in North Carolina. The plant which I have named *Pterozamites decussatus* is supposed, by Prof. Heer of the University of Zurich, to be *Pt. longifolius.*

Among the ferns, the *Acrostachites oblongus*, Em., appears to be the same plant which has been referred to as the Pecopteris Whitbyensis. Its side veins, instead of being forked, are reticulated, as represented in my North Carolina Geological Report, and in the sixth part of the American Geology. The Pecopteris (Aspidites) bullatus, Bunbury, of the Richmond coal-field is, according to Prof. Heer, the P. Stuttgartiensis, and the P. Carolinensis, Em., is related to the Gutbiera angustiloba, Stern. It becomes G. Carolinensis, Heer. The result which I had come to, on an examination of all the fossils which had been found was, that the upper sandstone is equivalent to the Keuper of Europe. On this question, I am confirmed by the results of an examination of the plants figured in my North Carolina Report, by Prof. Heer, who remarks, in a note which I have received, that certain species, such as the Pterozamites longifolius, Equisetum columnare, and Pecopteris Stuttgartensis, are characteristic of the Keuper and Marves Irisces of Germany, France, and Switzerland; and certain others are closely related to species found in Europe, in the Keuper and lower Lias, but are all different specifically; but there are none which are really Oolitic, either in Virginia or North Carolina.

Such being the result of the examination of the flora of these plant beds, it is plain that it sustains the conclusion which I had arrived at, that the fauna of the bituminous slates and lower sandstone must be infra Liassic; indeed, the evidence which these beds independently furnish is conclusive; for, if none of the Saurians can be referred to genera which are known to be Liassic, but must be referred to the Thecodont type, and are related to them generically, the beds containing them must be parallel to those beds in Europe which contain the similar types of organization; otherwise, the fossils of a distant country furnish us with no safe guides for our researches. Another important fact is the total absence of all the Mollusca which belong to the Jurassic series in Europe, which, when it is considered, is true of a series seven or eight thousand feet thick, should not fail to impress the minds of geologists, that this negative fact militates strongly against the idea that our sandstones of Connecticut, Virginia, and North Carolina are of that age. Then, again, we should not lose sight of the fact, that the physical features of the sandstones of both continents have many things in common. The irregularities in thickness, the frequent repetitions of conglomerates and breccias, mark a period when aspects must have been quite similar. Towards the close of this period in the upper sandstone, bones of a bird have been found, and single Saurian bones, accompanied with a few fish scales. There is, however, a great barrenness, thus far, of fossils in the upper beds. You have no reason to expect that they will hereafter prove richer, since the upper part has been cut through by the North Carolina Central Railway, without exposing any new beds of fossils which were before unknown.

The question of the Permianage of the bituminous slates and lower sandstone must turn upon the age of the Bristol beds, England, which have been referred to. Some of the most distinguished European geologists now refer them to the Bunter period; and there are certain facts disclosed by the beds of North Carolina which favor this view. An examination and comparison of the Saurians, however, of the Keuper, Muschelkalk, and Bunter sandstones with those of Deep and Dan rivers prove the absence of the Nothosaurus and other Enaliosaurs of the Trias. It is true, the teeth of the Rutiodon resemble the former, but the jaw is quite different, and the Rutiodon is clearly not an Enaliosaur. While we may admit that the age of the lower beds of the sandstones under consideration is not fully established, yet it seems we are borne out in the conclusion that they are infra-Liassic, and furthermore, that the upper masses of North Carolina, at least, are nearly or quite parallel with the Keuper of Europe.

III. ZOÖLOGY.

1. ON THE STRUCTURE OF THE HEAD IN VERTEBRATA, AND ITS RELATIONS TO THE PHYLLOTACTIC LAWS. By THEODORE C. HILGARD, M. D., St. Louis, Mo.

THE MATHEMATICAL LAW OF PHYLLOTAXIS.

THIS well-known and remarkable law, established by Carl Schimper, in 1830, is, *in nuce*, the following: the NUMBER of leaves, scales, floral parts, etc., invariably forming distinct *sets*, whether by *singly* alternating around the stem, or as *whorls*, is always — with few exceptions, and which are secondarily thence derivable — one of these:

1, 2, 3, 5, 8, 13, 21, 34, 55, 89, 144, 233, 377, and so on — constituting a numeric series, which commences with the unit and progresses by the sums of each two ultimate members obtained. If placed at different heights, they regularly alternate in their respective radial positions in such a manner, that *immediately before an* (ichnographic) *repetition obtains, their sum amounts to one of the serial numbers*, and each two organs proximate in height, or along the stem, *throughout the set, diverge in the same direction of spiral*, and, by that number of ichnographic interstices, which, in the numeric series above given, is the *antepenultimate to the number of leaves comprised in the set*, or "cycle." Each new cycle commences with the element which repeats the position of the first member in the previous set, which, when the cycles repeat, may be assumed at any point, but, if otherwise, is to be counted from the element where divergence is different above and below, or the spiral assumes an opposite direction, as is sometimes the case between the various cycles of floral elements constituting the flower.

The number of elements, contained in a cycle of spiral disposition, divides the whole circumference into as many ichnographic interstices, each leaf, however, being at a different height. If designated by the ordinals as they *ascend* along the stem, if e. g. eight in number, their angular or *peripheric juxtaposition* will be the following, in the (*short-way*) direction of the spiral: I, IV, VII, II, V, VIII, III, VI, I, leaping three interstices, or two leaves, to arrive at the ordinal next in number; if five, they ichnographically succeed (in the short direction) thus: first, fourth, second, fifth, third, first, or lowest. This is called the law of DIVERGENCE.

My explanation of the *production* of these numbers and divergences, or *organic* law of Phyllotaxis, so kindly noticed by Prof. Peirce and Agassiz, at the last meeting and elsewhere, and more lengthily treated in the Proceedings of the St. Louis Academy of Science, is shortly this: —

Each foliar part, or phyllotactic element, originates, in the bud, of a single cell, at the termination of the succulent marrow-core, from amidst the others, in their very centre, and at their base. Cytogenesis, or origination of cells, always takes place out of a preëxistent one, according to modern researches. Hence any new element must spring up from within a cell, of either the marrow-core or an element already formed.

If the first element be supposed to produce the second, and the third between them, and that, at corresponding rates of maturity, each member commences and continues rhythmically to put forth single progeny, desisting by sets of coevals by rhythm, or, corresponding to the first element, then all the NUMBERS *of the series — and no others — are successively produced,* until checked by the effunction of the first cell, and, *corresponding to the same,* that of all the later ones; e. g., the fifth parturition of the first cell corresponding to the second one of the third member, and to the first one of Nos. IV and V. Each one later in degree and number is later in fulfilling its term, as the rudera of leaves spring up successively, and *not simultaneously* by sets of coevals. No. I, if effunct at the 5th term, and Nos. IV and V, correspondingly, at their first, then a cycle of 13 foliar elements is obtained.

If every issue be deposited on the side towards the ELDEST *of immediate neighbors, then the lawful* DIVERGENCE, *or, interpolation of new parts between the old ones,* (in their centre, originally, but assuming such radial direction,) *is effected.*

This suggested mode of *production* of the phyllotactic numbers and divergences, we would call the "GYRAL LAW," being a system of *elementary generation in ascension around the centre,* and *by distinct genealogies; a spiral progress of generation,* confirmatory of the view, that the progress both of *physical and dynamic light* — of *power and* *life*—ensues *in a spiral motion;* a view propounded by the original founder of the "atomic theory," the learned E. Swedenborg.

TORSION OF FIBRE.

My theory of the successive production and interpolation of subsequent elements at once affords a clue to the universal phenomenon of a natural "*warp*" or *torsion of fibre* in stems. Climbers twine in a direction *opposite to that of their torsion*, and to the latter the climbing itself is generally ascribed. If justly so, an actual torsion or twisting *force* would be required.

The *infra-foliar* respective *sectors of stems, developing under the influence of leaves*, in climbers are soft and slender; hence, the new elements, if *actually interposed*—as my theory requires—between more ancient and rigid parts, by their own *increase, while sprouting*, would be *distorted towards the free side*, which is in the direction of the spiral, while their base, far beneath, remains comparatively fixed, so as to produce a *continuous warp*, and that in the direction of the *phyllotactic spiral;* not identical with it, but having the same cause, and being considerably less in its amount, which is superadded to that of the foliar spiral.

If the torsion of fibre be actually derived from this process of origination, then the *coil* of the climber must be *opposite to the foliar spiral*, the latter requiring to be coincident with that of the fibre; and, as far as my observation goes, this opposition of coil and foliar spiral seems to obtain.

From the above, it would follow that there must be no coiling climbers with *whorls immediately corresponding*, or *crosswise either;* a *spiral alternation* of whorls being required, in verticillate stems, to produce a torsion.

The *rotary motion* of the drooping flagelliform *tops of vines*, abundantly established by observation, at once finds its explanation in this confirmed supposition of an actual *process of torsion.* When a resistance is met with, and the tip does not slip by, but is checked, then, probably by the very power which forces the top of the limb round its longitudinal axis, the *coiling* commences.

If such a limb be artificially coiled round a pole in a direction opposite to the one *each species of climber invariably maintains*, it *resists* that coil, and, if loosed after a while, *whirls round in the opposite direction*, no doubt by its very process of development, as above assumed; and if artificially kept in a wrong coil, its progress of growth is perceptibly *checked;* all of which is confirmatory of the theory, that it is the *crescive expansion which causes torsion.*

The *Direction of Spiral* is known to be one and the same in the same species of climbers.

In branched plants, trees, etc., the spiral direction is different in different branches, but said to average about equal odds in the whole tree.

The first *material* sign of the determination of spiral direction being the *position assumed by the third element*, — a late comer, and recondite within the most intrinsic and vital parts; it is likewise evident that *the first cause of that direction must be predetermined within the* BIODYNAMIC CONSTITUTION, and not caused by conditions extraneous to the vital peculiarity of the vegetable, the result being invariably the same, under whatever physical conditions placed, if not, as a factor, complicated with the vital phenomena themselves, to which, hence, the actuating cause requires to be referred.

CONCLUSION. — TRANSMITTED PROLIFICACY.

The very *termination of cycles by the serial numbers*, if produced by the cytogenetic process assumed, is demonstrative of a strict relation of every later issue of rudiments to the activity of the *first one*, and, likewise, of a coördinate equality of prolific power among the members of *equal grade* (although not time) *of descent;* the same number of parturitions being reserved to each member of the same grade, although brought to action gradually and *successively*, not by a simultaneous act. From this *solidarity of cycles*, and prolificacy *graduated and diminished in direct proportion to genealogical degree*, it would appear that the prolificacy of subsequent elements was *derived from the preceding ones*, by a sort of continuous "*seminal process without fructification*," (*vide* "Weisungen ueber die mosaische Schoepfungsgeschichte," p. 436, Wien, bei M. Auer, 1855,) a sort of "pullulation" of cells, upholding the vital processes and *change of substance* or actual *regeneration* of the living body itself, by cytogenesis, — and as a production of new *individuals* known in the genus Aphis, in the "change *of* generations," and in so many well-authenticated cases of botanical "parthenogenesis," as a *generation without material sexuality.*

A *first cell*, a *graduated prolificacy*, and a *cessation of prolification*, require a recommencement, by a new *cell progenitorial* of a future cycle, assuming in its first element a *certain position* toward that of the previous cycle; with *leaves*, a *corresponding* radial position; with *flowers*, mostly an *alternate* one, frequently combined with a change in the direction of spirals, but not among organs of equal function, as, e. g. repeated cycles of *petals or stamina.*

Thus, by its number and disposition, the *cycle* appears demonstrable as a thing *objectively constituted.*

ENCEPHALIC CYCLE.

The brain, embryology shows, originates in a *circle of three little vesicles*, to which, subsequently, two more are added, and *interpolated* in a *gyral sense;* the 4th, between the first and second; the 5th, between the second and third. The first vesicle subsequently develops into the "*medulla oblongata*," with the "*corpora olivaria*" as a pair of ganglia or "*nuclei*," and gives rise to all the tactile and motor nerves of the sensuous organs.

The second vesicle develops into the "*corpora quadrigemina*," replete with "nucleolar" (*grey*) substance, and, together with the *gustative* pair of nerves, *arises from the first element.*

The third vesicle develops into the "*corpora striata*," and gives rise to *olfactory* nerves.

The fourth section of the brain, the "*cerebellum*" with the "pons Varolii," arising between the first and second element, passes the *auditory* nerves. It contains a pair of "*nuclei cerebelli.*"

The fifth section, arising between the second and third, develops into the major part of the brain, with the "*thalami optici*," as ganglia, giving rise to the *optic* nerve.

The cerebral *hood* or circle, afterwards by development, stretches out into a straight line, perhaps as the foliar elements *arise along a scion*, or, by a *rupture of the cycle* between the first and third elements.

The segments of the brain seem to *originate* in the ascending succession, in which their corresponding senses are, *by common agreement*, placed: 1st, taction; 2d, gustation proper; 3d, olfaction; 4th, audition;

5th, vision. But the *local* succession of themselves and their *contingent cerebral vertebræ*, is that of gyral *ordinals along the periphery:* 1st, 4th, 2d, 5th, 3d, — taction, audition, gustation, vision, olfaction, — like a ring gyrally developed as a quinary cycle, *ruptured between the first and third element*, and somewhat stretched, much in the fashion of the apex of a *crosier.*

A *simile* of this obtains in the successive lengths of the five fingers; if the thumb be accounted the second in magnitude, they follow thus: middle-finger, thumb, gold-finger, index, little finger, — like the calyx of a rose-bud if *ruptured between the fifth* (smallest) *and second* element.

STRUCTURE OF THE HEAD IN VERTEBRATA.

Interpretation of the Skull.

Oken and Carus were the first to explain the segments, forming the cranial cavity, as *homologues to vertebræ.*

Each vertebra consists of five essential parts: one vertebral *block* ("body"); two *flank*-pieces ("neurapophyses"), with an incision or perforation transmitting a proper pair of nerves; and two *arch or top*-pieces ("neural spine"). These parts originate singly, and meet by sutures; and in the spine, mostly by a substantial coalescence, subsequently unite where they meet.

There are also a pair of "*transverse processes*" ("pleurapophyses") at the base of the arch. On the cranium they appear as mere *ridges*, apparently proper to the vertebral *bodies*, and by pairs *corresponding to their original number.*

Oken recognized four vertebræ or neural arches. The first, he considered the occiput, with the *basilar bone* or "*clivus Blumenbachii*," for a block, with the "*pars petrosa*" of the temporal bone, on either side, for its *flank*-pieces, perforated by the auditory nerve, and with the tabulate part as its *vault* or "neural spine."

The second, according to him, was constituted by the "*sphenoid*" bone and parietal bones, the former consisting of a *block* and two "*alæ magnæ*," with a posterior incision for the passage of the glossopharyngeal nerve. Besides, the "*processi ensiformes*" on either side, as *flank*-pieces of the "visual" vertebra, give passage to the optic nerve; the *arch*-pieces to which are the frontal bones; and the vertebral *body*, distinct in animals, is contiguous to that of the sphenoid bone, and, in man, much reduced and coalesced with it.

This was the third ring. As the fourth, he considered the ethmoidal bone, with the *crista galli* and *lamina perpendicularis* for a body, the perforated sides as flank-pieces, and the nasal bones as arch-pieces; the latter, however, receding from the cranial cavity.

Carus adopted six vertebral rings, — three to form the *cranium*, three the *face*, — basing on the tabulate facial bones of *Myxine*, a cartilaginous fish. In all others, however, the facial bones are developed after the type of *extremities*, which likewise, in an embryonic state, first appear as *slabs*.

According to Oken, each of the cranial vertebræ gives passage to one *pair of specific nerves;* claiming, on anatomical grounds, the glossopharyngeal nerve as that of gustation, — the real function of the *lingual sense* then not being known, and its nerve remaining in litigation.

As I have demonstrated at a former meeting (Proceedings of 1854), the *specific* lingual sense, residing in the surface of the *tongue* and *soft palate*, comprises merely *sour*, *sweet*, *salt*, *bitter*, — all the rest being olfactions. When the glossopharyngeal nerve has been severed, animals no longer reject bitter food. Bitterness being tasted *all over* the tongue and soft palate, it was concluded by Mueller, that the trigeminate nerve ("nervi palatini") was concerned in this specific gustation, and all followed his accidental mistake. But the glossopharyngeal nerve supplies not only the surface of the tongue with its *sensitive* fibres, but *also the soft palate*, by numerous branches from its *circulus tonsillaris*, — and *no other parts.* Thus the nerve, I claim, covers the *whole* area, and does *not extend beyond*, — which I beg to correct in my previous essay.

ACTUAL NUMBER OF CRANIAL VERTEBRÆ.

A comparison with the five specific cycles of the *flower* — of which hereafter — seemed to require a *fifth*, or rather another *first* vertebra of the skull, and that must be subservient to the *cutaneo-motor* system, or the sense of *touch* and fleshy fabric.

The proposition once conceived, I found the following proofs in confirmation.

FIRST AND SECOND VERTEBRA.

The posterior half of the basilar bone is marked with a pair of ridges, or *transverse processes*, corresponding to an anterior one, and by their presence, demonstrative of *a proper vertebral body*, of which the flank-pieces are the condyloid processes, perforated by the *motile* hypoglossic nerve, as a homologue to the *os petrosum* of the auricular vertebra, which I hold to be the flank-piece of the *second* in succession.

The *arch*-pieces of the *first* vertebra, I first discovered in a withered lamb's skull, with a *cross suture across the occiput*, thus divided into two sets of top-pieces; the first set, *proceeding from the condyle*, with a *median suture* in the longitudinal dividing line of the cranium; while the rhombic anterior part of the occipital bone, severed from it, remained as an apparently single piece, its median suture being fused, as in the insequent parietal bones of the sheep.

The suture dividing the arch-pieces of the first, or "motile," from those of the second, or "auditory," vertebra, obtains, perhaps, in *all mammals*, the first being generally more strongly developed, and serving for the insertion of the powerful neck-muscles, (compare cat and musk-rat,) while the actual vault of the auditory vertebra, under stress of the temporal bones, in the cat, is reduced to the appearance of a small triangular intercalary osselet; still being the strict homologue of the rhombic vault-piece, or "key-stone," (Owen,) in the cranium of fishes, mammals, etc.

INFANT'S SKULL.

All these homologous parts and their arrangement, I found very plainly expressed in the infant European skull, viewed from the inside. The flight of *five* pairs of perforated flank-pieces, like the ribs of a ship, and in homology to the *bicapitate ribs* of the "hæmal arch," is very evident. The occipital *squama* is *almost cross-divided* by two fissures, and Tschudi informs us, and figures, that even the adult *skull of ancient Peruvians* has a *transverse suture across the occiput.* It is *this* suture which divides the occiput of vertebrata, leaving the "keystone" to represent the fused arch-pieces of the second vertebra.

TEMPORAL BONES.

Apart of its perforated *pars petrosa* being the *neural rib*, or "neurapophysis," to the auditory vertebra, the temporal bone typically consists of *three separate elements*, generally forming a *glenoid cavity* for the reception of the lower jaws. In the fish skull, it is inserted into the large crevices between the vertebral elements proper, thus forming part of the cranial vault; in the sheep, cat, and infant skull, it can be cleft away without considerably opening the cranial cavity. The temporal bones invariably are the *tripartite coxal apparatus* of the lower jaw, or *extremity belonging to the lingual vertebra*.

NASAL VERTEBRA.

The skull of *Catostomus* (buffalo-fish of the Mississippi) has this vertebra largely developed in a long block-piece, a pair of perforated flank-pieces, and a fused tabulate top-piece. This top-piece, by a downward crest, *forms the partition between the ethmoidal bones*, and by a suture is *severed from the block* of this vertebra. It remains the true homologue of the "*crista galli*" and "*lamina perpendicularis*." In amphibia, birds, and mammals, its top-pieces *remain fused*, not distinct, as the nasal bones are, and are gradually *overlapped by the frontal bone* (birds), so as to be *withdrawn from the surface*, forming an actual part of the cranial cavity, in the shape of the *median ridge of the ethmoidal bone*—the *block* of the nasal vertebra being very much shortened, and found between the *processi ensiformes* of the sphenoid bone and the commencement of the *lamina cribrosa* of the ethmoidal bone.

FISH-HEAD.

My (hitherto hypothetical) "*first vertebra*" is developed largely and *independently* in all its parts in the skull of the cat-fish (Pimelodus), whilst *preserving all the homologies to the posterior part of the occiput* of other fishes and vertebrata in general.

In the fish, the skull, at its flat base, destitute of palatal processes, harbors a cylindric *cephalothorax* of *five* pair of ribs, bearing the fringes of the gills, and in number corresponding to the *five vertebræ* of the skull, as here assumed. These ribs are loosely attached to the base of the skull at their upper end, and below meet in a *sternum.*

Besides these ribs, *each cranial vertebra*, except apparently the "first" one, *bears one pair of appendages*, each being a perfect homologue to the *pairs of extremities* of the trunk.

Oken demonstrated the osseous ring behind the gill-slit to be the true homologue of the *upper extremities*, inasmuch as in higher animals it *gradually recedes*, forming itself the *shoulders and arms.* In the fish, it arises from the *auricular* vertebra's flank-pieces.

NASAL EXTREMITIES.

In *Catostomus* the nasal vertebra bears a finely developed pair of *movable extremities*, by which the snout is porrected. In other fishes, and in amphibia, it is seen to gradually merge into the *nasal bones* and intermaxillaries.

VISUAL EXTREMITIES.

The skull of the *cock* shows, at the anterior end of the large orbit, one tripartite, coxal apparatus, which belongs to the intermaxillaries, and another, the temporal coxa, at its posterior end. Besides, from *within* the orbit emerges a striking analogue of the bird shoulder and wing (arm): a stout *clavicle*, at its top bearing an *uncinate process*, and a slender *zygomatic blade*, giving rise, at their point of junction, to a palatal homologue of a *humerus*, its *elbow* touching that of the other side, and issuing into a palatal fore-arm. The similarity is so striking, that, if detached, it might be passed for the shoulder and wing of a large humming-bird.

In the fish, this pair of extremities, much resembling in its structure the *digitate antennæ* of insects, is removed downwards, its coxal apparatus forming a sort of *manubrium sterni* to the cephalothorax, as, in man, the upper extremities, by their clavicles, join the *sternum* and thoracal fabric. This visual extremity of fishes is porrected backward like the legs and feet of the seal, and its numerous digitate *rays* are applied to the lower jaw, forming part of the lid of the gill-slit. In poisonous snakes, these rays form the *erectile fangs*, movable on a sort of foreshortened hand and *arm.* In mammals, this structure retreats toward the vertebral blocks, its clavicle, humerus, and uncinate process forming, on either side, the *pterygoid process of the sphenoid bone.*

In birds, the lower jaw, being *dislodged from the temporal bones* — its proper coxa — also *hinges on the pterygoid coxa,* together with the palatal extremity properly thereunto belonging, the lower jaw being *suspended from the temporal bones* merely by muscles and sinews.

In the fish, we find the large lid-bone of the gill-slit apparently belonging to a pair of extremities, perhaps as a *phalangal* bone, and perhaps of the eye extremity. In *Ephippium,* as remarked by zoölogists heretofore, this plate is seen to gradually be *converted into the tympanic bone,* and *tympanum* itself.

THE GUSTATIVE EXTREMITIES

are the temporal bones, as a coxal tripod, and the lower jaw as its members.

THE AURICULAR EXTREMITIES

are the osseous ring, dividing the head from the body, and situated directly behind the gill-slit. They, originally, in fish, arise from the auricular vertebra, to which they are firmly attached, and in higher animals gradually assume the development and position of the *upper extremities,* by *displacement from their respective vertebra.*

In the cat-fish (Pimelodus), the uncinate process of the auricular coxa, or *shoulder,* forms the stout ray of the pectoral fin. The pectoral *plate,* strongly indented (*sutured*) with the opposite one and closing the ring, as the palatal humeri likewise approximate with their elbows, is the auricular or proper *humerus,* by analogy with *Catostomus,* etc., and is probably *the analogue of the so-called lower jaw of insects.*

LOWER EXTREMITIES.

Another pair of extremities — the "lower," or *pelvic* one — remains to be *referred to its originating vertebra,* while the first cranial vertebra, so conspicuously developed in *Pimelodus,* from analogy seems to require a *corresponding pair of extremities.* No doubt the pelvic fins, hind legs, or lower extremities, are the *ones corresponding to the first* *cranial vertebra*, but are removed like the visual one in the fish, and the auricular one, or arms, in higher animals.

In the human fabric—the last and highest of vertebral types known—the whole of the vertebral system is received into the appendages of the head, namely, between the nasal bones, the jaws and the extremities. Man is erected within his head, as it were.

PARALLELISM BETWEEN INTELLECTUAL AND SENSUOUS CONCEPTIONS.

There is a natural correspondence between the divisions of the brain and their specific nerves, as above demonstrated, on the one hand, and between the intellectual functions and sensuous functions on the other—"feelings" being compared to feeling; "tastes," or appreciation of the *congenial* and *pleasurable*, to gustation and olfaction; "ear" and "audition" to *comprehension*, the extremities, proper to its vertebra, the arms and hands being likewise the *prehensile* ones; *discrimination* to seeing. It is not improbable that a consistent theory of psychology and psychic organology may spring from the comparison between intellectual and sensuous processes and organs on the one hand, and organologic developments of flowers on the other, as I would indicate by the following outlines of a comparison between these four: the sensuous apparatus, the sexual organism, and the digestive system, on the one hand, and the *five cycles of perfect flowers*, on the other,—a parallelism first essayed by the late Oken.

THE ANTHIC LAW.

The perfect flower, such as the rose and apple, consists of *five cycles*, which *successively alternate in position*, except the last, the *embryonic* one, or seed, which is contained within the fourth or *utricular* one, *i. e.*, the capsule, alternate to whose lobes are the *antheric*, *glandular*, *or punctiferous* (celluliferous) organs, to which the calyx, first or *somatic* cycle, corresponds in position; while the second, or *ligulate* cycle, the corolla, alternating with the former, in its position, is coincident with both the utricular and the embryonic ones. The calyx and antheral lobes, forming one quinal star, alternate with the other, which comprises the petals, follicles, and seeds.

Want of space does not now permit me to give more than a mere statement of correlations, the stringency whereof I must leave for another occasion to develop, but give a short synopsis, in order to complete the schematic interpretation of the structure of the head.

In the animate body, the calyx is represented in the *cutaneous* or truncal lobes; the flower, in the *ligulate;* the antheric, in the *glandular;* the capsular, in the *utricular;* the embryonic, in the *disciferous*: the lenses, tympani and glottis, and the embryo.

The sexual organs are known to exactly correspond in the sexes after the following manner: the uterine segments and seminal vesicles are homologues; so are the ovaries and *testes*, the *corpora cavernosa clitoridis* and *do. penis*, and, to my views, the *labia interna*, as a single, erectile, ligulate or laminate element, to the *corp. cav. urethræ.*

Oken demonstrated a correspondence between the external earducts and the tubæ; the internal ear-ducts, (*tubæ Eustachianæ*,) and the womb-segments; the *parotides* and the ovaries and *testes;* and between the tongue and erectile laminæ. I commence with the utricles, as the centre, harboring, at their opening, like a matured *seed*, or *focus of life*, the discoid organs, and appendaged with the corresponding *laminate* or ligulate ones, as with their contingent petals, between which enter the *cutaneous lobes* of embryology, enclosing each a corresponding *gland* or *antheral* organ.

In the head we have the two *choanæ and lachrymal canals* as the first *pair of utricles*, harboring, at their extremities, the two *lenticular systems* of the eyes, *wrapt* into the laminal appendages, the *eye-chalyces* of the embryonic state, which afterwards close up, by a suture, to eyeballs. The second pair, likewise meeting at the fauces, are the *internal ear-ducts*, with *tympani* as discoid organs, and *external ducts and conchs* as ligulate appendages. The *odd* utricle is the *fauces*, originally distinct (by *atresia*) from the intestines. The *ligulate appendage* of the faucal utricle is the *tongue;* its *disc*, the *glottis*. Their respective *stigmas* are the epiglottis, the ear-valves, and the lachrymal caruncles.

The *odd* cutaneous lobe, in the embryonic state, is the forehead, nose, and central piece of the upper lip. As its gland, it contains the osseous intricacies of the nose, and, chiefly, the *lungs*. The insequent *pair of* *lobes* are the *cheeks*, containing, as their glands, the parotides. In the angle between the cheeks and forehead, the eyes are located by the above law of *alternation of cycles.*

The *second pair* of cutaneous lobes are those which ultimately unite at the chin, and probably (*musculus platysma myoides*) comprise the *mammal* lobes and their respective glands. In the angle between them and the cheeks the external ear obtains, and *between* them the tongue is *included.*

The odd sexual *cutaneous lobe* proceeds from the umbilicar region, and forms the prepuce, with a *frenulum* indicative of a "ventral suture" of margins. To it correspond the *kidneys* as *antheroic and gland*, with the lungs as their paragon. In the angle between it and the *labia externa*, or scrotal lobe, end the *ligamenta uteri teretia*, of an originally utricular character, and terminated by the erectile laminæ, called *corpora cavernosa clitoridis*, homologues to the *corp. cav. penis*, in the males. The first pair of cutaneous lobes include the *testes*, whose homologues are the ovaries. The second pair of lobes are the *nates*, containing the *glandular Cowperi s. Berthelinianæ.* Between them and the scrotal lobes no doubt belong, schematically, the *tubæ*, with the segments of the *womb as utricular elements.* They are, however, retracted into the intestinal cavity, and correspond to the ears no less by this recondite position. Between the nates, in the suture, appertains the *corp. cav. urethræ* in the male, the doubled *labian internum* — a simile to the odd labellum of orchids — of the female. The odd of the five utricular organs is the *vagina* and its correspondent in the male, with the tripartite *hymen* as *discoid element* (compare *tympanum*), and giving rise to the odd one of erectile laminæ.

We return to the glands. Besides the glandular cycle above mentioned, a second glandular or antheral one obtains in the facial, as in the sexual "flower." This second cycle is, of course, coincident with the laminal appendages, the *thyreoid gland* with the tongue, the *tonsils* with the ears, or, in the embryonic state, gill-slits, the *lachrymal glands* with the eyes.

The cutaneous nerves pervade the *skin* and fabric generally, being subservient to localization. The gustative nerve expands on the *lamina*, the tongue; the olfactory one, in the osseous *sinuosities* (glandular?); the auditory, on the continuation of the *utricle* into the "*os pe-*

trosum"; the *optic*, on the surface of the transparent system of refraction.

Thus it appears that each of the specific sensations, *by its organs* and sexual homologues, as well as by their intellectual correspondents, has a specific relation to the *cycles* of the flower, to its *functions* and *form generally*. Therefore the idea may not be void of plausibility that the *special* forms of flowers, or combination of their elements, may also correspond to the *subdivisions* of our sensations and intellectual processes, and their combination into a whole to intellectual combinations, as hinted above.

The five floral cycles correspond to the five nutritive systems, which take their rise, embryonically, from the navel. Reproduction or "regeneration," *development* and *change of substance*, may be said to be essentially a process of *generation*, likewise, as hinted above.

From a simple stretched vascular duct, the five-valved and four-chambered *heart* is developed; from a short and straight intestine, the tortuous *bowels*, with five chief sections, *œsophagus*, *stomach*, *duodenum ileum*, *colon*. To the vascular system belongs probably, as an *independent fifth utricle*, or chamber, the *vena portarum*. Two *glands Thymus*, *pancreas* and *glands suprarenales* are a set, and the spleen and lymphatic glands another one, while the fauces and lungs above the intestine, and the bladder, liver, and kidneys below it, belong partly to the sensuous, partly to the sexual system.

A minute study of the gradual development of the intestinal organs would no doubt serve to throw more light on their systematic construction. From a comparison with the corresponding organs of the head and pelvis, the *skeleto-kreatine* part of the trunk may correspond to the cutaneous system, whose function is *localization;* the *mucous* ducts, by a comparison with the *vagina* and *urethra*, as surfaces of ligulate organs, to the second, gustative, or *assimilatively testing* cycle; the lobed liver, spleen, and pancreas to the *glandular or vitalizing* cycle; the lymphatic and *vascular utricles*, centering in the *heart and portal vein*, to the fourth or *vibrating and conducting* cycle; and that ocean of *vitalized discoid lenticles*, the blood, to the fifth or *fociferous* cycle; the systems and organs of *regeneration* affording an evident analogy to those of *generation* and the *anthic fabric* generally.

IV. ETHNOLOGY.

1. Thoughts on Species. By James D. Dana, of New Haven, Conn.

While direct investigation of individual objects in nature is the true method of ascertaining the laws and limits of species, we have another source of suggestion and authority in the comprehensive principles that pervade the universe. The source of doubt in this synthetic mode of reaching truth, consists in our imperfect appreciation of universal law. But science has already searched deeply enough into the different departments of nature, to harmonize many of the thoughts that are coming in from her wide limits; and it is well, as we go on in research, to compare the results of observations with these utterings of her universality.

I propose to present some thoughts on species from the latter point of view, reasoning from central principles to the circumferential, and, if I mistake not, we shall find the light from this direction sufficiently clear to illumine a subject which is yet involved in doubts and difficulties.

The questions before us at this time are, —

1. What is a species?
2. Are species permanent?
3. What is the basis of variations in species?

1. *What is a species?*

It is common to define a species as *a group* comprising such individuals as are alike in *fundamental* qualities; and then, by way of elucidation, to explain what is meant by fundamental qualities. But the idea of a group is not essential; and moreover it tends to confuse the mind by bringing before it, in the outset, the endless diversities in individuals, and suggesting numberless questions that vary in answer for each kingdom, class, or subordinate group. It is better to approach the subject from a profounder point of view, search for the true idea of distinction among species, and then proceed onward to a consideration of the systems of variables.

Let us look first to *inorganic* nature. From the study of the inorganic world, we learn that each element is represented by a specific amount or law of force; and we even set down in numbers the precise value of this force as regards one of the deepest of its qualities, chemical attraction. Taking the lightest element as a unit to measure others by, as to their weights in combination, oxygen stands in our books as 8; and it is precisely of this numerical value in its compounds: each molecule is an 8 in its chemical force or law, or some simple multiple of it. In the same way, there is a specific number at the basis of other qualities. Whenever, then, the oxygen amount and kind of force was concentred in a molecule, in the act of creation, the species oxygen commenced to exist. And the making of many such molecules instead of one, was only a repetition in each molecule, of the idea of oxygen.

In combinations of the elements, as of oxygen and hydrogen, the resultant molecule is still equivalent to a fixed amount, condition, or law of chemical force; and this law, which we express in numbers, is at the basis of our notion of the new species.

It is not necessarily a different amount of force; for it may be simply a different state of concentration, or different rate or law of action. This should be kept in mind in connection with what follows.*

The essential idea of a species, thence deduced, is this: a *species* corresponds to *a specific amount or condition of concentred force, defined in the act or law of creation.*

Turn now to the organic world. The individual is involved in the germ-cell from which it proceeds. That cell possesses certain inherent qualities or powers, bearing a definite relation to external nature, so that, when having its appropriate nidus or surrounding conditions, it will grow, and develop out each organ and member to the completed result, and this, both as to all chemical changes, and the evolution of the structure which belongs to it as a subordinate to some kingdom, class, order, genus, and species in nature. The germ-cell of an organic being develops a specific result; and like the molecule of oxygen, it must correspond to a measured quota or specific law of force. We cannot apply the measure, as in the inorganic kingdom, for we have learned no method or unit of comparison. But it must nevertheless be true, that a specific predetermined amount, or condition, or law of force, is an equivalent of every germ-cell in the kingdoms of life. I do not mean to say that there is but one kind of force; but that whatever the kind or kinds, it has a numerical value or law, although human arithmetic may never give it expression.

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* When we have in view oxygen and the elements, we are apt to think of their molecules as distinguished by a different *amount* and *kind* of force. But when we consider the many different compounds that may be made of the same elements (as carbon and hydrogen), in the very *same* proportions, we are led to conceive of these as differing molecularly in a different *law* of the same force or forces. When, again, we see the same element under conditions as diverse as any two compounds, as in cases of allotropism, we are still better satisfied with adopting, for the present, the most general expression — a different law of action or condition of molecular force.

A species among living beings, then, as well as inorganic, is based on *a specific amount or condition of concentred force defined in the act or law of creation.*

Any one species has its specific value or law of force; another, its value; and so for all: and we perceive the fundamental notion of the distinction between species, when we view them from this potential stand-point. The species, in any particular case, began its existence when the first germ-cell or individual was created; and if several germ-cells of equivalent force were created, or several individuals, each was but a repetition of the other: the species is in the potential nature of the individual, whether one or many individuals exist.

Now in organic beings — unlike the inorganic — there is a cycle of progress involving growth and decline. The oxygen molecule may be eternal as far as any thing in its nature goes. But the germ-cell is but an incipient state in a cycle of changes, and is not the same for two successive instants; and this cycle is such that it includes in its flow, a reproduction, after an interval, of a precise equivalent of the parent germ-cell. Thus an indefinite perpetuation of the germ-cell is in fact effected; yet it is not mere endless being, but like evolving like in an unlimited round. Hence, when individuals multiply from generation to generation, it is but a repetition of the primordial type-idea; and the true notion of the species is not in the resulting group, but in the idea or potential element which is at the basis of every individual of the group; that is, the specific law of force, alike in all, upon which the power of each as an existence and agent in nature depends. Dr.

Morton presented nearly the same idea when he described a species as *a primordial organic form.*

Having reached this idea as the starting point in our notion of a species, we must still, in order to complete and perfect our view, consider what is the true expression of this potentiality. For this purpose, we should have again in mind, that a living cell, unlike an inorganic molecule, has only a historical existence. The species is not the adult resultant of growth, nor the initial germ-cell, nor its condition at any other point; it comprises the whole history of the development. Each species has its own special mode of development as well as ultimate form or result, its serial unfolding, inworking, and outflowing; so that the precise nature of the potentiality in each is expressed by the line of historical progress from the germ to the full expansion of its powers, and the realization of the end of its being. We comprehend the type-idea only when we understand the cycle of evolution through all its laws of progress, both as regards the living structure under development within, and its successive relations to the external world.

2. *Permanence of species.*

What now may we infer with regard to the permanence or fixedness of species from a general survey of nature?

Let us turn again to the inorganic world. Do we there find oxygen blending by indefinite shadings with hydrogen or with any other element? Is its combining number, its potential equivalent, a varying number, — usually 8, but at times 8 and a fraction, 9, and so on? Far from this, the number is as fixed as the universe. There are no indefinite blendings of elements. There are combinations by multiples or submultiples, but these prove the dominance and fixedness of the combining numbers.

But further than this, fixed numbers, definite in value, and defiant of all destroying powers, are well known to characterize nature from its basement to its top-stone. We find them in combinations by volume as well as weight, that is, in all the relations of chemical attraction; in the mathematical forms of crystals and the simple ratios in their modifications, — evidence of a numerical basis to cohesive attraction; in the laws of light, heat, and sound. Indeed, the whole constitution of inorganic nature, and of our minds with reference to nature, as Professor Peirce has well illustrated, involves fixed numbers; and the universe is not only based on mathematics, but on finite determinate numbers in the very natures of all its elemental forces. Thus the temple of nature is made, we may say, of hewn and measured stones, so that, although reaching to the heavens, we may measure, and thus use the finite to rise toward the infinite.

This being true for inorganic nature, it is necessarily the law for all nature, for the ideas that pervade the universe are not ideas of contrariety, but of unity and universality beneath and through diversity.

The units of the inorganic world are the weighed elements and their definite compounds, or their molecules. The units of the organic are *species*, which exhibit themselves in their simplest condition in the germ-cell state. The kingdoms of life, in all their magnificent proportions, are made from these units. Were these units capable of blending with one another indefinitely, they would no longer be units, and species could not be recognized. The system of life would be a maze of complexities; and whatever its grandeur to a being that could comprehend the infinite, it would be unintelligible chaos to man. The very beauties that might charm the soul would tend to engender hopeless despair in the thoughtful mind, instead of supplying his aspirations with eternal and ever-expanding truth. It would be to man the temple of nature fused over its whole surface and through its structure, without a line the mind could measure or comprehend.

Looking to facts in nature, we see accordingly everywhere, that the purity of species has been guarded with great precision. It strikes us naturally with wonder, that even in senseless plants, without the emotional repugnance of instinct, and with reproductive organs that are all outside, the free winds being often the means of transmission, there should be rigid law sustained against intermixture. The supposed cases of perpetuated fertile hybridity are so exceedingly few as almost to condemn themselves as no true examples of an abnormity so abhorrent to the system. They violate a principle so essential to the integrity of the plant-kingdom, and so opposed to nature's whole plan, that we rightly demand long and careful study before admitting the exceptions.

A few words will explain what is meant by perpetuated fertile hybridity. The following are the supposable grades of results from intermixture between two species:—

1. No issue whatever — the usual case in nature.

2. Mules (naming thus the issue) that are wholly infertile whether among themselves or in case of connection with the pure or original stock.

3. Mules that are wholly infertile among themselves, but may have issue for a generation or two by connection with one of the original stock.

4. Mules that are wholly infertile among themselves, but may have issue through indefinite generations by connection for each with an individual of the original stock.

5. Mules that are fertile among themselves through one or two generations.

6. Mules that are fertile among themselves through many generations.

7. Mules that are fertile among themselves through an indefinite number of generations.

The cases 1 to 5 are known to be established facts in nature; and each bears its testimony to the grand law of purity and permanence. The examples under the heads 2 to 5 become severally less and less numerous, and art must generally use an unnatural play of forces or arrangements to bring them about.

Again, in the animal kingdom, there is the same aversion in nature to intermixture, and it is emotional as well as physical. The supposed cases of fertile hybridity are fewer than among plants.

Moreover, in both kingdoms, if hybridity be begun, nature commences at once to purify herself as of an ulcer on the system. It is treated like a disease, and the energies of the species combine to throw it off. The short run of hybridity between the horse and the ass, species very closely related, reaching its end *in one single generation*, instead of favoring the idea that perpetuated fertile hybridity is possible, is a speaking protest against a principle that would ruin the system if allowed free scope.

The finiteness of nature in all her proportions, and the necessity of finiteness and fixedness for the very existence of a kingdom of life, or of human science, its impress on finite mind, are hence strong arguments for the belief that hybridity cannot seriously trifle with the true units of nature, and at the best, can only make temporary variations.

It is fair to make the supposition, that, in case of a very close proximity of species, there might be a degree of fertile hybridity allowed;

and that a closer and closer affinity *might* give a longer and longer range of fertility. But the case just now alluded to seems to cut the hypothesis short: and, moreover, it is not reasonable to attribute such indefiniteness to nature's outlines, for it is at variance with the spirit of her system.

Were such a case demonstrated by well-established facts, it would necessarily be admitted; and I would add, that investigations directed to this point are the most important that modern science can undertake. But until proved by arguments better than those drawn from domesticated animals, we may plead the general principle against the *possibilities* on the other side. If there is a law to be discovered, it is a wide and comprehensive law, for such are all nature's principles. Nature will teach it not in one corner of her system only, but more or less in every part. We have, therefore, a right to ask for well-defined facts, taken from the study of successive generations of the interbreeding of species known to be distinct.

Least of all should we expect that a law, which is so rigid among plants and the lower animals, should have its main exceptions in the highest class of the animal kingdom, and its most extravagant violations in the genus Homo; for if there are more than one species of man, they have become in the main indefinite by intermixture. The very crown of the kingdom has been despoiled; for a kingdom in nature is perfect only as it retains all its original parts in their full symmetry, undefaced and unblurred. Man, by receiving a plastic body, in accordance with a law that species most capable of domestication should necessarily be most pliant, was fitted to take the whole earth as his dominion, and live under every zone. And surely it would have been a very clumsy method of accomplishing the same result, to have made him of many species, all admitting of indefinite or nearly indefinite hybridization, in direct opposition to a grand principle elsewhere recognized in the organic kingdoms. It would have been using a process that produces impotence or nothing among animals, for the perpetuation and progress of the human race.

There are other ways of accounting for the limited productiveness of the mulatto, without appealing to a distinction of species. There are causes, independent of mixture, which are making the Indian to melt away before the white man, the Sandwich Islander and all savage people to sink into the ground before the power and energy of higher intelligence. They disappear like plants beneath those of stronger root and growth, being depressed morally, intellectually, and physically, contaminated by new vices, tainted variously by foreign disease, and dwindled in all their hopes and aims and means of progress, through an overshadowing race.

We have therefore reason to believe, from man's fertile intermixture, that he is one in species; and that all organic species are divine appointments which cannot be obliterated, unless by annihilating the individuals representing the species.

It may be said, that different species in the inorganic world combine so as to form new units, and why may they not in the organic? It is true they combine, but not by indefinite blendings. There is a definite law of multiples, and this is the central idea in the system of inorganic nature. In organic nature, such a law of multiples, if existing, would be general, as in the inorganic; it would be an essential part of the system, and should be easily verified, while, in fact, observation lends it no support, not even enough to have suggested the hypothesis.

In one kingdom, the *inorganic*, there is multiplication of kinds of units by combination, according to the law of multiples, and no reproduction; while in the *organic*, there is reproduction of like from like, and no multiplication of kinds by combination. And thus the two departments of living and dead nature widely diverge.

Neither does the possibility of mere mixture among inorganic substances afford any analogy to sustain the idea of possible hybrid mixture indefinitely perpetuated, among living beings. The mechanical aggregation of units that make up ordinary mixture, is one thing; and the combination that would alter a germ, one of the units in organic species, even to its fundamental nature, is quite another. This last is not aggregation. It is as different from mere mixture as is chemical combination, and stands somewhat in the same relation, so that the analogy has no bearing on the question.

3. *Variations of Species.*

But there are variations in species, and this is our next topic. The principles already considered teach, as we believe, that each species has its specific value as a unit, which is essentially permanent or indestructible by any natural source of change; and we have, therefore, to admit in the outset, if these principles are true, that variations have their limits, and cannot extend to the obliteration of the fundamental characteristics of a species.

To understand these variations, we may again appeal to general truths.

Variation is a characteristic of all things finite, and is involved in the very conditions of existence. No substance or body can be wholly independent of every or any other body in the universe. The most comprehensive and influential law in nature, most fundamental in all change, composition or decomposition, growth or decay, is the law of mutual sympathy, or tendency to equilibrium in force through universal action and reaction.

The planets have their orbits modified by other bodies in space through their changing relations to those bodies. A substance, as oxygen or iron, varies in temperature and state of expansion from the presence of a body of different temperature; in chemical tendencies from the presence of a luminous body like the sun; in magnetic or electrical attraction from surrounding magnetic or electrical influences. There is thus unceasing flow and unceasing change through the universe. All the natural forces are closely related as if a common family or group, and are in constant mutual interplay.

The degree or kind of variation has its specific law for each element; and in this law the specific nature of the element is in a degree expressed. There is to each body or species, the normal or fundamental force in which its very nature consists; and, in addition, the relations of this force to other bodies, or kinds, amounts or conditions of force, upon which its variations depend. One great end of inorganic science is to study out the law of variables for each element or species. For this law is as much a part of an idea of the species, as the fundamental potentiality; indeed, the one is a measure of the other.

So again, a species in the *organic* kingdoms is subject to variations, and upon the same principle. Its very development depends on the appropriation of material around it, and on attending physical forces or conditions, all of which are variable through the whole of its history. Every chemical or molecular law in the universe is concerned in the growth, — the laws of heat, light, electricity, cohesion, etc.; and the progress of the developing germ, whatever its primal potentiality, is unavoidably subject to variations, from the diversified influences to which it may be exposed. The new germ, moreover, takes peculiarities from the parent, or from the circumstances to which its ancestry had been exposed during one or more preceding generations.

There is then a fixed normal condition or value, and around it librations take place. There is a central or intrinsic law which prevents a species from being drawn off to its destruction by any external agency, while subject to greater or less variations under extrinsic forces.

Liability to variation is hence part of the law of a species; and we cannot be said to comprehend in any case the complete idea of the type until the relations to external forces are also known. The law of variables is as much an expression of the fundamental qualities of the species in organic as in inorganic nature; and it should be the great aim of science to investigate it for every species. It is a source of knowledge which will yet give us a deep insight into the fundamental laws of life. Variations are not to be arranged under the head of *accidents;* for there is nothing accidental in nature; what we so call, are expressions really of profound law, and often betray truth and law which we should otherwise never suspect.

This process of variation is the external revealing the internal through their sympathetic relations: it is the law of universal nature reacting on the law of a special nature, and compelling the latter to exhibit its qualities: it is a centre of force manifesting its potentiality, not in its own inner working, but in its outgoings among the equilibrating forces around, and thus offering us, through the known and physical, some measure of the vital within the germ. It is therefore one of the richest sources of truth open to our search.

The limits of variation it may be difficult to define among species that have close relations. But, being sure that there are limits, that science, in looking for law and order written out in legible characters, is not in fruitless search, we need not despair of discovering them. The zoölogist, gathering shells or mollusks from the coast of eastern America and that of Japan, after careful study, makes out his lists of identical species, with the full assurance that species are definite and stable existences; and he is even surprised with the identity of characters between the individuals of a species obtained from so remote localities. And as he sees zoölogical geography rising into one of the grandest of the sciences, his faith in species becomes identified with his faith in nature and all physical truth.

If, then, we may trust this argument from general truths to special, — general *truths* I say, for general principles as far as established are truths, — we should conceive of a species from the potential point of view, and regard it as —

a. A concentred unit of force, an ineffaceable component of the system of nature; but

b. Subject to greater or less librations, according to the universal law of mutual reaction or sympathy among forces.

And, in addition, in the *organic* kingdom,

c. Exhibiting its potentiality not simply or wholly in any existing condition or action, but through a cycle of growth from the primal germ to maturity, when the new germ comes forth as a repetition of the first to go another round in the cycle and perpetuate the original unit; and, therefore, as follows from a necessary perpetuity of the cycle —

d. Exhibiting identity of species among individuals, by perpetuated fertile intermixture in all normal conditions, and non-identity by the impossibility of such intermixture, the rare cases of continuation for one or two generations, attesting to the stability of the law, by proving the effort of nature to rid herself of the abnormity, and her success in the effort.

e. The many like individuals that are conspecific do not properly constitute the species, but each is an expression of the species in its potentiality under some one phase of its variables; and to understand a species, we must know its law through all its cycle of growth, and its complete series of librations.

We should therefore conceive of the system of nature as involving, in its idea, a system of units, finite constituents at the basis of all things, each fixed in law; these units in inorganic nature as adding to their kinds by combinations in definite proportions; and those in organic nature adding to their numbers of representative individuals, but *not* kinds, by self-reproduction; and all adding to their varieties by mutual reaction or sympathy. Thus, from the law within and the law without, under the Being above as the Author and sustainer of all law, the world has its diversity, the cosmos its fulness of beauty.

I would remark again, that we must consider this mode of reaching truth, by reasoning from the general to the special, as requiring also its complement, direct observation, to give unwavering confidence to the mind; and we should therefore encourage research with a willingness to receive whatever results come from nature. We should give a high place in our estimate to all investigation tending to elucidate the variation or permanence of species, their mutability or immutability; and at the same time, in order that appearances may not deceive us, we should glance towards other departments of nature, remembering that all truth is harmonious, and comprehensive law the end of science.

A word further upon our conceptions of species as realities. In acquiring the first idea of species, we pass, by induction, as in other cases of generalization, from the special details displayed among individuals to a general notion of a unity of type; and this general notion, when written out in words, we may take as an approximate formula of the species. One system of philosophy thence argues that this result of induction is nothing but a notion of the mind, and that species are but an imaginary product of logic; or at least, that since, as they say (we do not now discuss this point), genera are groupings without definite limits which may be laid off variously by different minds, so species are undefined, and individuals are the only realities — the supposed limits to species being regarded as proof of partial study, or a consequence of a partial development of the kingdoms of nature. Another system infers, on the contrary, that species are realities, and the general or type-idea has, in some sense, a *real* existence. A third admits that species are essentially realities in nature, but claims that the general idea exists only as a result of logical induction.

The discussion in the preceding pages sustains most nearly the last view, that species are realities in the system of nature while manifest to us only in individuals; that is, they are so far real, that the idea for each is definite, even of mathematical strictness (although not thus precise in our limited view), it proceeding from the mathematical and finite basis of nature. They are the units fixed in the plan of creation, and individuals are the material expressions of those ideal units.

At the same time, we learn, that while species are realities in a most important and fundamental sense, no comprehensive type-idea of a species can be represented in any material or immaterial existence. For while a species has its constants, it has also its variables, each variable becoming a constant so far only as its law and limits of variation are fixed; and in the organic kingdoms, moreover, each individual has its historic phases, from the germ through the cycle of growth. The general idea sought out by induction, therefore, is not made up of *in*variables. Limited to these, it represents no object, class of objects, or law, in nature. The variables are a necessary complement to the invariables; and the complete species-idea is present to the mind, only when the image in view is seen to be ever changing along the lines of variables and development. Whatever individualized conception is entertained, it is evidently a conception of the species in one of its phases,—that is, under some one specific condition as to size, form, color, constitution, etc., as regards each part in the structure, from among the many variations in all these respects that are possible: mind can picture to itself individuals only, and not species, and one phase at a time in the life of an organic individual, not the whole cycle.

We may attempt to reach what is called the typical form of a species, in order to make this the subject of a conception. But even within the closest range of what may be taken as typical characters, there are still variables; and moreover, we repeat it, no one form, typical though we consider it, can be a full expression of the species, as long as variables are as much an essential part of its idea as constants. The advantage of fixing upon some one variety as the typical form of a species is this,—that the mind may have an initial term for the laws embraced under the idea of the species, or an assumed centre of radiation for its variant series, so as more easily to comprehend those laws.

Again, abrupt transitions, and not indefinite shadings, have been shown to be the law of nature. In proceeding from special characters to a general species-idea, nature gives us help through her stepping-stones and barriers. In former times, man looked at iron and other metals from the outside only, and searching out their differences of sensible characters, gradually eliminated the general notion of each, by the ordinary logical method of generalization. But science now brings the elements to the line and plummet, and reaches a fixed *number* for iron and other elements as to chemical combination, etc. By this means, the studying out of the idea of a species seems almost to have escaped from the domain of logic into that of direct trial by weights and measures. It is no longer the undefined progress of simple reason with a mere notion at the end, but an appeal to definite measurable values, with stable numbers at bottom, fixed in the very foundations of the universe. So, in the organic kingdoms, where there is, to our limited minds, still greater indefiniteness in most characters, the barrier against hybridity appears to stand as a physical test of species. We are thus enabled, in searching into the nature of a species, to strike from the outside detail to the foundation law.

The type-idea, as it presents itself to the mind, is no more a subject of defined conception than any mathematical expression. Could we put in mathematical terms the precise law, in all its comprehensiveness, which is at the basis of the species iron, as we can for one of its qualities, that of chemical attraction, this mathematical expression would stand as a representative of the species; and we might use it in calculations, precisely as we can use any mathematical term. So, also, if we could write out in numbers the potential nature of an organic species, or of its germ, including the laws of its variables, this expression would be like any other term in the hands of a mathematician; the mind would receive the formula as an expression for the species, and might compare it with the formulas of other species. But, after all, we have here a mere mathematical abstraction, a symbol for an amount or law of force, which can be turned into conceptions, only by imagining (supposing this possible) the force in the course of its evolution of concrete realities, according to the law of development and laws of variations embraced within it.

2. On the supposed Uniformity of Cranial Type, throughout all Varieties of the American Race. By Daniel Wilson, University College, Toronto, Canada West.

The universal prevalence of certain characteristics common to all the nations of the New World, and especially of a uniformity of cranial conformation throughout the entire native population of North and South America, has so often been asserted, that it is unnecessary to quote authorities in proof of this. Were such requisite, a considerable list of distinguished writers might be named who have maintained this opinion; though more careful inquiry tends to prove that the greater number of such authorities merely reaffirm the views advanced by Dr. Samuel Morton, as conclusions derived from the valuable data accumulated in his *Crania Americana*. But there is one author, at once so distinguished among American men of science, and so peculiar in the point of view from whence he has regarded the entire question of American ethnology, as to merit special attention. Professor Agassiz, in his *Sketch of the Natural Provinces of the Animal World, and their relation to the different Types of Man*, reaffirms the homogeneous characteristics and ethnic insulation of the American Indian on entirely novel and independent grounds. After defining the evidence on which the opinion is based, that *the boundaries within which the different natural combinations of animals are circumscribed on the surface of the earth coincide with the natural range of distinct types of man*, he proceeds to show that America, including both its northern and southern continent, differs essentially from Europe with Asia, or Africa, in being characterized throughout by a much greater uniformity in all its natural productions, than any thing which comparison enables us to trace in the old world. He then adds: "With these facts before us, we may expect that there should be no great diversity among the tribes of man inhabiting this continent; and indeed the most extensive investigation of their peculiarities, has led Dr. Morton to consider them as constituting but a single race, from the confines of the Esquimaux down to the southernmost extremity of the continent. But, at the same time, it should be remembered that, in accordance with the zoological character of the whole realm, this race is divided into an infinite number of small tribes, presenting more or less difference one from another."

The latest views of Agassiz, as set forth in his contribution to the *Indigenous Races of the Earth*, present us with the same opinions, advanced with additional confirmation from other data. Passing from the general zoölogical analogies in the distribution of species to the special one of the monkey, he remarks on the diversity of opinions among men of science as to the genus *Cebus*, which some zoölogists recognize as one species, others separate into two or three, while others again subdivide it into as many as ten:—"Here we have, with reference to one genus of monkeys, the same diversity of opinion as exists among naturalists respecting the races of man. But in this case, the question assumes a peculiar interest, from the circumstance that the genus *Cebus* is exclusively American; for that discloses the same indefinite limitation between its species which we observe also among the tribes of Indians, or the same tendency to splitting into minor groups, running really one into the other, notwithstanding some few marked differences, — in the same manner as Morton has shown that all the Indians constitute but one race, from one end of the continent to the other. . . . . In the old world, notwithstanding the recurrence of similar phenomena, the range of variation of species seems less extensive, and the range of their geographical distribution more limited. In accordance with this general character of the animal kingdom, we find likewise that, among men, with the exception of the Arctic Esquimaux, there is only one single race of men extending over the whole range of North and South America, but dividing into innumerable tribes; whilst, in the old world, there are a great many well-defined and easily distinguished races, which are circumscribed within comparatively much narrower boundaries."

Since the idea of the homogeneous physical characteristics of the whole aboriginal population of America, extending from Terra del Fuego to the Arctic circle, was first propounded by Dr. Morton, it has been accepted without question, and has more recently been made the basis of many widely comprehensive deductions. Philology and archæology have also been called in to sustain this doctrine of a special unity of the American race; and to prove that, notwithstanding some partial deviations from the prevailing standard, the American Indian is essentially separate and peculiar; *a race distinct from all others.*

The strong-hold, however, of the argument for the essential oneness of the whole tribes and nations of the American continents, is the supposed uniformity of physiological, and especially of physiognomical and cranial characteristics; an ethnical postulate which has not yet been called in question.

On first visiting the American continent, and enjoying the opportunity of judging for myself of the physical characteristics of the aboriginal race of the forests, I did so under the full conviction of meeting with such a universal approximation to the assumed Normal type, as would fully bear out the deductions of previous observers, and especially of one so persevering in the accumulation of the requisite materials on which to base a legitimate result, as the author of the *Crania Americana.* When, therefore, I proceeded to open some Indian graves in Canada, and to endeavor to procure crania from others on ascertaining of their disturbance, it was solely with a view to possess myself of one or two specimens of the peculiar American type of cranium, which possessed a special interest to me from its approximation to the ancient brachy-cephalic skull, familiar to me, as found in one important class of early British barrows. It was accordingly, simply with a sense of disappointment that I found the results of repeated efforts, in different localities, supplied me with crania, which, though undoubtedly Indian, exhibited little or no trace of the rounded form, with short longitudinal diameter, so strikingly apparent in the ancient crania of Central America and the mounds. Appreciating, as I did, the invaluable labors of Dr. Morton, — which will be more fully prized, as the important science they tend to elucidate commands a wider attention and more careful study, — it did not occur to me at first to question any of the results so frequently reiterated by him, and repeatedly confirmed by the concurrence of later writers. Slowly, however, the idea has forced itself upon me, that, to whatever extent the affirmed typical form of the American cranium may prevail in other parts of the continent, the crania most frequently met with along the north shores of the great lakes are deficient in some of its most essential elements.

In order to institute such a comparison as will satisfactorily test this question, it is necessary to define the essential requisites of the American type of cranium; for, neither Dr. Morton, nor his successors, have overlooked the fact of some deviation from the supposed Normal type, not only occurring occasionally, but existing as a permanent characteristic of certain tribes, including those to which I have more particularly to refer. Dr. Morton recognized a more elongated head as pertaining to certain tribes, of which he names the Lenapé stock, the Iroquois, and the Cherokees, to the east of the Alleghany mountains; and the Mandans, Ricaras, and Assinnaboins, to the west. But such elongation he speaks of as a mere slight variation from the more perfect form of the normal skull; and he adds: "even in these instances the characteristic truncation of the occiput is more or less obvious." *

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* Crania Americana, p. 69.

So also Dr. Nott, after defining the typical characteristics of the American cranium, remarks: "Such are more universal in the Toltecan than in the barbarous tribes. Among the Iroquois, for instance, the heads were often of a somewhat elongated form, but the Cherokees and Choctaws, who, of all barbarous tribes, display greater aptitude for civilization, present the genuine type in a remarkable degree. My birth and long residence in southern States have permitted the study of many of these living tribes, and they exhibit this conformation almost without exception. I have also scrutinized many Mexicans, besides Catawbas, of South Carolina, and tribes on the Canada lakes, and can bear witness that the living tribes everywhere confirm Morton's type." *

We cannot err in taking the very interesting cranium found by Dr. Davis and Mr. Squier in a mound in the Scioto valley, Ohio, as an example of the true typical head; for it is produced as such by Dr. Nott, in the "Types of Mankind," and is described, in the words of Dr. Morton, in Dr. Meigs's *Catalogue of Human Crania, in the collection of the Academy of Natural Science of Philadelphia*, issued by order of the Academy, during the present year, as "An Aboriginal American; a very remarkable head. This is, perhaps, the most admirably formed head of the American race hitherto discovered. It possesses the national characteristics in perfection, as seen in the elevated vertex, flattened occiput, great interparietal diameter, ponderous bony structure, salient nose, large jaws, and broad face. It is the perfect type of Indian conformation, to which the skulls of all the tribes, from Cape Horn to Canada, more or less approximate." As shown by the front view of this skull, it presents no trace of pyramidial conformation.

Of this skull, the measurements which involve the most essential typical elements, and so furnish precise materials for comparison, are:—

Longitudinal diameter	6.5	inches.
Parietal "	6.	"
Vertical "	6.2	"
Intermastoid Arch	16.	"
Horizontal circumference	19.8	"

So that, in fact, the cranium very closely corresponds in its measurements, in length, breadth, and height. Still further it may be observed, on examining the full-sized view of the skull, as given by Messrs. Squier and Davis (Pl. XLVII.), that the singular longitudinal abbreviation of this skull is nearly all posteriorly. A line drawn through the meatus auditorius externus in profile, parallel to the elevated forehead, divides it into two unequal parts, of which the anterior and posterior parts are nearly in the ratio of two to one. To this type the ancient Peruvian and Mexican crania unquestionably approximate. Of one of the former, from the Temple of the Sun, (Pl. XI.) Dr. Morton remarks: " A strikingly characteristic Peruvian head. As is common in this series of skulls, the parietal and longitudinal diameters are nearly the same," namely, longitudinal diameter, 6-1, parietal diameter, 6. So far, therefore, as such evidence goes, it appears to justify the conclusion arrived at by Dr. Morton, that the people represented by the mound skulls in his possession, " were one and the same with the American race, and probably of the Toltecan branch." *

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* Types of Mankind, p. 441.

The conformity affirmed to exist between the ancient Mexican and Peruvian skulls, and those of the modern barbarous tribes, may also be so far asserted as a partial approximation in relation to some of them, and appears to receive a fuller confirmation when carefully selected examples are referred to; as a sufficient number occur to indicate the occasional reappearance of some of the most striking typical peculiarities.

Of a similar nature is the correspondence pointed out by Dr. Nott,† between the Scioto mound skull and that of a Cherokee chief, who died a prisoner near Mobile, in 1837. In this example, in so far as can be judged from the comparison of both by drawings in profile, without precise measurements, the points of agreement are indisputable, though even here amounting to no more than an approximation. The vertical occiput of the ancient skull, — more markedly vertical in the original drawing than in the small copy, — is only partially represented in the other; the square form of the ancient profile in the coronal region becomes conoid in the modern one; and the intersecting line drawn through the *meatus* shows a very partial reproduction in the modern example, of the remarkable preponderance of anterior cerebral development, which — if not produced by artificial means — is the most singular characteristic of the ancient head.

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* Crania Americana, p. 229.

† Types of Mankind, p. 442.

But while acknowledging such approximation of the selected modern Cherokee cranium to the ancient type, neither the legitimate deductions following from this, nor from the other examples referred to by Dr. Nott, appear to bear out his conclusions, that not only that type "is found among tribes the most scattered, among the semi-civilized and the barbarous, among living as well as among extinct races;" but "that *no foreign race has intruded itself in their midst, even in the smallest appreciable degree.*" The examples of Cherokee heads referred to in the *Table of Anatomical Measurements* in the Crania Americana, in so far as they fairly represent the cranial characteristics of this tribe or nation, seem to indicate that the Mobile chief is an exceptional case; and this is further borne out by the special example selected by Dr. Morton, and figured in his great work; "the head of a Cherokee warrior, who was known in the army, by the name of John Waring." The following are its most characteristic measurements, exhibiting such a wide divergence from the normal type, as illustrated in that of the Scioto mound, as to substitute contrast for comparison: —

Longitudinal diameter	7.2
Parietal "	5.3
Vertical "	5.3
Intermastoid arch	14.1
Horizontal circumference	19.1

In the typical head the longitudinal, parietal, and vertical diameters closely correspond; in this, the excess of the longitudinal over the parietal and vertical diameters is such, as is rarely exceeded in the modern Anglo-Saxon, or even the longer sub-celtic head. Yet, that such an excess in the longitudinal diameter did not present to the experienced eye of Dr. Morton any striking deviation from the form of the modern Indian head, is proved by his noting of this very example: "Nor is there any-thing remarkable in the form of the skull."

Bearing in remembrance, then, the partial nature of the approximation so far apparent between the ancient and modern American cranium, personal observation leads me to believe that such is to be found, — with exceptional instances of closer affinities, and also with important divergencies from the typical Indian form and character, not exceptional, but pertaining to the whole nation,—among the still numerous examples of the Algonquin stock, as represented by the Chippeways. Of these I have examined, and compared by the eye, many at widely scattered locations; on Lake Simcoe and the Georgian Bay; at Mackinaw, in Lake Huron, and at Sault St. Marie; at Ontonagon, La Point, the Apostle islands, and the St. Louis river, on Lake Superior; as well as such chance opportunities as occur in the neighborhood of Niagara Falls, and on the streets of our Canadian towns and villages. Physiognomically they present the wide and prominent mouth, high cheek-bones, and broad face, so universally characteristic of the American Indian; but they by no means present in a remarkable degree the wide and massive lower jaw, which has been noted as of universal occurrence among the Red Indians. Still more noticeable is the absence of the aquiline nose, so characteristic generally of the true Indian in contradistinction to the Esquimaux. The eye may be fully depended on for physiognomical characteristics; it is of much less value in testing variations from any assumed cranial type, especially in reference to comparatively minute divergencies of measurement. Nevertheless, their heads appear to me to be essentially brachy-cephalic, as compared with those of other tribes in part displaced by them; but—in so far as may be judged from the observation of the living head covered with the thickly matted and long, coarse hair of the Indian—they are not remarkable for vertical elevation.

The following table presents the results of an examination of six pure-breed Chippeways, at the Indian reserve on Lake Couchiching, with the addition of two others, the only examples of the same nation, given by Morton, in the *Crania Americana.* From these it will be seen that, while in the majority of them a certain approximation of the longitudinal to the parietal diameter is discernible, it is of a very partial nature, except in one instance (No. 5), where a manifest correspondence to certain relative proportions of the mound-builder type of head is apparent.

TABLE I. — CRANIAL MEASUREMENTS. — (CHIPPEWAYS.)

	Longitudinal Diameter.	Parietal Diameter.	Frontal Diameter.	Intermastoid Arch.	Horizon'l Circumference.
1. Joseph Shilling	7.5	6.1	5.6	14.4	22.9
2. James Inglesol (Kobsequan) .	7.4	6.	5.	14.8	22.3
3. Jac. Crane (Now-keise-gwab) .	7.1	6.	5.4	15.4	22.1
4. Peter Jacobs (Pah-tah-se-ga) .	7.3	5.8	5.4	15.	22.6
5. Jacob Shilling	6.9	6.	5.1	14.7	22.
6. William Snake	7.1	6.	5.5	15.1	22.
7. Crania Americana, No. 683 . .	7.3	5.8	4.8	15.1	20.9
8. Crania Americana, No. 684 . .	7.2	5.5	4.3	14.8	20.2

Some of the measurements in the living head are necessarily affected by the hair, always coarse and abundant with the Indian. Others again, such as the vertical diameter, cannot be taken; but the mastoid processes are sufficiently prominent to leave very little room for error in the measurement of the intermastoid arch; and this suffices to show the very exceptional approximation of the modern Chippeway head — in so far as it is illustrated by these examples — to the ancient type, in the proportional elevation of the vertex. In the horizontal circumference some deduction must be made for the hair, to bring it to the true cranial measurement in all the six living examples.

The Chippeways have been selected for reference here, because — taking the above measurements, along with other observations — they appear to indicate a nearer approach to some of the assumed characteristics of the American cranial type, in this widely spread branch of the Indian stock, than is observable in other northern races, and especially than is apparent on an examination of skulls belonging, as I believe, to the original Huron occupants of the country around Lakes Simcoe and Couchiching, where the Chippeways more especially referred to are now settled, and of the greater part of Upper Canada, when first explored.

But the divergent characteristics noticeable in these, and still more in the crania of older Canadian graves, are by no means confined to those named, as a few examples will suffice to show. Such a radical divergence from the assumed normal type as has been already noted in Dr. Morton's selected Cherokee cranium, is no less obvious in that of the Miami, — the head of a celebrated chief, eloquent, of great bravery, and uncompromising hostility to the Whites: (*Crania Americana*, p. 182.)

Longitudinal Diameter	7.3
Parietal Diameter	5.5
Vertical Diameter	5.5
Intermastoid Arch	14.5
Horizontal Circumference	19.8

In the example of the Potawatomies, "A skull of a genuine Potawatomie, remarkable for its capacity behind the ears." (Ibid., p. 186.)

Longitudinal Diameter	7.8
Parietal Diameter	5.7
Vertical Diameter	5.3
Intermastoid Arch	16.
Horizontal Circumference	22.1

In that of the Blackfeet, the largest of two brought to Philadelphia by Catlin, and noted by Dr. Morton for its great breadth between the parietal bones. It is also very markedly pyramidal. Nevertheless, here also the longitudinal diameter is nearly two inches in excess both of the parietal and vertical diameters. (Ibid., 202.)

Longitudinal Diameter	7.1
Parietal Diameter	5.4
Vertical Diameter	5.1
Intermastoid Arch	13.8
Horizontal Circumference	19.9

So also Dr. Morton says of the Menominees: "I have received a series of Menominee skulls, embracing eight specimens. They are something larger than the average of Indian crania; and although for the most part they present a *rather oval shape*, they are all marked by a gently flattened occiput." (Ibid., 179.) A reference to the Catalogue of the Morton Collection at Philadelphia discloses the important fact, that of those marked by the shorter longitudinal diameter, Nos. 35, 44, and 563 are females.

Again, of the Delawares he remarks: "The few Delaware skulls in my possession are more elongated than is usual in the American tribes; they are also narrower in proportion in the parietal diameter, and less flattened on the occiput."

Such are some indications of data, — derived from a source altogether unexceptionable in the present argument,— which seem to render it impossible to uphold the views so repeatedly affirmed, of the physiognomical, physiological, and, above all, the cranial unity characterizing the whole ancient and modern aborigines of the New World.

I omit, meanwhile, any reference to the characteristics ascribed by Dr. Morton to the Iroquois and Hurons or Wyandots, those tribes to whom, with the greatest probability, may be assigned the crania specially examined by me, found along the shores of Lake Ontario, the north shore of Lake Erie, and on Lake Huron.

Of Indian skulls dug up within the district once pertaining to the Huron or Wyandot branch of the Iroquois stock, I had observed and cursorily examined a considerable number before my attention was specially drawn to the peculiar characteristics now under consideration, owing to my repeated rejection of those which turned up, as failing to furnish specimens of the assigned typical American head. Since then I have carefully examined and measured twenty-nine Indian skulls, with the following results:

1. Only three exhibit such an agreement with the American type, as, judged by the eye, to justify their classification as true brachycephalic crania. One of these (No. 11), a very remarkable and massive skull, was turned up at Barrie, on Lake Simcoe, with, it is said, upwards of two hundred others. It differs from all the other Canadian crania in exhibiting the vertical occiput so very strikingly, that, when laid resting on it, it stands more firmly than in any other position. Of the Scioto Valley cranium, Dr. Morton remarks, in reference to the occiput, "Similar forms are common in the Peruvian tombs, and have the occiput, as in this instance, so flattened and vertical as to give the idea of artificial compression; yet this is only an exaggeration of the natural form, caused by the pressure of the cradle-board in common use among the American nation." I think it extremely probable that further investigation will tend to the conclusion that the vertical or flattened occiput, instead of being a typical characteristic, pertains entirely to the class of artificial modifications of the natural cranium familiar to the American ethnologist alike in the disclosures of ancient graves, and in the customs of widely separated living tribes. In the case of the Barry skull, there can be little doubt that the flat-

tened occiput is the result of artificial compression, of a much more decided nature than that of the cradle-board of the papoose.

It is not undeserving of notice here, that the example selected by Cuvier, among his "crania pertaining to the four principal types of the human species," to illustrate the American race, exhibits a strikingly marked prolongation of the occiput. It is described as: "*Crâne trouvé dans une caverne, près du Village de Maïpuré près des bords de l'Orénoque; rapporté par M. de Humboldt;*" * and suffices to indicate in some degree how far the opinion already quoted from Humboldt's *Researches* coincides with his own independent observations.

2. In addition to what has been above remarked in reference to the probable artificial origin of the supposed typical form of the occiput, assigned by Dr. Morton to the whole American race, I am struck, in the majority of the examples examined, with the total absence of any approximation to the flattened occiput. Sixteen of the crania referred to exhibit a more or less decided posterior projection of the occiput, twelve of these being markedly so, and seven of them presenting a striking prolongation of it.

3. The tendency to the pyramidal form, occasioned by the angular junction of the parietal bones, is apparent in the majority of the skulls examined. I have noted its occurrence more or less prominently in fourteen crania, of which five exhibit a strongly marked pyramidal form, extending to the frontal bone. In some, however, it is only slightly indicated, while in several it is totally wanting.

4. I am further struck with the frequency of the very partial projection, and in some examples the total absence of the superciliary ridge, a characteristic which I am not aware has been noted before. In six of the skulls carefully noted by me, this is particularly manifest, and, along with their pyramidal vertex and predominant longitudinal diameter, suggests affinities hitherto overlooked, with the Esquimaux form of skull.

5. I would also note, that whereas Dr. Morton states, as the result of his experience, that the most distant points of the parietal bones are, for the most part, the protuberances, I have only found such to be the case in two out of twenty-nine Canadian skulls. The

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* *Cuvier: Le Régne Animal. Races Humaines, planches* 1 *et* 2, *pl.* 8, *fig.* 2.

widest parietal measurement is generally a little above the squamous suture.

6. The occurrence may also be noted, in several of these crania, of wormian bones of such regularity of form and position, as to constitute indications at least seemingly confirmatory of the supposed tendency to the development of an *interparietal or superoccipital* bone, first pointed out by Dr. Bellamy. This, which is a permanent cranial characteristic in some of the mammalia, is regarded by Dr. Tschudi as an osteological feature peculiar to the Peruvians, and is, he affirms, traceable in all the skulls of that race.

TABLE II. — CRANIAL MEASUREMENTS. — WESTERN CANADA (HURONS).

	1. Long. Diam.	2. Parie. Diam.	3. Front. Diam.	4. Verti. Diam.	5 Inter-mastoid Arch.	6. Inter-Mast. Line.	7. Occip. Frontal Arch.	8. Do. from Oc. prot. to root of nose.	9. Horiz. circumference.
1. Orillia . . .	7.5	5.7	4.5	5.6	15.6	4.25	15.	13.	21.1
2. " . . .	7.4	5.5	4.4	5.4	14.7	4.5	—	12.	20.6
3. Oakridges . .	7.6	5.5	4.7	6.	15.7	4.6	15.	13.7	21.2
4. " (female)	6.8	4.8	4.2	5.	13.6	4.	13.2	11.3	18.9
5. Windsor . . .	6.6	5.3	4.2	5.5	14.5	4.2	13.5	12.2	19.
6. Peterborough .	7.7	5.5	4.9	5.8	15.4	4.6	15.	13.6	21.1
7. Windsor . . .	7.	5.7	4.7	5.7	15.2	4.3	14.5	12.9	20.1
8. " . . .	7.	5.7	4.5	5.7	16.1	4.	14.4	12.4	20.1
9. " . . .	7.4	6.1	4.9	5.7	—	4.5	15.5	13.4	21.4
10. Ponetanguishene	7.8	5.6	4.6	5.9	15.5	4.5	15.6	13.5	21.3
11. Barrie	6.6	6.4	5.2	5.3	16.	4.6	14.4	12.1	20.7
12. Burlington Bay	7.	5.25	4.4	5.3	14.	4.	13.6	11.9	19.5
13. " "	7.6	5.6	4.4	5.4	15.2	4.2	14.9	12.9	20.9
14. Burwick . . .	7.2	5.1	4.4	5.6	14.3	4.3	14.7	12.4	21.
15. Tecumseth . .	7.3	5.6	4.4	5.5	14.5	4.9	14.4	12.5	20.25
16. " (fem.)	7.2	5.2	3.9	5.	14.1	3.6	14.25	12.9	19.7
17. " . .	7.9	6.	4.6	5.7	16.	3.4	16.1	14.25	22.
18. " (fem.)	7.6	5.25	4.3	5.6	14.	4.1	14.25	12.6	20.2
19. " (fem.)	7.5	5.2	4.1	5.1	13.4	4.2	14.8	13.	20.5
20. " . . .	7.4	5.6	4.6	5.5	15.	4.4	15.	13.6	20.9
21. " . . .	7.6	5.4	4.2	5.7	15.1	4.4	15.3	14.	20.9
22. Owen Sound .	7.	5.5	4.2	5.	13.8	4.	14.	12.2	19.8
23. " " .	7.3	5.3	4.25	5.25	14.4	4.2	14.25	12.4	20.4
24. " " .	7.2	5.4	3.8	5.25	14.5	3.9	14.2	12.	19.9
25. " " .	7.7	5.4	4.7	5.6	14.6	4.2	15.	13.	21.4
26. Oro	7.4	5.4	—	4.25	15.25	4.	14.9	12.4	20.4
27. Owen Sound .	7.5	5.9	5.1	5.5	15.	4.25	15.6	13.3	21.8
28. " " .	7.6	5.5	4.5	5.4	14.6	4.5	14.9	13.1	21.3
29. Oro	7.5	5.6	4.4	5.5	15.5	4.3	15.2	13.	21.4

The table of measurements of skulls procured from Indian cemeteries to the north of Lakes Erie and Ontario (Table II.), supplies some, at least, of the elements essential to the formation of a sound judgment on the question under consideration. It embraces twenty-nine examples. To these I have added, in another table (Table III.), the corresponding measurements of the skull of the celebrated Mohawk chief, Joseph Brant, Tayendanaga, from a cast taken on the opening of his grave, at the interment of his son, John Brant, in 1852. I have also further added, from the *Crania Americana*, the Iroquois and Huron examples given there, which, it will be seen, agree in the main with the results of my own independent observations; while a comparison of the two tables will be satisfactory to those who may not unnaturally hesitate to adopt conclusions based on the amount of evidence produced, adverse to opinions re-affirmed under such various forms by so high an authority as Dr. Morton, and adopted and made the basis of such comprehensive inductions by his successors.

TABLE III. — CRANIAL MEASUREMENTS (SIX NATIONS).

	1. Long. Diam.	2. Parie. Diam.	3. Front. Diam.	4. Verti. Diam.	5. Inter-mast. Arch.	6. Inter-mast. Line.	7. Occip. Front. Arch.	8. Do. from Oc. prot. to root of nose.	9. Horiz. Circumference.
Mohawk: Brant . . .	7.8	6.	5.		15.6?			13 ?	22.
Oneida, Morton, No. 33 .	7.5	5.6	4.1	5.8	14.4	4.3	14.9	——	20.8
Cayuga, " No. 417 .	7.8	5.1	4.2	5.4	14.2	4.5	15.5	——	20.8
Huron, " (fem.) No. 607	6.7	5.6	4.1	5.2	14.5	3.9	14.	——	19.2
Huron, " No. 15 .	7.2	5.3	4.3	5.5	15.	4.4	14.2	——	19.8
Iroquois, " No. 16 .	7.5	5.5	4.5	5.7	15.2	4.5	15.1	——	20.8
Iroquois, " A. N. S.	7.1	5.4	4.2	5.3	14.3	4.	14.1	——	20.

The intimate relations in language, manners, and the traditions of a common descent, between those northern and southern branches of the Iroquois stock, render these two tables, in so far as they present concurrent results, applicable as a common test of the supposed homogeneous cranial characteristics of the aboriginal American, in relation to the area of the great lakes. Twenty-nine skulls, such as the first table supplies, or thirty-six as the result of both, may, perhaps, appear to be too small a number on which to base conclusions adverse to those promulgated by an observer so distinguished and so persevering as Dr. Morton, and accepted by writers no less worthy of esteem and deference. Still more may these data seem inadequate, when it is remembered that Dr. Morton's original observations and measurements embraced upwards of three hundred American skulls. But, in addition to the fact, that the measurements now supplied are only the more carefully noted data which have tended to confirm conclusions suggested by previous examinations, in a less detailed manner, of a larger number of examples, an investigation of the materials which supplied the elements of earlier inductions will show, that only in the case of the ancient "Toltecan" tribes did Dr. Morton examine nearly so many examples; while, in relation to what he designated the "Barbarous Race," to which the northern tribes belong, even in Dr. Meigs' greatly enlarged catalogue of the Morton Collection, as augmented since his death, the Seminole crania present the greatest number belonging to one tribe, and these only amount to sixteen.

In contrast to the form of head of the true American race, Dr. Morton appends to his *Crania Americana* drawings and measurements of four Esquimaux skulls, familiar to me, if I mistake not, in the collection of the Edinburgh Phrenological Society. In commenting on the views and measurements of these, he remarks: "The great and uniform differences between these heads and those of the American Indians will be obvious to every one accustomed to make comparisons of this kind, and serve as corroborative evidence of the opinion that the Esquimaux are the only people possessing Asiatic characteristics on the American continent." In some respects this is undoubtedly true; the prognathous form of the superior maxilla, and the very small development of the nasal bones, especially contrast with well-known characteristics of the American aborigines. But having had some little familiarity in making comparisons of this kind, it appears to me, notwithstanding these distinctive points, that an impartial observer might be quite as likely to assign even some of the examples of Iroquois and other northern tribes figured in the *Crania Americana*, to an Esquimaux, as to a Peruvian, Mexican, or Mound-Builder type. Compare, for example, the vertical and occipital diagrams, furnished by Dr. Morton, of the Esquimaux crania (p. 248) with those of the Iroquois and Hurons (pp. 192–194). Both are elongated, pyramidal, and with a tendency towards a conoid, rather than a flattened or vertical occipital form; and when placed along-side of the most markedly typical Mexican or Peruvian heads, the one differs little less widely from these than the other. The elements of contrast between the Hurons and Esquimaux are mainly traceable in the bones of the face — physiognomical, but not cerebral.

Taking once more their cranial measurements as a means of comparison, these, when placed along-side each other, equally bear out the conclusions already affirmed. For comparison, I select, in addition to the Scioto Valley Mound-Builder, the following, as those pointed out by Dr. Morton's own descriptions as among the most characteristic he has figured: Plate XI., Peruvian from the Temple of the Sun.— "A strikingly characteristic Peruvian head." Plate XI., C. — "Here again the parietal and longitudinal diameters are nearly equal. The posterior and lateral swell of this cranium is very remarkable, and the vertex has the characteristic prominence." Of the Mexican skulls, Dr. Morton remarks of Plate XVII.: "with a better forehead than is usual, this skull presents all the prominent characteristics of the American race, — the prominent face, elevated vertex, vertical occiput, and the great swell from the temporal bones upward;" and of Plate XVIII.: "a remarkably well characterized Toltecan head, from an ancient tomb near the city of Mexico."

TABLE IV. — COMPARATIVE CRANIAL MEASUREMENTS.

	Long. Diam.	Parie. Diam.	Front Diam.	Verti- Diam.	Inter. Mast. Arch.	Inter- Mast. Line.	Occip. front Arch.	Horiz. Cir- cumf.
Scioto Mound	6.5	6.	4.5	6.2	16.	4.5	13.8	19.8
Peruvian	6.1	6.	4.7	5.5	16.	4.5	14.1	19.5
"	6.	5.9	4.4	5.	15.5	4.	13.2	19.
Mexican	6.8	5.5	4.6	6.	15.6	4.4	14.6	19.9
Toltecan	6.4	5.7	4.5	5.4	14.6	4.5	13.5	20.2
Iroquois	7.5	5.5	4.5	5.7	15.2	4.5	15.1	20.8
Cayuga	7.8	5.1	4.2	5.4	14.2	4.5	15.5	20.8
Oneida	7.5	5.6	4.1	5.8	14.4	4.3	14.9	20.8
Huron	7.2	5.3	4.3	5.5	15.	4.4	14.2	19.8
Esquimaux	7.5	5.4	4.6	5.4	14.3	4.1	15.2	20.4
"	7.3	5.5	4.4	5.3	14.1	4.3	14.4	20.3
"	7.5	5.1	4.3	5.5	14.8	3.9	15.5	20.3
"	6.7	5.	4.4	5.	13.6	4.	13.9	18.9

These examples I refer to in preference to those presented in the previous table as the result of my own observations, as they are necessarily unbiased. They are the specimens of the very stock I refer to, selected or brought by chance under the observation of Dr. Morton, and included as the characteristic or sole examples of its tribes or nations, in his great work. But the same conclusions are borne out by the examples obtained within the Canadian frontiers; and they seem to me to lead inevitably to this conclusion, that if crania measuring, in some cases, two inches in excess in the longitudinal over the parietal and vertical diameters, and in others nearly approximating to such relative measurements, — without further reference here to variations in occipital conformation, — if such crania may be affirmed, without challenge, to be of the same type as others where the longitudinal, parietal, and vertical diameters vary only by small fractional differences, then the distinction between the *brachycephalic* and the *dolichocephalic* type of head is, for all purposes of science, at an end, and the labors of Blumenbach, Retzius, Nilsson, and all who have trod in their footsteps, have been wasted in pursuit of an idle fancy. If differences of cranial conformation of so strongly defined a character, as are thus shown to exist between the various ancient and modern people of America, amount to no more than variations within the normal range of a common type, then all the important distinctions between the crania of ancient European barrows and those of living races, amount to little; and the more delicate details, such as those, for example, which have been supposed to distinguish the Celtic from the Germanic cranium, the ancient Roman from the Etruscan or Greek, the Sclave from the Magyar or Turk, or the Gothic Spaniard from the Basque or Morisco, must be utterly valueless. If external circumstances or the progress of civilization exercise any influence on physical form, a greater diversity of conformation is to be looked for in Europe than among the Indians of America, where, as in Africa, nearly the same habits and modes of life have characterized the whole "Barbarous Race," throughout the centuries during which Europe has had any knowledge of them. · But, making full allowance for such external influences, it seems to me, after thus reviewing the evidence on which the assumed unity of the American race is founded, little less extravagant to affirm of Europe than of America, that the crania everywhere and at all periods, have conformed, or even approximated to, one type.

As an hypothesis, based on evidence accumulated in the *Crania Americana*, the supposed homogeneity of the whole American aborigines was perhaps a justifiable one. But the evidence was totally insufficient for any such absolute and dogmatic induction as it has been made the basis of. With the exception of the ancient Peruvians, the comprehensive generalizations relative to the Southern American continent strangely contrast with the narrow basis of the premises. With a greater amount of evidence in reference to the Northern continent, the conclusions still go far beyond any thing established by absolute proof; and the subsequent labors of Morton himself, and still more of some of his successors, seem to have been conducted on the principle of applying practically, and in all possible bearings, an established and indisputable scientific truth, instead of testing by further evidence a novel and ingenious hypothesis.

Dr. Latham, after commenting on the manifest distinctions which separate the Esquimaux of the Atlantic from the tribes of the American aborigines lying to the south and west of them, as elements of contrast which have not failed to receive full justice, adds: "It is not so with the Eskimos of Russian America, and the parts that look upon the Pacific. These are so far from being separated by any broad and trenchant line of demarcation from the proper Indian, or the so-called Red Race, that they pass gradually into it; and that in respect to their habits, manner, and appearance, equally. So far is this the case, that he would be a bold man who should venture, in speaking of the southern tribes of Russian America, to say, *here the Eskimo area ends, and here a different area begins.*" * The difference thus pointed out may be accounted for, to a considerable extent, by the diverse geographical conformation of the continent, on its eastern and western sides, which admit in the latter of such frequent and intimate intercourse as is not unlikely to lead to an intermixture of blood, and a blending of the races, however primarily distinct and diverse. The evidence presented here, however, refers to tribes having no such intercourse with the Esquimaux, and distinguished from them by many important characteristics, in manners, social habits, and external physiognomy. Nevertheless, if these conclusions, deduced from an examination of Canadian crania, are borne out by the premises, and confirmed by further investigation, this much at least may be affirmed: that a marked difference distinguishes the northern tribes, now or formerly occupying the Canadian area, in their cranial conformation, from that which pertains to the aborigines of Central America and the southern valley of the Mississippi; and in so far as the northern differ from the southern tribes, they approximate more or less, in the points of divergence, to the characteristics of the Esquimaux: — that intermediate ethnic link between the Old and the New World, acknowledged by nearly all recent ethnologists to be physically a Mongol and Asiatic, if philologically an American.

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* Varieties of Man, p. 291.

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3. On the word Celt. By J. P. Lesley, of Philadelphia.

If the new meaning which I propose for the word Celt be accepted, it will place it on a different ethnological footing. The Celtic family has been divided into two groups, the Gallic, and the Cimbric; and Gauls, Gael, Galli, *κελται*, *γαλαται*, cymri, *κιμμεριοι*, have been pronounced, more or less distinctly, to be Celtic cognomens. Historical and hypothetical migrations have been called in to account for the extensive range of these and similar names. Yet no one seems to feel the identity with them of such extralimital names as Koord and Chaldee, nor to recognize the evidently identified *Culdee* as any thing more than a religious title.

An analysis of the word, by the following formula, will put us on the track of its aboriginal use.

$$K.B.L = K'L \text{ and } K.B.L.t = K'Lt.$$

the final *t* being an attributive or possessive form. It is a law of pronunciation that dissyllables become, in process of time, monosyllables by losing either the initial, medial, or final, whichever be the weakest, or, according to another law, whichever be most difficult for the organs of the particular people in question to pronounce. The final disappears in rapid speech, and when it is a dental, and especially a liquid, as in Hebrew, bar, English, boy, etc. The medial is dropped sometimes when a guttural, but most commonly when a labial. The result is a diphthong; and the artistic construction of the Sanscrit diphthongs is merely an exception to this more general, and, in fact, universal rule. The difficulty of telling in all cases precisely what radical has dropped out from the middle of the diphthong is great, as for example in the English, Flail, which may come from Flabellum, Flap, or from Flagellum, Flügel. But it remains true that whenever, in modern words, we see a diphthong written, or a broad u or o or double oo representing a diphthong, we must begin by supposing the loss of a middle radical, and commonly a labial. The initial is dropped by some recondite organic influence, hard to make out, and also by virtue of a peculiar aboriginal mythologic influence, which it is the object of this paper to make clear. Often, we have all these abbreviations in one set of words, as cap, pate, and hat, are seen to be all contractions of caput, when we rightfully view caput as = cover. It is a curious characteristic of the Polynesian and Australian dialects, that they drop the initial whether it be a guttural or dental. Thus the Sydney *kabara* and Muruya *Kapan* become the Peel river *Bura*, the lake Maquarie *Walong*, the Bathurst *Balang*, the Wellington *Budian*, all meaning *head*, caput, *κεφαλη*, in Basque *Burua*. I have selected this word, however, not from any peculiarity in exhibiting this law, but from its very extensive range, and because it lies so near the analysis of the word Celt. To show this, I will throw some of its forms into the following table, by which the changes will be patent to the eye.

THE FORMS OF THE WORD HEAD.

Australian,	KA ——			
Mexican,	OK ——			
Chinese,	HEE ——			
Sanscrit,		kaKU-Bh —	KA-PA-LA	
German,		KO-PF —	HA-UP-T	
Latin,			CA-PU-T	
Greek,			KE-ΦA-ΛE	— BU-Dian
Australian,			KA-PA-N	— BA-Long
			KA-BA-RA	— WA-Long
				— BU-RA

Basque,	— BU-RUa	
Robber dialect of Italy,	— BO-RE-La	
Delaware and other neighboring North American Indians,	— WI-hL — WI-LA — WY-ER	
English,	— PA-Te	
Welsh,	— PE-N	— — UTa.
Polynesian,	— PE-Nu	— — ULa.

Here we have the central, typical, and full form, face to face with the lateral, partial, or suppressed forms, and it is to be remarked that this central form occupies, geographically and historically, the central position. It is the Sanscrit and Greek which gives us the pure KA-BA-R form; and yet its original life was vigorous enough to make it reappear, even in Australia. Caput and kapan, seem at first mere aberrants ($L = R \times D = T \times N$), until we introduce the Caucasian dialects, which are, perhaps, the best analytical reagents we possess, when the final in caput is seen to be an affix, as we have in Ingoushi K-WA-R-té, contracted into K'OR-té, and in Tchetchen K'AR-té and K'OR-te. We may, therefore, rewrite our series thus:—

KA"	kaKUBh'	KAPALa	'BURa	
OC"	KOPF'	KEFALe	'BURua	'' ULa.
HEE"		KABARa	'WILa	'' U'tu.
		HAUP' t	'WyeR	
		CAPU' t	'BORela	
		KAPA' n	'BaLong	
			'WALong	
			'BU'dian	
			'PA'te	
			'PE'n	
			'PE'nu	

Not to ignore, however, the transmutation L,R × D,T,N, we may consider 'ula, 'utu, bura, pate, pen, to be equivalents, and even caput and κεφαλη; but there is no alternative with the Caucasian kawarté, and the Australian terminal nasals. They prove the full form to be cabart, or cabalt, and the aboriginal meaning follows,— Belonging to or representing the cabar, i. e., cabalistic, — which the human head in all ages has been, and hence the tonsure which represents the emerged summit of the mountain surrounded by the waves of hair.

It will be seen that the point of this analysis lies in the contraction *kawarté*, *karté*. The medial labial is gone. It is also gone in Coptic, KARA, Sanscrit, S-CHIRA, English, S-CULL. It falls away, in fact, in all uses and applications of this curious root-word *kabala*. Arabic *Gibel* becomes French *Col.* Wellington *Giber* (man) becomes Sydney *kore*. Lesgian *chabbar* becomes Kisti *ker* and English *clay*. Lesgian *khimmir* becomes English *child*. Lesgian *gubur* (neck) becomes Georgian *kaili*, French *Col.* Cabriolet becomes curricle. Cowries, the Berbers call kurdi. In all languages and places we must work with the formulæ,

$$KBR = K'R,\ KBL = K'L,\ KBN = K'N,\ KBLt = K'Lt,$$

and its affiliated formulæ which I will not discuss here,

$$TBR = T'R,\ TBL = T'L,\ TBD = T'D,\ etc.$$

Both are essentially mythological, and represent those cabalistic mysteries which have always centered round the trimurti symbol of the east, the ship-mountain-flood, Vishnu Brahma Siva, UAM, which ought to be written $\left\{\begin{matrix}\text{U}\\ \text{A}\\ \text{M.}\end{matrix}\right.$ The word Celt expressed this symbol, its meanings, representatives, priesthood, worshippers, etc.

This can be set in a clear light by a few illustrations. Every thing representing a ship was called Celtic; a *coble* is a boat; canoe, kahn, is the contracted form. A camel is the ship of the desert. Every thing resembling the motion of a ship received the same epithet; a horse was called caballus; a limping man was said to hobble, contracted, by the rule above, into halt. The peculiar wavy motion of the s-quir-il probably gave it its name, as much as its upright posture, with folded hands, at the mouth of its burrow.

Every thing representing the mountain was cabalistic. Its Shemitic name is gibel (every thing huge and strong and round was called geber, great, gibbous). Its miniature is called a *ca'ern;* it *covers* the caer, or shrine, the *cavern*, which, as the *chamber* of judgment and punishment, was called the *carcer*, *ja'il*, the *κοιλον*, *cœlum*, *hollow*, *hölle*, *hell*, *cell*, or *hole*. When mountain tops emerged from the sea they were particularly Celtic, and hence so many islands called Cypris, Capri, Cabra, etc. Trees, like mountains, were Celtic, as the Cypress of the grave-yard, and the Gopher of which tradition makes the ark (κιβωτος). The Fir or Pine has only lost the KA- prefix. The metal of the caern builders was *copper*, the Celtic metal par excellence. It was only to distinguish this mountain class of Celtic things from the ship class, that a later (?) use of T for K as a prefix became common, in TABOR, TOR, TOWER, *ταυρος*, Thurm, Tumulus, Dome, Tom, etc.

The third class of Celtic words bear reference to the third element of the Trinity symbol, the M water. Cobalt was so called from its ultra marine color. It was by the river Chebar, descending directly from Ararat, that Ezechial had all his visions. The Kelpies are water spirits as the Kobolds are mountain sprites, and we have kelp explained in the Hebrew, KLB, *κυων*, hound, canis (*KBN*=*K'N*), and by the dog-headed priests of Egypt. What dogs are to men, priests are to the gods, and such was Caleb to Joshua. It was now this reference to the human part of the mystery, to the spiritual beings involved in the event, to the Noachidæ as fathers and deities, and their enemies, the waves, personified into demons, that fixed the use and meaning of the epithet Cabalistic, Celtic, so deeply in Ethnology. Wherever the story of the Cabiri went, the tellers of it were guebers, capulets, ghibelines, gael. When the old pure arkism was driven into mountains and peninsulas by heresies, and received its name of bar-bar ism, its priests and people were called giaurs, infidels (i. e. to the heresies), men of Cimmerian darkness, Cimbri, Welsh, strangers, and enemies. It is significant that the aborigines of both sides of the Atlantic bear this title in almost identical and in the purest and completest forms, namely, the Basques (Escabaras as they call themselves) and the Esquimaux, while the only ethnological connection between them is a very doubtful one, through the Fins. And it is equally significant that this complete form S-CA-BA-RA, when lawfully contracted, denominates the *Scots* and *Scyths*, two equally imperfectly related races.

The fact is that when Cabar became a name for every arkite symbol discovered in nature or constructed by priestly art, and the sum total of such symbolic objects, natural and artificial, was grouped under the mystical designation of the Cabala, the lore which grew up with successive generations of bards, and moved across the face of the earth from century to century with missionary zeal from the original arkite centre wherever that may have been, into the most distant islands of the oceans, has left its traces in our day in all languages, and has fixed the epithet Celtic to innumerable mountains, buildings, parts of the human frame, instruments of labor and pleasure, and intellectual ideas. This is the only true meaning which it has. It can have no true ethnological significance, because its propagation must have ignored the limitations of race and nation in every age. No race or nation was peculiarly Celtic, except so far as they received the Cabala, and favored the Druids or Cabalists, who were distinguished from the people whom they taught what they pleased and ruled as they chose by a peculiar language, which itself must have had its own principle of growth; a different principle from that of the vernacular tongues in the midst of which it received its diverse developments. Even to this day a man who speaks unintelligibly is said to gobble, jabber, speak gibberish; birds are said to gobble; frantic persons to cut *capers*, not from imitating goats (*CaPeR* = *Go'aT*), but dervishes. It was thus that the priests and then their followers came to be called Chaldees in Assyria, and Culdees in Britain, and everywhere Celts. The Free Masons claim Solomon as the founder of their order, and strange enough, he, or the writer who assumes his place, twice calls himself a Celt. It is surprising that every meaning has been given to the word קהלת, *Qelt*, in the first and twelfth chapters of Ecclesiastes, but the simple meaning involved in spelling the word without the masoretic punctuation. As if to leave no doubt of its relationship it is spelt with Q, opening up its whole etymology; Q-ELT, CU-ELT, CaWeLt, Cabalist; Ani Coheleth as it is commonly pronounced, I the preacher, is literally and simply Ani qelt, I Celt.

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4. LAWS OF DESCENT OF THE IROQUOIS. By LEWIS H. MORGAN, of Rochester, N. Y.

THE institutions of the Iroquois were founded upon the family relationships; in fact, their celebrated league was but an elaboration of these relationships into a complex, and even stupendous system of civil polity. At the base of this system were their laws of descent.

They were unlike both the civil and the canon law; but yet were original and well defined. The chief differences were two: first, descent among the Iroquois followed the female line, or passed through the mother, while in each of the former systems it follows the male, or passes through the father. In the second place, the collateral lines, with the Iroquois, were finally brought into or merged in the lineal; while, in the other cases, every remove from the common ancestor separated the collateral lines from the lineal, until after a few generations actual relationship ceased among collaterals.

To bring out distinctly this code of descent, it will be necessary to give a brief explanation of the division of the Iroquois into tribes, the union of the several tribes into one nation, and of the several nations into one league. Without a reference to their civil organization, it would be impossible to present it in an understandable form.

In each of the five nations who composed the original league, there were eight tribes, named as follows: Wolf, Bear, Beaver, and Turtle; Deer, Snipe, Heron, and Hawk. The Onondaga nation, therefore, was a counterpart of the Cayuga, each having the same number of tribes, and of the same name; so also, interchangeably, of the Oneida, the Mohawk, and the Seneca nations. In effect, the Wolf tribe was divided into five parts, and one fifth part of it placed in each of the five nations. The remaining tribes were subjected to the same division and distribution. Between the individual members of the Wolf or other tribe thus divided, or, in other words, between the separated parts of each tribe, there existed the tie of consanguinity. The Mohawk of the Turtle tribe recognized the Seneca of the Turtle tribe as a relative, and between them existed the bond of kindred blood. In like manner the Oneida of the Hawk tribe received the Onondaga or the Cayuga of the same tribe as a relative, not in an ideal or conventional sense, but as actually connected with him by the ties of consanguinity. Herein we discover an element of union between the five nations, of remarkable vitality and power. A cross-relationship existed between the several tribes of each nation and the tribes of corresponding name in each of the other nations, which bound them together in the league with indissoluble bonds. If either of the nations had wished to cast off the alliance, it would have broken this eightfold bond of consanguinity. Had the nations fallen into collision with each other, it would have brought Hawk tribe against Hawk tribe —

in a word, brother against brother. The history of the Iroquois exhibits the wisdom of these organic provisions; for, during the long period through which the league subsisted, they never fell into anarchy, nor even approximated to a dissolution from internal disorders.

Originally, with reference to marriage, the four tribes first named were not allowed to intermarry; neither were the last four. In their own mode of expressing the idea, each four were brother tribes to each other, and cousins to the other four; but either of the first four could intermarry with either of the last four. Thus Hawk could intermarry with Beaver, Heron with Turtle, Deer with Wolf; but not Beaver with Turtle, nor Hawk with Heron. This was the ancient law; but in process of time its rigor was relaxed, until finally the prohibition was confined to the tribe of the individual. At no time in the history of the Iroquois could a man marry a woman of his own tribe, even in another nation. All of the members of a tribe were within the prohibited degrees of consanguinity; and to this day, among the descendants of the Iroquois, this law is religiously observed. Husband and wife, therefore, were in every case of different tribes. The children were of the tribe of the mother. Here, then, we discover the central idea of their laws of descent: to place the father and mother in different tribes, and to assign the children to the tribe of the mother. Several important results followed, of which the most remarkable was, the perpetual disinheritance of the male line. As all titles, as well as property, descended in the female line, and were hereditary in the tribe, the son could never succeed to his father's title of sachem, nor inherit even his tomahawk.

A tribe of the Iroquois, it thus appears, was not, like the Grecian and Roman tribes, a circle or group of families, for two tribes were necessarily represented in every family; neither, like the Jewish, was constituted of the lineal descendants of a common father; on the contrary, it involved the idea of descent from a common mother; nor has it any resemblance to the Scottish clan, or to the canton of the Switzer. It approaches, however, nearest to the Jewish. Denying geographical boundaries, a tribe of the Iroquois was composed of a part of a multitude of families, as wide spread as the territories of the race, but yet united together by a common tribal bond. The mother, her children, and the descendants of her daughters in the female line, would, in perpetuity, be linked with the fortunes of her own tribe;

while the father, his brothers and sisters, and the descendants in the female line of his sisters, would be united to another tribe, and held by its affinities. No circumstances could work a translation from one tribe to another, or even suspend the nationality of the individual. If a Cayuga woman of the Hawk tribe married a Seneca, her children were of the Hawk tribe and Cayugas, and her descendants in the female line, to the latest posterity, continued to be Cayugas and of the Hawk tribe, although they resided with the Senecas, and by successive intermarriage with them had lost nearly every particle of Cayuga blood. Neither could intermarriage with one of a foreign nation confer the Iroquois nationality upon the wife or children of the marriage, and the same *vice versa.* If a Mohawk married a Delaware woman, she and her children were not only Delawares still, but ever continued aliens, unless naturalized as Mohawks, with the forms and ceremonies prescribed in cases of adoption.

Such property as they possessed, as planting lots, orchards, articles of apparel, etc., descended in the female line; that is to say, the wife and children took nothing from the father and husband, as they were of another tribe, except it was given to them by the deceased before his death, in the presence of witnesses. The property went to the brothers and sisters of the deceased, or to the children of the sisters. The property of husband and wife was kept distinct during the marriage, and held by separate ownership; and upon the death of the mother, her property was inherited by her children. Usually, planting lots, orchards, etc., belonged to the female. In case of divorce, each took their separate effects. The children belonged to the mother, and the authority and control of the father over them ceased from the moment of separation.

Here, then, we find, as the principal feature of this code of descent, the deflection of the inheritance from the son, and its bestowal upon the brother or nephew of the deceased. The great object of this singular provision had relation to the descent of the title of chief, or sachem, which will be elsewhere considered.

The next feature of importance in their system of descent was the breaking up of the collateral line, by merging it in the lineal, whereby the number of those who were bound together by the nearer family ties was largely multiplied. In three removes from the common ancestor, in most cases, and in four, absolutely, this result was effected.

It was accomplished by bringing the degrees of relationship nearer to each other than they are in the civil or the canon law. Thus a mother and her sisters stood equally in the relation of mothers to the children of each other; the grandmother and her sisters were equally grandmothers, and so up in the ascending series. The children of two sisters were the children equally of each other, and the grandchildren of the one were the grandchildren of the other, and so down in the descending series. On the side of two brothers the degrees were reckoned in the same manner. A difference, however, was made between the children of a brother and the children of a sister, in their relationship to each other. Thus the children of two sisters were brothers and sisters to each other; they were all of the same tribe. So also were the children of two brothers, although they might be of different tribes. But the children of a brother and the children of a sister were cousins, as in the civil law; they were necessarily of different tribes. The sister was aunt to the brother's children, and the brother was uncle to the sister's, and the children of these nephews and nieces were the grandchildren equally of each. Again, the cousins themselves were interchangeably uncles and aunts to the children of each other, and grandfathers and grandmothers to their children. By this simple process of reckoning degrees, the subdivision of a family into collateral branches was rendered impossible. A cousin who stands in the fourth degree of the civil law was the most remote collateral recognized in their code of descent, or, rather, allowed from the lineal line.

To render these degrees of relationship intelligible, it must be remembered, that a part only of the kindred of an individual were of the same tribe with himself. Thus, Sa-go-ye-wat-hä, or Red Jacket, was of the Turtle tribe of the Seneca nation. His brothers and sisters, his mother and her brothers and sisters, and his maternal grandmother and her brothers and sisters, were necessarily of the Turtle tribe; so also were the children of his sisters, and thus down through the female line. But his father, and his brothers and sisters, and his paternal grandfather, and his brothers and sisters, would be of a different, and might be of several different, tribes; so, also, his sons, and the children of his sons, would be of a different tribe, unless these sons should marry back into the Turtle tribe, against which there was no prohibition.

These laws of descent were not confined to a special class, but were of universal application; and to this day, among the descendants of the ancient Iroquois, they are preserved and recognized unchanged, and are as familiar to the rudest Indian as the alphabet is to us.*

To understand the practical use of this code of descent in its most important relation, namely, the descent of the title of sachem, it will be necessary to examine briefly the structure of the League of the Iroquois. At the institution of the league, fifty permanent sachemships or hereditary titles were created, and named. They were then distributed among the nations as follows: nine of them were assigned to the Mohawk, nine to the Oneida, fourteen to the Onondaga, ten to the Cayuga, and eight to the Seneca nation. These titles were made hereditary in certain tribes, some of which received two or more, and others none. These sachemships could never pass out of the tribe to which they belonged, except with its extinction. While the office of sachem was absolutely hereditary in the tribe, it was, at the same time, elective as between certain of the male relatives of the deceased sachem of the same tribe with himself.

The title of sachem was surrounded by insuperable barriers against the designs of talented and ambitious men, for reasons of policy; and the safeguards against usurpation were too deeply integrated in their institutions to be overcome or superseded. How this was accomplished was, for a long period, difficult to be understood; but the intricacy is removed by the single fact, before stated, that the title was hereditary in the tribe, but elective as between certain of the male relatives of the deceased sachem. It will not be necessary to explain minutely how the choice was made, further than to say, that, if the title belonged to the Wolf tribe, the new sachem must be "raised up," to use their own expression, from the same tribe. As the son of the sachem was of another tribe, he was out of the line of succession; but his brothers were of the Wolf tribe, and so were his sister's sons; hence we find that the succession fell upon a brother of the deceased ruler, or upon a nephew. Between a brother of the deceased and the son of a sister there was no law establishing a preference; neither as between several brothers on one side, or several sons of a sister on the other, was there any law of primogeniture. They were all equally eligible, and the law of election came in to decide between them. The choice was made by the wise men and matrons of the tribe; and among the latter the mother of the deceased ruler exercised a decisive influence.

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* The following are the names of the several degrees of relationship recognized among the Iroquois, in the dialect of the Senecas. ä as in art. ă as in at.

Hoc-sote′, Grandfather.
Uc-sote′, Grandmother.
Hä′-nih, Father.
No-yeh′, Mother.
Ho-ah′-wuk, Son.
Go-ah′-wuk, Daughter.
Kä-yä′-dä, Grandchildren.

Hoc-no′-seh, Uncle.
Ah-geh′-huc, Aunt.
Hä-yan-wan-deh′, Nephew.
Kä-yan-wan-deh, Niece.
Dä-ya-gwä-dan-no-dä, Brothers and Sisters.
Ah-gare′-seh, Cousin.

Upon the decease of a sachem, and the choice of a successor, a council of all the Sachems of the League was convened to "raise up" the new ruler, and invest him with his title. To this council belonged the exclusive power of investing with the office; and no one could become a sachem in fact, until this ceremony of investiture was performed. These councils lasted several days, and were attended with many forms and ceremonies. They are still held in western New York as often as each alternate year.

These sachems were the rulers of the people, partly by elective, and partly by hereditary, right; but their duties and authority were confined exclusively to the affairs of peace. When assembled together, they formed the general council of the league, and possessed, in themselves, the executive, legislative, and judicial powers of the Commonwealth. In the same manner the several sachems of each nation composed the national council, which exercised a separate government over all the affairs of their respective nations, such as did not relate to the general welfare.

Many years after the formation of the league a new office was created, the office or title of *chief*. It was of lower rank than that of sachem, and was not hereditary. It was in the strict sense elective, and the reward of merit, and ceased with the life of the individual. To this class the most distinguished of the war captains and orators of the Iroquois belonged; among them, Tä-yen-dä-nae-ga, or Joseph Brant, and Sa-go-ye-wat-hä, or Red Jacket. At the present time the Seneca nation, in western New York, have eight sachems, as of old, who hold their titles by the original tenure, and about seventy chiefs, who hold by election.

It thus appears that the government of the Iroquois was an hereditary oligarchy, or the rule of a few select men, under limited and peculiar laws of descent. Unlike the oligarchies of civilized states, which have proved the most unstable of all forms of government, that of the Iroquois had a systematic, even constitutional, structure; and endured from age to age unbroken and unchanged, growing with increasing strength until the advent of the Saxon race.

It is an interesting fact, that the sachems of the Iroquois at the present day, although the league is dismembered, and the nations are scattered, still bear the same individual names which were borne by their predecessors at the establishment of the league. Thus Ho-no-we-nă-to, which means "Keeper of the Wampum," was the name given to one of the fourteen original Onondaga sachems. All of his successors, through many generations, down to the present Ho-no-we-nă-to, now at Onondaga, have held the same title, and borne the same name. Do-ne-ho-gä-weh, the "Keeper of the Door," was the name of one of the eight original Seneca sachems. This title, in like manner, has been held by all of his successors, down to the present day. Ely S. Parker, an educated Seneca, at the present time in the civil service of the United States, now holds this sachemship. When he was raised up, a few years since, his former name, Hä-seh-no-an-da, was "taken away," to use again their mode of expression, and the name Do-ne-ho-gä-weh bestowed in its place, by which alone he is now known. The office of sachem, therefore, is a title of nobility, but descending in the female line, instead of the male, and having attached to it the authority and powers of an hereditary ruler of the Iroquois.

Such is a brief outline of their laws of descent. The closer they are inspected, the more the unity and clearness of the system appears; but to enter into minute details would not be appropriate to the present occasion. In itself considered, this code of descent is of very little importance; and although it furnishes a singular illustration of the cast of the Indian intellect, its presentation here could not be justified on the grounds of mere curiosity.

This brings me to the precise object which led to the presentation of this subject; and it is this: — Can this code of descent, or any other original, well-defined, Indian institution, be used as a test of the truthfulness of history? In the second place, can it be employed as an instrument in the attempt to solve the great problem of the origin of our Indian races? If it can be used for either purpose, that fact invests it with a high degree of importance.

Let us observe that the primary institutions of a people are necessarily permanent from age to age, and only change when the whole constitution of society is changed. While in the nomadic or hunter state, institutions of this character are as permanent as the state in which they are developed. It is only by the entire and absolute transmutation of a race from the hunter to the civilized condition, that such institutions can be eradicated; and even then they rarely disappear entirely, as witness the tribes of the Athenians. Not even language itself will be found to be more stable than the domestic institutions within certain limits. Now it is very possible that a primary institution of an aboriginal people, may, if diligently explored among the races of the earth now living in the nomadic or hunter state, lead to important deductions in relation to the ultimate affinity, as well as origin, of generic races. And these results would repose with solidity upon the necessary permanence of such institutions, as this fact of permanence excludes the idea of accidental coincidence.

The uses of this code of descent, as a test of history, may be illustrated by the following example, but in a case in which the truth is of but little importance. Mr. Stone, in his Life of Brant (I. 8–14), thought it desirable to prove that Brant was a sachem; and therefore a man of position and influence among the Iroquois. To do this he attempts to show that his father was a Mohawk sachem of the Wolf tribe, and thus devolves the title upon him by descent. The names of the two persons, each of whom was reputed to be the father of Brant, are given, and they are both claimed, by the author, to be sachems; but it so happens that neither of their names, nor that of Brant, is found in the authentic list of the nine Mohawk sachems. And, further, had he succeeded in showing that his father was a sachem, that fact would have insured Brant's disinheritance.

A more interesting example is found in the descent of the pretended crown of Mexico, or of the Aztec monarchy. According to current histories, the crown descended either to a *brother* or to a *nephew* of the deceased emperor, and not to his *son*. As between his brother and his nephew it was elective; but under what laws or regulations the election was made, no very satisfactory account is given. Mr.

Prescott, in his work upon the Conquest of Mexico (I. 23), says, "The government was an elective monarchy. Four of the principal nobles, who had been chosen by their own body in the preceding reign, filled the office of electors, to whom were added, with merely an honorary rank, the two royal allies of Tezcuco and Tlacopan. The sovereign was selected from the *brothers* of the deceased prince, or in default of them, from his *nephews*. Thus the election was always confined to the same family. The candidate preferred must have distinguished himself in war, though, as in the case of the last Montezuma, he was a member of the priesthood. This singular mode of supplying the throne had some advantages. . . . . The scheme of election, however defective, argues a more refined and calculating policy than was to have been expected from a barbarous nation." This account is very brief and general, but it so happens that the truthfulness of this law of descent is verified in both particulars. Montezuma was succeeded by his brother Cuitlahua; and upon his decease, a few months afterwards, he was succeeded by his nephew Guatemozin, also a nephew of Montezuma.

Had the researches of this elegant writer brought him in contact with the real institutions of the Aztecs which controlled this question of descent, he would have discovered, there is every reason to believe, that the people were divided into tribes, with laws of descent precisely similar to those of the Iroquois. That no Aztec could marry into his own tribe; that the children were of the tribe of the mother; and that all titles were hereditary in the tribe, and descended in the female line. This also furnishes a sufficient reason why Montezuma was succeeded by his brother, and the latter by his nephew; and why it was impossible for a son to succeed his father.*

In a note (I. 39, n.), Mr. Prescott adverts to the existence of tribal divisions of the people, as follows: "The people of the provinces were distributed into calpulli, or tribes, who held the lands of the neighborhood in common. Officers of their own appointment parcelled out these lands among the several families of the calpulli, and on the extinction or removal of a family, its lands reverted to the common stock, to be again distributed. The individual proprietor had no power to alienate them. The laws regulating these matters were very precise, and had existed since the occupation of the country by the Aztecs." The land system of the Iroquois was much the same, but with this addition, which was probably true also of the Aztec, that an individual could sell his improvements; and as the fee of the land belonged to the people in common, it was practically equivalent to owning the fee. The Indian races of this continent have always held their lands by this simple tenure. Any one can occupy vacant land, and improve it; and this act makes it private property. When abandoned, it reverts to the common stock, and is open to the next occupant. If he sells his improvements, as he may, the purchaser is protected in his enjoyment. The note above referred to contains all that Mr. Prescott thought worthy of mention concerning the tribes of the Aztecs; and he does not appear to have recognized their tribal organization as a primary institution of any importance.

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* The tribal device of Montezuma was an "Eagle," from which it is to be inferred that he belonged to the "Eagle tribe," and that the sachemship held by him was hereditary in that tribe.

It is well known that the early Spanish writers, upon the conquest, are full of contradictory assertions, exaggerations, and fabulous statements. Very little reliance is to be placed upon them. On the other hand, the institutions of our Indian races are obscure and complicated; and can only be worked out by careful and patient research, carried down to minute particulars. Those of the Iroquois were unknown, until within the last twenty years, although the Jesuit missionaries, and both French and English travellers, had written volumes upon their civil and domestic affairs. The real structure and principles of the league eluded their inquiries. Its general features were well known, but were so incumbered by errors that the knowledge was of but little value. It would not be surprising if the same were true of the institutions of the Aztecs.

Now the institutions of all of the aboriginal races of this continent have a family cast. They bear internal evidence of a common paternity, and point to a common origin, but remote, both as to time and place. That they all sprang from a common mind, and in their progressive development have still retained the impress of original elements, is abundantly verified. The Aztecs were thoroughly and essentially Indian. We have glimpses here and there at original institutions which suggest at once, by their similarity, kindred ones among the Iroquois and other Indian races of the present day. Their intellectual characteristics, and the predominant features of their social condition, are such as to leave no doubt upon this question; and we believe the results of modern research, upon this point, concur with this conclusion. Differences existed, it is true, but they were not radical. The Aztec civilization simply exhibited a more advanced development of those primary ideas of civil and social life, which were common to the whole Indian family, and not their overthrow by the substitution of antagonistic institutions.

Judged, then, from the institutional point of view, the Aztec monarchy, as described to us by current histories, will not bear the test of criticism. So far as the structure of the government is concerned, a serious doubt rests upon the whole narrative. The testimony, drawn from the very nature of true Indian institutions, denies that the Aztec government was a monarchy. Nay, it asserts that it is utterly impossible that it could have been a monarchical government. Venturesome as this statement may appear, it is yet proclaimed and vindicated by the principles and structure of Indian society. If we could now break through the overlying mass of fable and exaggeration, and bring to light the real institutions of the Aztecs, it would be found, there is every reason to believe, that their government was an hereditary oligarchy, very similar to that of the Iroquois. That Montezuma, so far from being emperor of the Aztecs, was one of a large number of sachems, who, equally, by their joint authority in council, administered the affairs of the commonwealth. That, as the leading sachem residing in the metropolitan city, he was first brought in contact with the Spaniards; and they, having taken it for granted that he was the emperor, determined that he should be emperor, right or wrong. The splendor and power of the Aztec monarch, as set forth in their recitals, tended, in no inconsiderable degree, to magnify their exploits.

The evidence which supports this position is drawn from the nature of their institutions, so far as they are known, from the structure of Indian society, and from the general principles of action which distinguish the hunter state. The necessary limits of this article will not admit of the presentation of this evidence here. Attention, however, may be called to a single fact stated by Mr. Prescott. "According to some writers of authority," he says, "there were thirty great *caciques*, who had their residence, at least a portion of the year, in the capital, and who could muster a hundred thousand vassals each, on their estates. Without relying on such wild statements, it is clear from the testimony of the conquerors, that the country was occupied by numerous powerful chieftains, who lived like independent princes on their domains" (I. 26).

This division of generic races into tribes, with descent in the female line, has prevailed very generally throughout the Indian family. Besides the Iroquois, and with every probability the Aztecs, we may mention that the Creeks, were divided into ten tribes; the Chickasaws into six; the Ojibeways into thirteen; and the Delawares into three. As the last two were the principal branches of the Algonquin family, it is reasonable to infer that similar laws of descent prevailed throughout all of its subdivisions.

Neither was it confined to this continent, but is found, at the present time, on some of the islands of the Pacific. In 1852, missionary stations were established among the Micronesian islands. One of them, Ascension island, belonging to the Kingsmill group, was occupied by the Rev. Dr. Gulick. He writes to the Board as follows: "The present population (of this island) is divided into five hostile tribes, each of which is divided into seven or eight independent clans" (Miss. Her., May, 1855, p. 131); or, as we should say, there were five nations, and each nation was divided into seven or eight tribes. In like manner, the Rev. Mr. Snow, an associate missionary, but located upon Strong's island, writes, in relation to the succession of chiefs, that by a singular provision the office descended to a nephew of the deceased chief, and not to his son. In a previous report, Dr. Gulick, referring to the oldest European resident upon the island, and who had married a native woman, says, "A former wife of Mr. Corgat was a high chief, and his children, now living, are high chiefs in the tribe, for *rank descends through the wife*" (Miss. Her., 1853, p. 90). The writer has been informed, but cannot verify the fact at the present moment, that similar laws of descent prevail among the native tribes of Australia, and also of New Grenada, in South America.

Whether this code of descent came out of Asia, or originated upon this continent, is one of the questions incapable of proof; and it must rest, for its solution, upon the weight of evidence, or upon probable induction. Its existence among American races, whose languages are radically different, and without any traditional knowledge among them of its origin, indicates a very ancient introduction; and would seem to point to Asia as the birth-place of the system. The writer has no means of ascertaining whether similar laws of descent existed among the Tartar races. It is to the Tartar branch of the Mongolian family that attention would first be excited, from the pertinacity with which some of them have adhered to the nomadic state, and from the similarity of domestic habits to be found in the hunter life. Among the classic races we should hardly expect to find any similar institution, and yet in the account given of the Lycians, of Asia-Minor, by Herodotus (Herod. Lib. 1, c. 173), we have a glimpse at a system of descent, which, as far as it is explained, is analogous to that of the Iroquois. He says, "Their customs are partly Cretan, and partly Carian; but they have one peculiar to themselves, in which they differ from all other nations, for they take their name from their mothers and not from their fathers; so that if any one ask another who he is, he will describe himself by his mother's side, and reckon up his ancestry in the female line. And if a free-born woman marry a slave, the children are accounted of pure birth; but if a man, who is a citizen, even though of high rank, marry a foreigner, the children are infamous."

Before dismissing this subject, another question remains to be considered. For what particular end was this code of descent established? What great object was sought by making the title of chief or sachem hereditary in the female line, thereby perpetually disinheriting the son? Several reasons may be assigned without, perhaps, giving the real one. It is quite apparent that it secured the purity of descent by a rule infallible; for the sachem was necessarily the son of his mother, although he might not be the son of his mother's husband. Again, by assigning one or more sachemships to a tribe, the individuality of the tribe was maintained; and thus the unity and integrity of Indian society, in its primary organization, were preserved. But neither of these sufficiently account for it. There is another view of the matter which may, perhaps, come nearer the truth. The hunter state is essentially one of individual freedom; and this freedom, as well as the mode of life to which it was incident, became a passion to which the Indian mind was attuned from its lowest depths. He desired, and would tolerate, no other state. Under this tribal organization, generic stocks might divide and subdivide indefinitely, until they spread over a continent, and still preserve the integrity of the hunter state. They could also unite their tribes into nations, and the nations into a league, for mutual protection. It mattered not how far they spread apart, by emigration, from the original centre, a national unity would be maintained until they were separated, geographically, by intervening races. Now it is evident that this tribal organization, with the descent of the office of sachem in the female line, and limited to the tribe, was eminently adapted to the hunter state; and, secondly, that it was preëminently suited to the formation of confederacies of kindred tribes. The original tribe divided into two, and subsequently, as their numbers increased, the two subdivided into four, and perhaps again into eight, creating new sachemships with each subdivision, and spreading abroad over a larger area. They were still of common origin, bound together by the ties of consanguinity, and the several tribes intermingled through and through by intermarriage. Although, by the lapse of time, they had become scattered over a large territory, all the necessary elements of federation existed in the very structure of their society. Hence it was perfectly simple and natural to retrace the steps by which they had been weakened through subdivision, and seek for its obvious correction in a league of tribes. This is, in reality, a condensed statement of the actual history of our Indian races. They have undergone a process of repeated and continuous subdivisions from age to age, but counteracted here and there by confederacies. We know that these confederacies have existed, and still exist, in places, all over this continent; as witness, among others, the league of the Iroquois, the Powhattan confederacy, in Virginia, the Sioux league of the Seven Council fires, and the alliance between the Aztecs, Tezcucans, and Tlacopans. But, on the other hand, we have never known of an Indian monarchy on any part of it, unless we accept the pretended Aztec monarchy. By the junction of several tribes into one nation, and several nations into a confederacy, the people are brought under the joint authority of the sachems of the several tribes, who, in general council, administer all such affairs as relate to the common welfare, leaving each tribe and nation to the particular government of its own sachems. Such a government was that of the Iroquois; and substantially, without much doubt, the form of government which prevailed among the whole Indian family upon this continent.

But, on the other hand, break down this barrier of descent in the female line, and allow the sachem to perpetuate his power in his own family, and it is easy to see that a consolidation of tribes under one sachem would be the result; and this process of consolidation would progress by conquest, until despotism would succeed to freedom, and the hunter state would be overthrown.

This tendency was arrested at the fountainhead, by two simple provisions. The first, was the disinheritance of the son by their laws of descent; and the second, was the total separation of the military from the civil power. This last fact has been elsewhere adverted to; but its importance has not been made sufficiently prominent. Under the Indian theory of government, the sachem was a civil ruler, whose duties were confined to home or peace affairs. He was not prohibited from going out upon the war-path, but before he could do so, he was compelled to lay aside his office. The Indian war system was purely a voluntary one. Theoretically, every nation was at war with every other nation with whom it had not formed an actual alliance. Any one, but a sachem, could enlist a band, and go out upon his own responsibility, and he thus become the captain, by the compact of enlistment, for this single enterprise only, of those who agreed to follow him. When two or more bands united upon a common expedition, each one continued under its own captain, and all joint military measures were agreed upon by the war captains in council, as they rarely ever appointed a commander-in-chief. When the Iroquois sachems, in general council, declared war against any other nation, no levies were made, or public measures adopted for its prosecution, but it was left to private enterprise and the sense of patriotism; and the war itself was conducted by volunteers; by volunteer captains, as well as warriors. Now it is easy to see what a powerful influence these two limitations, upon the authority and influence of their sachems, exercised for the perpetuation of the hunter state. Whether they are to be placed, with reference to this state, in the relation of *cause*, or of *effect*, they go far to account for the otherwise extraordinary phenomena of Indian life.

From these limitations, the office of sachem never conferred any great degree of authority or influence; neither was it favorable for the development of talent. The war captains and orators were usually far more influential, among the people, than their rulers. We have a singular proof of this fact in the sachems of the Iroquois. Although there were fifty of them at all times in office,* and this number must be multiplied by as many generations as have passed away since the league was formed, yet not one of this long list of sachems is particularly distinguished in their own traditions, except the first To-do-dä′-ho, and the first Da-gä-no-wé-dä, who were the founders of the league; and not one of them has left a name among us, except Logan, who, under his true Indian name, was one of the ten Cayuga sachems, and Gä-ne-o-di′-yo, one of the Seneca sachems, better known as Handsome Lake. All of their distinguished men belonged to the class of chiefs, who were elected such in reward of merit; among the number may be mentioned Red Jacket (Sä-go-ye-wät′-hä), Brant (Tä-yen-dä-nac′-ga), Gar-ran-gu-la, Schenandoah (Sken-an′-do), Cornplanter (Gy-ant′-wä-ga), Steeltrap (Kar-is-ta-gi-a), and Big Kettle (So-nó-jo-waw-ga).

The American Indians must have discovered, by experience, that in the precise ratio in which they increased the power of their chiefs, they surrendered their personal freedom; and as their history upon this continent bears continuous and overwhelming testimony to their enthusiastic devotion to the hunter life, it is not unreasonable to conclude, that they cherished such institutions as would perpetuate this state in its integrity, and resolutely opposed such innovations as would endanger the freedom it bestowed.

C. PRACTICAL SCIENCE.

1. VIEWS AND SUGGESTIONS ON THE PRACTICE AND THEORY OF SCIENTIFIC PUBLICATION. By Lieut. E. B. HUNT, Corps of Engineers, U. S. A.

IT is much to be regretted that physical investigators labor under so many and serious disadvantages for the successful prosecution of their researches. In addition to such hinderances as result from the stern necessity of procuring a livelihood, there are many which do not seem necessary, but which still effectually obstruct that progress towards new truth, by which the man of science measures success. Not least among these adverse circumstances is the almost universal fact, that the man of science has not the command of those books and special memoirs, in which are recorded the methods and results of anterior investigations. Except in the rare cases of conjunction between science and wealth, there is a perpetual inability to compare the research in hand with the great body of related researches of the past and present times. It is scarcely possible that one's theme should be entirely his own property, and, except in this rare case, it is very needful that he should have free and habitual access to all records bearing on his particular line of research. Now this is utterly impossible, unless the investigator lives in convenient proximity to some one of the principal libraries. There is not yet, in this country, any single library at all equal to the entire demands of our whole body of investigators, and the two or three which are slowly approximating to this position of scientific catholicity, are only very partially available beyond the charmed circle of "an easy walk." In making a birds-eye examination of from six to seven thousand volumes of scientific memoirs, periodicals, etc., while gathering materials for an index of references, to be published by the Coast Survey, I have been more and more impressed with the vast affluence of the published records of science, and of the capacity of the many thousands of memoirs and articles passed in review for a strict classification under specialities. It is entirely a safe assertion, that no specialist, however restricted his speciality, is fully cognizant of all the published matter relating to his field; and also, that without this full knowledge of labor and thought already expended, he must be wasting much of his own strength in doing again what others have completed. In point of fact, most of the active, truth-seeking minds of this country are compelled to work on, remote from the chief central depositories of books and memoirs, happy if from their limited private resources they can secure, as a private possession, that carefully chosen minimum of special treatises which will give a starting point somewhere near the front line of actual research or discovery. Nearly all such isolated effort is expended too much at venture to bear the fruit of discovery to which it ought and is entitled to aspire. Not one in twenty of the members of this Association has the command of all those special records of research which are needful to the symmetry and completeness of his own labors. There may be, in the Astor Library, a dozen closely related memoirs of which a specialist knows the existence, and a dozen more of which he has never heard; but if he must journey some thousands or hundreds of miles, at his own cost, to study them, from 10 A. M. to 5 P. M., he is fortunate if he can use them on these conditions. The result is, that only a few amongst us are able to do as we should, had we a complete library within easy reach. Such is nearly the universal truth concerning those who are by nature endowed with the faculties requisite for successful research. It cannot, therefore, be entirely profitless to consider what possible alleviations of this condition are practicable.

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* There were, in reality, but forty-eight. Two of the Mohawk sachemships were left vacant after the demise of the first sachems who held the titles. They were Hä-yo-went-hä and Da-gä-no-wé-dä. Tradition says, they were accepted on these terms, which were imposed by the sachems themselves.

First, then, it is a fixed tendency of scientific research to subdivide, during its progress, into numerous and well defined specialities. Each new branch of investigation soon subdivides into other and more limited branches. Though all are ultimately derived from one grand trunk, so vast that not even the giants of mind can span its circumference, the special branches have a narrowness of diffusion, which brings them quite within the compass of a single scientific mind. It thus happens that though many thousands of separate memoirs or investigations are preserved in the aggregate records of science, the pure specialist will find but few which are related to his own chosen field; so few, indeed, that, could they be bought separately at the ordinary prices for printed matter, he might, without harming his fortunes, possess as his own the essential materials for a complete study of his speciality. If, however, he attempts this solution of his wants, he is at once confronted by the fact, that this small group of memoirs cannot possibly be purchased without buying in connection the very costly series to which they severally belong. For the most part, the series of scientific memoirs is grouped together, without reference to subject, in a most illogical congeries, having only the date and the publishing society in common. Hence the specialist, when outside the limits of easy access to a complete library, must rely almost entirely on separately published special treatises for his knowledge of what has been done, and the original memoirs of the master-minds must be forbidden treasures.

It is, however, obvious that such a lame condition of things is not one of original necessity; it has grown out of the history of scientific publication. If we go back to the times preceding the existence of academies of science, we find nothing answering to the scientific memoir. The academies formed from those who were rich in ideas, but poor in purse, first found that, by associating the limited wealth of a number, the great result, otherwise denied, could be attained. When, too, they assumed an imposing exterior, through the lustre of great names, the condescensions of patronage intervened, and the luxuries of type and engraving began to be lavished on the volumes in which their proceedings and choice memoirs were published. As science expanded its domains, and augmented its small array of followers, academies sprang up in profusion and in ever increasing numbers, until now they have outrun the knowledge of even the most learned librarians. Nearly all of them have published, or now publish, volumes of memoirs, which are the tokens of their vitality to all not within the chosen band. Wherever strong discovering minds have elaborated their researches, the volumes of memoirs are enriched with treasures of close reasoning, sagacious insight, and results laboriously attained. These memoirs are still fresh with the vigor of original thinking, and cannot be spared, as part of the training of future discovering minds. When original memoirs give place, in the studies of investigators, to bald abstracts, feeble analyses, or popularized digestions, these investigators will grow sinewless, and discoveries, if made at all, will come despoiled of all their proper grandeur. We cannot too strongly deprecate the tendency, even among specialists, to know the labors and investigations of original scientists only through abstracts and synopses or general reports. These are indispensable aids for the knowledge of the immense related area outside of one's speciality, which he can only expect to know at second-hand, and by the briefest method. But they are not among the legitimate means of advancing the boundaries of his own special field. Direct resort to original records is the prime condition on which the symmetry and well furnished culture of specialists must rest.

In the true and philosophical sense of the word, most of the printed scientific memoirs cannot be said to have been published. Publication implies something more than mere printing. It involves the idea of circulation or diffusion among the whole group of kindred minds specially interested in the particular subject-matter. A memoir on electro-magnetism, for instance, is not published when it is printed in the

St. Petersburg Bulletin, because very few of those specially prosecuting electro-magnetism habitually see this Bulletin, and single copies are not readily obtained. It is possible that nearly all of this class will somewhere see a bald announcement, that Lenz has, after many experiments, come to such and such conclusions; but this leaves them almost as much in the dark as before. Each memoir has its special public, diffused over all lands where science is cultivated, and is fully published only where it has reached the whole of this public. In some instances this special public would number less than a hundred; in some others, it would be many thousands. To all not members of its own proper public, any special memoir is as an utterance of unknown tongues, and however much they may reverence, they cannot duly appreciate it. If, therefore, a society or academy has procured the printing of a valuable memoir, it has the great matter of publication still left to attend to, so long as those specialists throughout the world, for whom the memoir is really meant, are not duly supplied with printed copies. A memoir may nearly as well not exist, as be printed and not properly circulated. As a matter of fact, a large proportion of the most valuable printed memoirs have never been published at all. There is enough science buried in the documentary files of our general and state governments to fill a long series of volumes. These scientific waifs are scattered broadcast among politicians and their constituents, but the humble devotee of science, who spends years of labor in kindred researches, vainly strives to possess himself of a copy. In such cases, myriads of copies may be printed, and yet the memoirs are not published. On the other hand, if there be a special memoir on a subject which only ten men have studied or will study, the supplying each of these ten with a copy is a complete publication.

A great portion of the failure to publish printed memoirs is clearly consequent on the mode of publication. Nearly every academy prints its own proceedings and memoirs, aided by citizens whose local pride is enlisted in the promise of scientific distinction. With the printing, however, the matter is prone to end, there being no effective mode of publication. Scientific periodicals, in like manner, though far more generally published than academic memoirs, are still limited to a small and inadequate circle of subscribers, and beyond this circle they are in effect not published.

It is evident, on reflection, that the great desideratum for the correction of the evil indicated, is a *universal publishing agency* for all scientific memoirs printed by societies and academies, and for the periodicals of the entire scientific world. Without at all interfering with the formation of regular library files by society donation or purchase, as is now done, such an agency would have enough to do in supplying all specialists with their special memoirs. The expansion of this idea into practical form would, of course, rest on the concurrence of all the societies, etc., which publish memoirs. I can, perhaps, most clearly indicate its workings by supposing the agency established; its system might be, briefly, this: —

An agency is duly organized and guarantied to the world, by being placed under the control of a board or well-established institution or society. The agent puts himself in correspondence with all the societies which publish memoirs, and, after fully explaining the objects of the agency, makes arrangements with each society to print a given number of separate copies of each memoir issued, making what might be called the specialists' edition. The society might either furnish these copies at the cost of manufacture, or at a fixed price, or, if it is able, it might donate them. Thus the universal agent will be supplied with unbound copies of each memoir. He is all this while supposed to be corresponding with all the specialists, for the purpose of ascertaining what class of memoirs each one may desire. He will soon be able to supply printed lists of titles, whereby each investigator can know what memoirs just published or announced as forthcoming he may desire. Thus a perfect understanding of the relation between supply and demand might be attained. This being all done, and the agency fully supplied, Prof. A. B., living anywhere, orders a given number of particular memoirs, at prices indicated in the list. He has only a limited surplus of means to bestow, but he can thus supply his exact wants at the minimum cost, and without having to buy a large number of memoirs he does not want. The result will be, that Prof. A. B., in a town remote from large libraries, can proceed in his chosen studies, with all the aids needful for placing himself in advance of the known, and thus becoming an effective discoverer. The main object would be not profit to the agency, but simply to give demand a full opportunity to call upon supply. As far as it is now possible, without reprints, the memoirs hitherto published would be included in the range of operations. An author publishing a special memoir in a periodical, by sending copies to such an agency, would be sure of their going to the right persons. It is likely that many economical reprints might in time be justified. The main idea, however, would be to insure the proper publication of new memoirs as they are printed. Such an agency ought not to be an ordinary commercial agency without guaranty of its continuance, but it should rest on a firm basis, so as everywhere to deserve and possess confidence.

There are laws of political economy underlying the philosophy of publication, which have been too little regarded in scientific issues. They have mostly been published on the plan of large prices and small circulation. The right plan is a maximum circulation and a minimum paying price. There are enough ready to buy almost any special memoir to authorize its being sold at a low price. If, however, a high price is affixed, it becomes at once prohibitory. Such publications as Taylor's Scientific Memoirs, and Liebig and Kopp's Reports, ought not to be expected to succeed at such prohibitory prices as they bear. All that is necessary to insure success to the principle of minimum paying prices and maximum circulation is simply to organize the demands of scientists throughout the world, so as to insure to such specialist a chance of purchasing precisely what he wants at fair rates. By doing this, scientific progress will be immensely stimulated, and the poor barriers of cliques, nations, languages, and races will be swept away before the widely diffused liberalities of truly cosmopolite science. If the imperfect glimpses towards some vital generalities which I have now essayed shall lead to a more correct appreciation of existing evils, and to active reflection on the possible meliorations in the condition of the scientific body, my purpose will be fulfilled.

TITLES

OF

COMMUNICATIONS.*

A. MATHEMATICS AND PHYSICS.

1. Note on the Conservation of Force. By Benjamin Peirce.
2. Memoir on the Molecular Constitution of Material Masses. By E. B. Hunt, Lieut. U. S. Engineers.
3. Note on the Gyroscope. By Benjamin Peirce.
4. Some further Statistics and Inquiries with respect to the Form, Magnitudes, etc., of the Asteroid Planets. By Stephen Alexander.
5. On the Resemblance between the Arrangements of the Solar System and the Phyllotaxis in Plants. By Stephen Alexander.
6. On the Special Harmonies of the Distances and the Periodic Times of the Bodies composing the Solar System, and the Physical Hypothesis to which this state of things seems to be referable. By Stephen Alexander.
7. On some Special Relations of the Straight Line, and the various Orders of Curves. By Stephen Alexander.

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* The following papers were also read, and would have been printed in full, if a copy of them had been furnished for publication. It was ordered by the Standing Committee, in conformity with the practice of several years, that "no notice of articles *not approved* shall be taken in the published Proceedings."

8. On the Diverse Weights employed in Modern Coinage. Part I. On Troy Weights. Part II. On Karat Grain Weights. Part III. On Avoirdupois Weights. Part IV. On the Metrical System of France. By J. H. Gibbon.

9. On a Peculiar Form of the Fiducial Barometer. By J. Constantine Adamson.

10. An Inquiry into the Causes of a Peculiar Discoloration of Coffee, which had been Stored in the same Building with Guano. By Dr. Lewis H. Steiner.

11. A Note on Arithmetical Complements. By Thomas Hill.

12. Sketch of a Plan proposed for reducing Observations upon Periodical Phenomena to a Series of Mean Dates, and the Advantages of this Method in Developing the Laws of Climate. By Franklin B. Hough.

13. A Critical Review of the different Measurements of Mount Washington, in the White Mountains of New Hampshire. By Prof. Arnold Guyot.

14. On a Series of New Meteorological and Physical Tables prepared for the Smithsonian Institution. By Prof. Arnold Guyot.

15. On the Barometric Formulæ for the Measurement of Differences of Level by the Barometer, and a proposed Modification of Laplace's Constant. By Prof. Arnold Guyot.

16. On a New Maximum Thermometer. By Thomas Green.

17. On a New Method of Determining the Commercial Value of Saltpetre. By E. N. Horsford.

18. On the Painting of Pictures with Both Hands. By E. N. Horsford.

19. On Surety Paper. By E. N. Horsford.

20. On some Phenomena of Ice. By Prof. Joseph Henry.

21. On the Physical Conditions determinate of the Climate of the United States. By Prof. Joseph Henry.

22. Observations on the Zodiacal Light at Quito, Equador, with Deductions. By Rev. Geo. Jones, U. S. N.

23. Some of the Phenomena of the Texas "Norther" and Climatology. By G. C. Forshey.

24. On an improved Construction of Ruhmkorff's Induction Apparatus. By E. S. Ritchie.

25. Improvement in the Construction of the Achromatic Telescope. By H. L. Smith, Kenyon Coll., Gambier, O.

26. On some Modes of preventing the Counterfeiting of Bank-Notes. By T. Sterry Hunt.

27. A Description of several new Ternary Compounds — Chromites. By A. K. Eaton.

28. On Bank-Notes, and the Means adopted for Prevention of Counterfeiting. By Benjamin Silliman, Jr.

29. Notice of a New System of Dressing Metallic Ores. By Benjamin Silliman, Jr.

30. Solar Eclipse of March 14–15, 1858. By Thomas Hill.

B. NATURAL HISTORY.

31. On the Age and Dip of the Connecticut River Sandstones, and the Intercalation of the Associated Trap. By Edward Hitchcock.

32. Contributions to the Geology of California and the Gadsden Purchase. By T. Antisell.

33. On the Origin of Anthracite and Bituminous Coal. By Charles Whittlesey.

34. Note on the Formation of Continents. By Benjamin Peirce.

35. On the Physical Structure of the Continent of Africa. By Arnold Guyot.

36. On the Cretaceous Formations of the United States and the North American Continent. By James Hall.

37. On the Carboniferous Limestones and Coal-Measures of the United States. By James Hall.

38. On the Direction of Ancient Currents of Deposition, and the Source of Materials in the older Paleozoic Rocks, with Remarks on the Origin of the Appalachian Chain of Mountains. By James Hall.

39. On the General Plan and Mode of conducting the Geological Survey of Great Britain. By A. C. Ramsay.

40. On the Physical Break and the Break in the Succession of Life in the British Rocks. By A. C. Ramsay.

41. On the Occurrence of Sharks' Teeth in the Drift of the Mississippi. By A. H. Worthen.

42. On the Parallelism between the Rocks of the Laurentian and Silurian Systems. By T. Sterry Hunt.

43. General Considerations on the Metamorphism of the Sedimentary Rocks. By T. Sterry Hunt.

44. On some Mineral Waters, and on the Origin of Magnesian Rocks. By T. Sterry Hunt.

45. On a Subsidence of the Coast of New Jersey, and of some of the adjoining States. By Geo. H. Cook.

46. On a Species of Trilobite from the Potsdam Sandstone of New York. By J. D. Dana.

47. On the Flexures of the Strata in the Broadtop Coal Field, Pennsylvania. By J. P. Leslie.

48. On the Parthenogenesis of Animals and Plants. By Berthold Seemann.

49. Remarks on the Classification of the Varieties of Man. By M. B. Anderson.

50. Remarks on the Collection of Indian Paintings and Antiquities exhibited by Mr. Paul Kane, at the Request of the Local Committee. By Daniel Wilson.

51. Observations on the Manners and Customs of the Indians of the North-west; illustrated by Paintings executed by the Author during his Travels. By Paul Kane.

52. On some Ethnological Specimens from the Island of Aneiteum, New Hebrides. By J. W. Dawson.

53. On certain Peculiarities of the Languages of Southern Africa. By J. Constantine Adamson.

54. On some Debatable Points in Natural Science. By E. J. Chapman.

EXECUTIVE PROCEEDINGS

OF THE

MONTREAL MEETING, 1857.

HISTORY OF THE MEETING.

The Eleventh Meeting of the American Association for the Advancement of Science was held at Montreal, Canada East, commencing on Wednesday, August 12, and continuing to Thursday noon, August 20.

The number of names registered in the book of members in attendance on this meeting is three hundred and fifty-one. One hundred and fifty-nine new members were chosen, of whom all but forty-one have already accepted their appointment by paying the assessment, and eighty-six of them signing the constitution. One hundred and seventeen others joined the Association by virtue of Rule 2 or 3, all of them signing the constitution, and paying the assessment. Nineteen others paid, without a formal election, or signing the constitution; and sixteen signed the constitution without a formal election, and without paying. One hundred and thirty-two papers were presented, of which one hundred and ten were read; but only a part have been printed. Some were thought unworthy of publication, and, in other cases, copies have not been furnished by the authors.

The sessions of the Association were held in the Court House.

The late President, Professor James Hall, of Albany, introduced to the Association the Vice-President for the meeting, Professor Alexis Caswell, of Providence. The Vice-President announced that the meeting would be opened by a prayer from the Lord Bishop of Montreal, Rev. Francis Fulford. The following prayer was then offered:—

"O Lord our God, who by thy Almighty word hast created the world, and now upholdest and governest all things in heaven and earth, receive, we beseech thee, these our humble prayers, which we offer to thy divine Majesty; and as thou didst at the first teach the hearts of thy faithful people by sending to them the light of thy Holy Spirit, and hast graciously promised to continue his presence with them that ask for it, grant us by the same spirit to have a right judgment in all things; and specially direct and guide the deliberations and actions of this Association, in connection with which we are assembled here this day. May all our works be begun, continued, and ended in thee; and while in the eager pursuit of knowledge and the deep investigations of science, may we be led to exclaim that 'The heavens declare the *glory* of God,' let us never rest satisfied, O Lord God Almighty, unless from thy Word, which thou hast given us, we can also learn *thy will*, and grow in grace, as well as knowledge, and in meetness for enjoying thy presence afterwards, when we may hope to see thee face to face, and know even as we are known. Let neither the splendor of any thing that is great, nor the conceit of any thing that is good in us, withdraw our eyes from looking upon ourselves as sinful dust and ashes. And thus clothed with humility, may we be meek, charitable, and courteous towards each other, so that no evil spirit of anger, jealousy, or vainglory, may find place in the midst of us to hinder or disturb our unity and peace.

"These petitions we offer up in the all-prevailing name of Jesus Christ, our only Mediator and Redeemer, with whose most perfect form of words we conclude our imperfect address to the Throne of Grace.

"'Our Father which art in heaven, hallowed be thy name, thy kingdom come, thy will be done on earth as it is in heaven: give us this day our daily bread, and forgive us our trespasses, as we forgive them that trespass against us; and lead us not into temptation, but deliver us from evil; for thine is the kingdom, and the power, and the glory, for ever and ever. Amen.'"

Then the Vice-President made the following address:—

GENTLEMEN OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE:—I congratulate you upon so large an attendance of members at the opening of this your Eleventh Annual Meeting. It augurs well for the interests of science, that so many of her votaries from distant portions of the country have come up to this gathering to lay their choicest contributions upon her altar, and welcome to their fellowship the humblest laborer in her cause.

I think it is also a matter of special interest that we are convened without the limits of the United States. However it may have been in former times, it certainly is not now the case, that mountains or seas interposed make enemies of nations. It is one of the felicities of our times, that, in the onward march of science, little account is taken of the boundaries which separate states and kingdoms. The discoverer of a new law in nature, the inventor of a new process in the arts, or a new instrument of research, is speedily heralded over land and ocean, is put in communication with the whole civilized world, and is everywhere hailed as a benefactor.

We have before us an illustration of the amenities of science. We of the United States are here assembled on British soil, little thinking that we have passed beyond the protection of American law,—little thinking, amid the generous hospitalities of this enterprising commercial capital of a noble province of the British Empire, that we are alien to the British Constitution. The American Eagle has left us, but I assure the gentlemen who have invited us here, that we feel in no danger of being harmed by the British Lion.

I have said that we are alien to the English Constitution. This, of course, is true only in the narrowest and most technical sense. I am proud to say, as I do upon deliberate conviction, that nothing is alien to the English Constitution which looks to the advancement of knowledge, the perfection of art, or the amelioration of the condition of humanity. I may further say,—and I here speak by permission of his Excellency, the Director of the Government,* who honors us by presiding at this meeting,—the proudest achievements of British arms have less enduring glory than that patronage of science, and those noble efforts for the advancement of Christian civilization, which, no less than her commercial greatness, place old England in the van of nations. And never, I may add, has that patronage been more efficient, or those efforts more praiseworthy, than under the reign of the present illustrious Queen, whose virtues are alike the ornament of her sex and her crown.

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* General Sir William Eyre.

There is seemingly a special fitness in our being here at this time. England and America are at this moment preparing to shake hands across the broad bosom of the Atlantic. The electric chain which is to bind them in perpetual friendship has been fabricated, and is now being deposited along the deep bed of the ocean, where no surface storms can disturb its repose or impair its energy. Thus United England and America may challenge the world, not, indeed, to the conflict of arms, but to the beneficent rivalry of extending the domain of science, of unfolding the wonderful mechanism of the heavens and the earth, and of rendering the powers of nature subservient to the uses of man.

But our congratulations on this occasion are not unmingled with sadness. It is a melancholy event which calls me to preside over your deliberations. The distinguished gentleman whom, at your last meeting, you elected to the presidency of this Association, is no longer numbered with the living.

Jacob Whitman Bailey, Professor of Chemistry, Mineralogy, and Geology in the U. S. Military Academy at West Point, died, after a protracted illness, on the 26th of February last, in the 46th year of his age. It is to be hoped that before the close of this meeting a fitting tribute will be paid to his memory, and to his eminent service in scientific research.

As a resident in the city where his youth was spent, I will simply say, that he gave early indications of that love of retired study and investigation which became so conspicuous in his maturer years. Many of his youthful companions remember the importance which he was accustomed to attach to every new variety of flower or pebble or shell which came in his way. At the age of seventeen he received an appointment as a cadet in the Military Academy at West Point. Though but a youth, and slender in health, he accomplished the severe course of academic studies with entire success. He held the honorable rank of the fifth in a class graduating forty-five members. He left the institution in 1832. In 1834 he was called to give instruction in the department of Chemistry, Mineralogy, and Geology. In 1838 he received the full appointment of Professor, and was the first to fill that chair. For more than eighteen years, says the official order announcing his death, he discharged the duties of that post with signal ability and success. In addition to the branches of science formally embraced in his professorship, he was well versed in Natural History, and specially in Botany. But it was doubtless in his extended microscopical researches that he has most contributed to the stock of human knowledge, and laid the foundation for the most enduring fame.

Amid the endearments of family and home, which ever awakened the finest sensibilities of his nature, he was called to drink deeply of the cup of affliction. His wife and daughter perished by the burning of a steamboat on the Hudson River on the 28th of July, 1852. He is believed never fully to have recovered from the shock of that heart-rending event.

Mine would not be the task, nor is this the place, to enter upon any analysis of his scientific labors. It is sufficient to say, that he established a reputation that does honor to American science, and places him among the most distinguished microscopists of the age.

In discharging the duties, gentlemen, of your presiding officer, I must bespeak your indulgence and your friendly coöperation. The courtesies which you are accustomed to show to each other will compensate for any inexperience on my part, or any want of knowledge in parliamentary forms and usages. We are met together with no narrow and selfish aims. Our object is to advance, so far as we may, the great cause of science, which embraces within its scope the great interests of humanity. In this noble enterprise, let us tender to each other every aid and every encouragement in our power.

At the conclusion of the President's address, Sir William E. Logan, in behalf of the Local Committee and the citizens of Montreal, welcomed the members of the Association in a few words: —

MR. PRESIDENT AND GENTLEMEN OF THE ASSOCIATION: — The welcome which I could give could only be a welcome to Montreal; but His Excellency, the Head of the Government, recognizing the importance of this meeting, and the interest attached to it, has kindly undertaken to give you a welcome, which will not be merely on the part of the citizens of Montreal, but on the part of the whole of Canada; and not merely of the whole of Canada, but on that of all the British North American possessions. I shall therefore leave it to His Excellency to give you that welcome.

His Excellency, the Administrator of the Government, General Sir William Eyre, K. C. B., next rose and said:—

LADIES AND GENTLEMEN:—It is not my desire or intention to detain you more than a few moments, as I have no doubt you will be anxious to commence the business of the meeting. But I wish to express, on behalf of Canada and myself, a warm and cordial welcome to all those who have honored us with their presence upon this occasion. It is a gratification to see so many of the citizens of the United States amongst us. Time has been when so large an influx from the other side of the frontier might have awakened other feelings, not, possibly, unmixed with some degree of apprehension. But, thank God, these feelings have passed away, and we can now unite in one common purpose, whether for the advancement of commerce or the promotion of science, feeling no other rivalship than that of a generous emulation. We have, many of us, enjoyed the hospitality of our neighbors on the other side. It is not for us to praise ourselves, but I trust that that virtue is not deficient on this side of the national frontier. Thanks to the benefactors of mankind, and the cultivators and promoters of science, we have ample means of conveying our thoughts and persons without much difficulty; and when that stupendous structure is completed which is striding across the river, and bids fair soon to afford the means of connection with both sides, I trust our communication will then be complete. I cannot conclude without expressing my regret at the absence of Professor Bailey, who had established for himself a European reputation, and whose presidency, upon this occasion, so many looked forward to with great pleasure. While we bow to that dispensation which has deprived us of his services upon this interesting occasion, I think we have, in Professor Caswell, a very worthy successor.

The President said he had much pleasure in introducing to the Association Professor Ramsay, who was here as the delegate from the Geological Society of Great Britain.

Professor Ramsay said it gave him great pleasure to find himself on this side of the Atlantic among his scientific friends, whether geologists, or those who devote their attention to other branches of science. He said scientific friends, because he had the honor and the pleasure of numbering many of those gentlemen among his friends. He had found, wherever he had gone, that there was a kind of brotherhood among scientific men, — that, go where you will, and merely announce your name, you are received with that affection, which is one of the greatest pleasures in scientific intercourse. He regretted that the gentleman first deputed by the Geological Society to attend this meeting, found he was not able to come. Sir Roderick Murchison was deputed, but his increasing years made him consider that it was advisable that he should not come, and the choice of the society then fell upon himself. He had no idea of coming, having made arrangements to go to the top of Mont Blanc, where he expected to derive the greatest possible pleasure; but being appointed to represent the society at this Association, he gave up that excursion, as he anticipated ten times more pleasure and ten times more instruction by coming to this meeting of the American Association.

The President said he had the pleasure of introducing Dr. Berthold Seemann as the delegate of the Linnæan Society of Britain.

Dr. Seemann expressed the great pleasure he had in being present, and in declaring the high esteem which the society he represented had always entertained for the American Association. The Linnæan Society had always taken a deep interest in this Association, and he had no doubt that this meeting would add another chapter to the proud history of American science. He begged to offer them the most sincere congratulations of the society he had the honor to represent.

The President said that Professor J. W. Dawson would now address them on behalf of the Natural History Society of Montreal.

Mr. Dawson said he did not intend to trouble them with saying any thing upon the present occasion; but, as his name had been placed upon the programme, he would take the opportunity to offer the Association a most hearty welcome to Montreal. He would not attempt to make any speech at present, but he hoped to have, on to-morrow evening, an opportunity of saying a few words, as it was proposed to invite all the members of the Association to an entertainment, which will enable them to have some social interchange of sentiment with one another. In behalf of the Natural History Society of Montreal, he would say that they most heartily rejoiced to see so large a representation of American science as that now present; and they hoped that this meeting would be very successful, — one which will give a strong and healthy stimulus to the pursuit of Natural History in this city and in Canada. If so, the Natural History Society will regard this as the greatest service which they have done to the cause of American science.

The Annual Address was delivered by the retiring President, Professor James Hall, on Tuesday afternoon, August 18.

On the afternoon of August 14, Dr. Hare delivered, in general session, his paper on "Travelling Whirlwinds."

On the evening of Friday, August 14, Dr. A. A. Gould, of Boston, offered a tribute to the memory and services of the late Professor J. W. Bailey, elected President of the Association for the Montreal Meeting. Professor D. Olmstead, of New Haven, followed in a similar tribute to the late William C. Redfield, the first President of the Association. These addresses are printed in this volume.

No lengthened abstract of the proceedings, scientific and executive, of the Montreal Meeting of the Association is necessary in this place, as they are contained in full in the papers and resolutions printed in this volume.

The Association voted to hold their next meeting at Baltimore, Maryland, on Wednesday, the 28th of April, 1858, having received an invitation to visit that city from the Maryland Historical Society and the Maryland Institute of the city of Baltimore.

The officers elected for the next meeting, are Professor JEFFRIES WYMAN, of Cambridge, President; Professor JOHN E. HOLBROOK, of Charleston, S. C., Vice-President; Professor WILLIAM CHAUVENET, of Annapolis, Md., General Secretary; and Dr. A. L. ELWYN, of Philadelphia, Treasurer. The Permanent Secretary, Professor JOSEPH LOVERING, of Cambridge, was elected, at Albany, for a second term.

During the session of the Association, the members and their ladies, besides receiving many private hospitalities, were elegantly entertained on Thursday evening, August 13, by the Natural History Society of Montreal; on Monday evening, August 17, by the Governors, Principal, and Fellows of McGill College; and on Thursday, August 20, by the Corporation of the City of Montreal. On Saturday afternoon, August 15, an excursion was made to St. Helen's Island, by invitation of Colonel Monro of the 39th Regiment; and on Thursday, August 20, an excursion was made, by invitation of the Local Committee, to St. Anne's and Beauharnois, in which an opportunity was afforded to examine the Potsdam limestone in that vicinity, and pass down the Lachine Rapids.

RESOLUTIONS ADOPTED.

Resolved, That a Sub-Committee of three be appointed to report to the Standing Committee such Rules and Regulations, of a permanent character, as may seem capable of promoting the regular despatch of business at future meetings of the Association.

Resolved, That a Preliminary Committee be appointed to inquire and ascertain what has been done upon the subject, in the several States, of the Registration of Births, Deaths, and Marriages, and report to the Standing Committee at their next meeting.

Resolved, That a Committee be appointed to wait upon the retiring President of the Association, and invite him to deliver his address at some proper time and place, and under more favorable circumstances than those which existed at the social meeting of August 13.

Resolved, That the retiring President be requested to notice in his address the services of members active in the advancement of science, who have died within the year.

Resolved, That the printing of the programmes for the remainder of this Meeting be placed under the superintendence of the Permanent Secretary, and that the Standing Committee of the Sections be called upon to deliver the programmes of their respective Sections and Subsections to the Permanent Secretary, the chairmen of the Sub-sections providing them with the means for this purpose.

Resolved, That a Committee be appointed to report on Dr. Hare's Storm-Curves.

Resolved, That two hundred and fifty extra copies of the Address of Professor Hall be printed, and presented to the author.

Resolved, That one hundred dollars be appropriated, according to the recommendation of the Permanent Secretary, to provide suitable accommodations for the property of the Association, amounting to nearly four thousand volumes.

Resolved, That no notice be taken, by title or otherwise, in the published Proceedings, of any article not approved by the Standing Committee.

Resolved, In accordance with the recommendation of the Physical Section, that a Committee be appointed on the Coast Survey (according to the request of the Superintendent), and that the books presented by the Superintendent be referred to this Committee.

VOTES OF THANKS.

Resolved, That the Association highly appreciate, and warmly recip rocate, the friendly sentiments expressed, at the opening of the session, by HIS EXCELLENCY, GENERAL SIR WILLIAM EYRE, K. C. B., Administrator of the Government.

Resolved, That the thanks of the Association be tendered to the CORPORATION AND CITIZENS OF MONTREAL, for their generous hospitality.

Resolved, That the thanks of the Association be returned to the LOCAL COMMITTEE, for their convenient arrangements for its accommodation, and for their unwearied exertions to promote the personal comfort of the members.

Resolved, That the Association tenders its thanks to the NATURAL HISTORY SOCIETY, for the elegant reception and entertainment given to the Association at Concert Hall.

Resolved, That the thanks of the Association be returned to the GOVERNORS, PRINCIPAL, AND FELLOWS OF MCGILL COLLEGE for the cordial reception and fraternal welcome extended to its members at Burnside Hall.

Resolved, That we recognize, with peculiar satisfaction, the presence of the eminent SCIENTIFIC GENTLEMEN FROM THE OLD WORLD, who have honored this meeting with their attendance.

Resolved, That the Association returns its grateful acknowledgments to the CORPORATION OF THE CITY OF NEW YORK, and to the ACADEMY OF SCIENCES OF THE CITY OF ST. LOUIS, for their polite invitations to hold our next meeting in their respective cities.

Also, to the JUDGES of the DISTRICT OF MONTREAL, and to the BAR, for the accommodation afforded to the Association in the use of the Court House.

Also, to the CANADIAN INSTITUTE, and to the MECHANICS' INSTITUTE, for civilities extended to the Association by these bodies respectively.

Resolved, That the thanks of the Association be tendered to the DIRECTORS of those Railroad and Steamboat Lines, on which free return tickets have been offered to members; and also to the DIRECTORS of those Lines of Ocean Steamers who have placed free passages from Europe at the disposal of the Local Committee.

Resolved, That the thanks of the Association are due to the MERCANTILE LIBRARY ASSOCIATION of Montreal, for affording the members the free use of their Reading Room, and tendering them complimentary tickets for the Exhibition of Paintings.

Resolved, That our cordial thanks be returned to Professor ALEXIS CASWELL, for the courteous, dignified, and efficient manner in which he has presided over the Association at its present meeting.

Resolved, That the thanks of the Association be presented to the PRESIDENT AND DIRECTORS OF THE BANK OF MONTREAL, for their liberality in negotiating the funds of the Association.

REPORT OF THE PERMANENT SECRETARY.

THE Permanent Secretary submits his annual report to the Standing Committee as follows: —

The affairs of the Association under his charge since the Albany Meeting relate chiefly to the collection of assessments; to the making of contracts, and the payment of bills, involved in the publications of the Association; to the examination of papers presented at the Albany Meeting, the correction of proof-sheets, and their transmission to the authors of the papers; to the distribution of the Albany volume to members entitled to receive copies; to assisting in arrangements at the Albany Meeting, and in preparations for the Montreal Meeting; and, finally, to issuing to members of the Association the following classes of circulars: 1st, calling on members for a copy of their papers, read at Albany; 2d, notifying new members of their election; 3d, calling upon delinquents for their arrears; 4th, transmitting receipts for assessments received by mail; 5th, notifying members when the printed volume was ready; 6th, notifying members *when* and *how* this volume had been sent to them. To these duties may be added the general correspondence during the interval between the tenth and eleventh meetings of the Association. The number of members in the Association is now so large, and the published volume so much exceeds in size any previous one, that the duties of the Permanent Secretary have required much of his time and attention. And yet these duties are of such a nature, and the members of the Association hang together by so loose a tenure, that he can hardly suppose that, with all his pains, he has discharged them to the satisfaction of every one.

It cannot be expected of the Treasurer of the Association, who receives no compensation, that he should do more than hold in trust the unappropriated funds of the Association. It is proper, on this account, as it is far the most convenient arrangement, that the assessments should be paid to the Permanent Secretary, as the Constitution allows, and as is, in fact, the general practice. Since the 20th of August, 1856, when the Albany Meeting began, down to the 12th of August, 1857, when the Montreal Meeting commenced (including exactly a financial year), $2,363.34 have been collected by the Permanent Secretary, partly by the sale of the volumes of Proceedings, but principally in payment of the last annual assessment and of arrears.

This sum exceeds, by $226, that collected in the same way during the preceding year, although the latter was considered financially highly prosperous to the Association. To this sum must be added $10, received from A. D. Bache, in repayment of Peters's plate "On the Solar Spots," $19 received from G. P. Putnam & Co. for the sale of Proceedings, $30 drawn from the Treasurer, Dr. A. L. Elwyn, and a balance of $106.31 remaining, from the previous account, in the hands of the Secretary, the whole constituting a fund of $2,528.65. From this income have been expended $2,417.30, the items of which expenditure will be found particularly given in the account-book which accompanies this report, but which may be generally stated as follows:—

1. Expenses in the publication and distribution of the Albany volume,	$1,723.62
2. Salary of the Permanent Secretary,	500.00
3. General expenses, including postage, and charges incurred at the Albany Meeting, etc.,	193.68

There remains, on this account, in the hands of the Permanent Secretary, a balance of $111.35, which has been carried in the account-book to the credit of the Association. The balance in the hands of the Treasurer has also increased by $30; so that it now amounts to $821.70. If this sum is added to the balance in the hands of the Secretary, it makes a total of $933.05 to the credit of the Association.

The Permanent Secretary would state, in conclusion, that the property of the Association in his hands consists of 3,898 volumes of Proceedings. These volumes are stored away in boxes, and a recent examination has shown that they are suffering for want of air, and other more appropriate accommodations. He recommends an appropriation of $100, in order to place them on suitable shelves in a dry place. This plan will not involve any annual charge, and the expense of it will not exceed, probably, the sum received from the sales in a single year.

JOSEPH LOVERING,
Permanent Secretary.

MONTREAL, August 17, 1857.

CASH ACCOUNT OF THE

Dr. AMERICAN ASSOCIATION *in*

Stationery for two years	$20.00
Munsell, for printing envelopes	9.50
Express to Albany	3.00
Horton, for services at Albany	16.00
Expenses in attending meeting	75.00
Bad bank-note	2.00
Rice & Kendall, for paper	299.52
Buck's Express	10.62
Stratton's Express	2.62
Kilbourn & Mallory, for wood-cuts	103.00
J. Bien, for printing 7 plates	112.50
Rice & Kendall, for paper for covers	6.90
Rice & Kendall, for paper	88.92
Allen & Farnham, for printing	356.81
Metcalf & Co., for printing	356.21
Gorham's bill for printing plate	15.25
Bradford, for printing plate	43.00
Mills, for services	15.00
Rice & Kendall, for paper	132.00
Higgins & Bradley, for binding	101.79
Buck's express	11.75
Index to Albany volume	5.00
Circulars to delinquents	15.00
Redfield, for plates	48.84
Express to Montreal	10.89
Postage and discount	56.18
Salary of Permanent Secretary	500.00
	$2,417.30
Balance to next account	111.35
	$2,528.65

PERMANENT SECRETARY.

Account with JOSEPH LOVERING.	Cr.
Balance from last account	$106.31
Draft on Treasurer	30.00
Cash from G. P. Putnam & Co., for sale of Proceedings	19.00
From A. D. Bache, for Dr. Peters's plate	10.00
Assessments, &c. (from No. 357 to 950)	2,363.34
	$2,528.65

STOCK ACCOUNT OF THE PERMANENT SECRETARY.

Volumes Distributed or Sold.

VOLUMES	I.	II.	III.	IV.	V.	VI.	VII.	VIII.	IX.	X.
DELIVERED TO										
Boston City Library *					*	*	*	*	*	
Boston City Library										*
Patent Office *							*	*	*	*
Drowne *	*	*	*	*	*	*				
Drowne							*	*	*	
J. B. Smith *	*	*	*	*	*	*				
J. Munsell *	*	*	*	*	*					
Nantucket Athenæum †						*	*	*	*	*
Munroe & Co.‡										20
Westermann *		*								*
J. C. Teeley *										*
Dudley Observatory										*
Albany State Library										*
Phil. Acad. Nat. Science †										*
Phil. Philos. Soc. †										*
Providence Athenæum †										*
Brown University										*
Yale College										*
Harvard College										*
American Academy †										*
Boston Nat. Hist. Society										*
Boston Athenæum										*
Michigan University *										*
Super't Education, Montreal										*
Smithsonian Institution †										*
C. M. Blake							*	*	*	*
W. H. Davies							*	*	*	*
J. P. Doyle *										*
Wurtelle *										*
W. M. Roberts							*	*	*	
Local Committee at Montreal										*
C. M. Morse									*	*
W. D. Henkle *										*
A. K. Eaton										*
Dr. A. S. Baldwin										*
W. Baylis *										*
C. P. Treadwell										*
E. J. Horan										*
W. F. Phelps										*
W. C. Little										*
To Members	1				1		21	17	28	431
Total	4	4	3	3	5	4	28	24	36	484

*** Sold.**
† By order of the Association.
‡ In exchange for twenty copies of Vol. II.

BALANCE OF STOCK.

Volumes.	I.	II.	III.	IV.	V.	VI.	VII.	VIII.	IX.	X.
Balance, March 20, 1857,	65	67	265	244	454	312	581	895	1015	
Received from Binders, .										1487
Received from Monroe & Co.,*		20								
Total, ...	65	87	265	244	454	312	581	895	1015	1487
Delivered to Members or sold, ...	4	4	3	3	5	4	28	24	36	484
Balance, March, 1858, .	61	83	262	241	449	308	553	871	979	1003

* In exchange for volumes less rare.

ACCOUNT OF G. P. PUTNAM & CO. WITH THE ASSOCIATION.

Volumes on hand.	I.	II.	III.	IV.	V.	VI.	VII.	VIII.	IX.	Value.
January 1, 1857,	4	4	4	69	7	152	8	8	8	$395.84
January 1, 1858,	0	2	0	67	2	148	1	4	3	$352.95
Now due, for sold,	4	2	4	2	5	4	7	4	5	$ 42.89

March 1, 1858.

REPORT OF THE AUDITORS.

We hereby certify that we have examined the preceding account of the Permanent Secretary, comparing the credits with the Treasurer's account, and the receipt book and the debits with the several vouchers, and find the whole correct, and the balance of one hundred and eleven dollars and thirty-five cents properly credited in the next account.

Signed, DENISON OLMSTED, } *Auditors.*
B. A. GOULD, JR., }

REPORT OF THE TREASURER.

I HAVE received from the Permanent Secretary a check on the Phœnix Bank of New York, for thirty dollars, and on the Charles River Bank a check for thirty dollars; from members in assessments, sixty dollars; in all, one hundred and twenty dollars. I have paid from the treasury Duval's bill (for lithographing), sixty dollars; a draft by Joseph Lovering, thirty dollars; in all, ninety dollars. The excess of receipts above expenditures (amounting to thirty dollars), added to balance of last year, which was seven hundred and ninety-one dollars and seventy cents ($791.70), makes the balance to next account eight hundred and twenty-one dollars and seventy cents ($821.70).

A. L. ELWYN, *Treasurer.*

CORRESPONDENCE.

WEST POINT, January 27, 1857.

DEAR SIR,—I have the honor to acknowledge with thanks the reception of your letter, informing me officially of my election to the Presidency of the American Association for the Albany Meeting.

As there is no prospect that my health will permit me to attend the meeting, I shall, in due time, send you a letter of thanks to the Society for the honor of the election, and regret that I cannot perform the duties of the office. I shall also give Prof. Caswell due notice.

Yours very respectfully,
J. W. BAILEY, *Prof. of Chemistry.*

PROF. LOVERING, Cambridge, Mass.

BALTIMORE, February 17, 1857.

DEAR SIR,—At a meeting of the Maryland Historical Society, held in the month of January, a Resolution was passed inviting the American Association for the Advancement of Science to hold its meeting in 1858 in the city of Baltimore, and appointing a committee consisting of Lewis H. Steiner, Rev. J. G. Morris, D. D., and Rev. G. W. Burnap, D. D., to present the invitation at the Montreal Meeting.

I have deemed it proper thus early to let you know officially of this invitation,—and as this Society represents our best citizens, it insures a hearty welcome.

With sentiments of highest respect, yours, etc.,
LEWIS H. STEINER, *Chairman of Committee.*

Prof. JOS. LOVERING, *Per. Sec'y Amer. Association.*

MARYLAND HISTORICAL SOCIETY'S ROOMS, Baltimore, January 8, 1857.

At a regular meeting of this Society, held on this evening, the following resolution, offered by Dr. Lewis H. Steiner, was unanimously adopted: "*Resolved,* That an invitation be extended by the Maryland Historical Society to the American Association for the Advancement of Science, at its meeting at Montreal, on the 12th of August next, to hold the meeting of the said Association in 1858 in the city of Baltimore, — and that the President be empowered to appoint a committee to present the invitation at the Montreal Meeting, and to express the earnest hope that it may be accepted."

Upon the adoption of the resolution, Dr. Lewis H. Steiner, Rev. Dr. J. G. Morris, and Rev. Dr. George W. Burnap were appointed to serve as the Committee.

J. SPEAR SMITH, *President.*
F. STREETER, *Secretary M. H. S.*

BALTIMORE, July 31, 1857.

GENTLEMEN, — The Maryland Institute of this city, established for the promotion of certain branches of science, whose universal cultivation and advancement form the object of your Association, have conferred upon the undersigned the honor of appointing them a committee to request that you will hold your next meeting in Baltimore.

The members of the Institute, with unanimity, displayed great solicitude that this request should be favorably entertained; but they have left to us, as more becoming, to present, in our discretion, the motives that are to be considered and may be most efficacious in influencing the acceptance of the invitation.

Among such motives, then, appearing to us entitled to great weight, are the centrality and accessibility of this city, more than of any other place where your meetings have been, or can be, held; the convenience of transportation of members of your Association to and from the meeting, which, through the agency of the Baltimore and Ohio Railroad, will be made less troublesome and more extensive than has been the case hitherto; the adaptation of the various apartments of the Institute, unrivalled, we believe, in America, either for the inaugural and valedictory assemblages of all your members, as well as of a numerous auditory which it is reasonable to suppose will be stimulated and improved by witnessing your proceedings, or for the separate meetings of different sections of your Association for the working prosecution of their respective subjects.

These motives have more especially a corporate regard. As to what concerns the Association more particularly in regard to its individual composition, we believe we say enough in adding that Baltimore has, in more than one instance, shown her appreciation of the presence of distinguished and philanthropic persons, and that in no instance has she been considered behindhand in the exhibition of a genial hospitality.

As to the period of holding your meeting, thus invited, the Institute appears to have thought it more respectful, instead of fixing any time, to leave it to you to determine the season and date most convenient to yourselves.

We sincerely hope, Gentlemen, that the suggestions we have had the honor of making will be acceptable to you; and that you will, before your adjournment, authorize us to report to our Principals the agreeable intelligence of your willingness to accept their invitation, and also of the precise date, against which preparations are to be made for your suitable reception.

And we have the honor to remain, Gentlemen,

CAMPBELL MORFIT.
J. H. ALEXANDER.
A. D. BACHE.

To the American Association for the Advancement of Science, now at Montreal, Canada East.

MAYOR'S OFFICE, New York, August 11, 1857.

DEAR SIR, — In behalf of the Corporation of this city, I beg leave, through you, to invite the American Association for the Advancement of Science, to hold their next Annual Meeting in this city.

It will give us pleasure to meet so distinguished a body of savans.

Very truly,
FERNANDO WOOD, *Mayor.*

Dr. L. FEUCHTWANGER.

The Academy of Science at St. Louis herewith respectfully tender to the American Association for the Advancement of Science a cordial invitation to hold its next Annual Meeting in the city of St. Louis.

As President of the Academy, permit me to assure the Association of an earnest and hearty welcome, should it consent to honor St. Louis with its presence.

With much respect, etc.,
B. F. SHUMARD, *Pres't Acad. Sci. St. Louis.*

To the American Association for the Advancement of Science.

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ERRATA.

PART I.

Page 67, line 4 from bottom, *for* Q' *read* Q.
" 69, in Formula, *for* A *read* a.
" 75, line 4, *for* any *read* any other.
" 75, " 17, *for* less *read* greater.
" 77, " 10, *for* invariable law *read* law.
" 78, " 8, *for* v *read* v^2.
" 79, " 11, *for* $\frac{D_{x-n}}{D'_x}$ *read* $\frac{D_{x-n}}{D_x}$.
" 81, " 4, *omit* or S'_x
" 82, " 4, " x.

PART II.

Page 27, line 4 from bottom, *for* feet *read* furlongs.
" 31, " 5 from top, *for* Hupan *read* Huppan.
" " " 8 from bottom, for feruginious *read* ferruginous.
" " " 11 from bottom, *for* woolly *read* woody.
" 36, " 10 from top, *for* linneus *read* limneus.
" 44, " 8 from bottom, *for* leave *read* leaves.
" 45, bottom line, *for* pale-red *read* pale sea.
" 65, line 18, *for* Temiscaming *read* Temiscamang.
" 46, " 22, *after* area *insert* of azoic rocks.
" 46, " 7, *for* Shibahahnahning *read* Shebahahnahning.
" 152, " 4, *for* where *read* when.

ERRATA

In the Volume of the Proceedings for 1856, Part I.

Page 85, line 5, *for* C *read* D.
" 87, " 23, *after* $\Sigma_x^{x+n} S'_x$ *supply* $-\frac{S'_x}{2}+\frac{L_x v^x}{2}$.

College Library
21 March

PROCEEDINGS

OF

THE AMERICAN ASSOCIATION

FOR THE

ADVANCEMENT OF SCIENCE.

TWELFTH MEETING,

HELD AT BALTIMORE, MARYLAND.
MAY, 1858.

CAMBRIDGE:
PUBLISHED BY JOSEPH LOVERING.
1859.

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EDITED BY
JOSEPH LOVERING,
Permanent Secretary.

CAMBRIDGE:
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OFFICERS OF THE ASSOCIATION

AT THE

BALTIMORE MEETING.

Prof. Jeffries Wyman,* *President.*
Dr. John E. Holbrook, *Vice-President.*
Prof. Joseph Lovering, *Permanent Secretary.*
Prof. W. M. Gillespie, *General Secretary.*
Dr. A. L. Elwin, *Treasurer.*

Standing Committee.

EX OFFICIO.

Prof. Jeffries Wyman,
Prof. John E. Holbrook,
Prof. Alexis Caswell,
Prof. Joseph Lovering,
Prof. W. M. Gillespie,
Dr. A. L. Elwyn,
Prof. John Leconte.

---

* In consequence of the non-attendance of the President and Vice-President, Professor Alexis Caswell was requested to act as President of the Baltimore Meeting.

AS CHAIRMEN OF THE SECTIONAL COMMITTEES

Prof. JOSEPH HENRY,
G. C. SWALLOW, Esq.

FROM THE ASSOCIATION AT LARGE.

Prof. J. D. DANA,
Prof. WOLCOTT GIBBS,
Prof. INCREASE A. LAPHAM,
Dr. JOHN L. LE CONTE,
Prof. STEPHEN ALEXANDER,
Rev. Dr. P. N. LYNCH.

Local Committee.

Hon. THOMAS SWANN, *Chairman.*
Dr. LEWIS H. STEINER, *Secretary.*

Gen. JOHN SPEAR SMITH,
Hon. JOSHUA VANSANT,
AUGUSTUS J. ALBERT, Esq.,
Dr. R. S. STEUART,
Rev. J. G. MORRIS, D. D.,
JOHN F. MEREDITH, Esq.,
Rev. G. W. BURNAP, D. D.,
WM. H. YOUNG, Esq.,
CHARLES F. MAYER, Esq.,
Hon. W. F. GILES,
Dr. JAMES BORDLEY,
GEORGE A. WARDER, Esq.,
J. D. PRATT, Esq.,
L. F. BARRY, Esq.,
P. G. VAN WINKLE, Esq.,
GEORGE W. BROWN, Esq.,
S. F. STREETER, Esq.,
Hon. S. O. HOFFMAN,
JONATHAN MEREDITH, Esq.,
Rev. OTIS H. TIFFANY,
Dr. CHRISTOPHER JOHNSTON,
LAWRENCE SANGSTON, Esq.,
WOODWARD ABRAHAMS, Esq.,
D. L. BARTLETT, Esq.,
THOMAS J. HAND, Esq.,
J. CRAWFORD NEILSON, Esq.,
G. H. HUNT, Esq.,
SAMUEL SANDS, Esq.,
GEORGE F. R. WAESCHE, Esq.

SPECIAL COMMITTEES.

A. Committees continued from former Meetings.

1. *Committee to Report in Relation to Uniform Standards in Weights, Measures, and Coinage.*

Prof. A. D. Bache,
Prof. Joseph Henry,
Prof. Wolcott Gibbs,
Prof. Benjamin Peirce,
Prof. John Le Conte,
Prof. W. B. Rogers,
Prof. J. H. Alexander,
Prof. John F. Frazer,
Dr. J. H. Gibbon,
Dr. B. A. Gould, Jr.,
Prof. J. L. Smith,
Prof. R. S. McCulloch.

2. *On the Registration of Births, Deaths, and Marriages.*

Dr. James Wynne,
E. B. Elliott, Esq.
Dr. F. B. Hough.

3. *Committee on the Coast Survey.*

Pres. F. A. P. Barnard,
Gen. Joseph G. Totten,
Prof. Benjamin Peirce,
Prof. John Torrey,
Prof. Joseph Henry,
Prof. J. F. Frazer,
Prof. William Chauvenet,
Prof. John Le Conte,
Prof. W. M. Gillespie,
Prof. Francis M. Smith,
Prof. W. H. C. Bartlett,
Prof. Wolcott Gibbs,
Prof. Stephen Alexander,
Prof. Lewis R. Gibbes,
Prof. Joseph Winlock,
Prof. James Phillips,
Prof. William Ferrel,
Prof. Edward Hitchcock,
Prof. James D. Dana.

4. *Committee to Memorialize the Legislature of New York in Reference to Fish-Breeding.*

Prof. Louis Agassiz,
Prof. J. D. Dana.

B. New Committees.

1. *Committee to Audit the Accounts of the Permanent Secretary and the Treasurer.*

Rev. Dr. P. N. Lynch,
Dr. John L. Le Conte,

2. *Committee to act with the Standing Committee in Nomination of Officers for the next Meeting.*

Section A.

Section B.
Philip E. Tyson, Esq.,
Prof. Joseph Leidy,*
Dr. L. A. Smith,
George Opdyke, Esq.*

3. *Committee on the best Method of conducting a State Geological Survey.*

G. C. Swallow, Esq.
Prof. Edward Hitchcock,
Prof. James Hall,
Prof. H. D. Rogers,
Philip T. Tyson, Esq.,
Dr. R. W. Gibbes,
Dr. Charles T. Jackson,
Oscar M. Lieber.

4. *Committee on the Report of the Committee on the Registration of Births, Marriages, and Deaths.*

Prof. Joseph Henry,
Dr. R. W. Gibbes,
Prof. W. M. Gillespie,
Prof. W. B. Rogers,
Prof. Samuel Jackson.

---

* On account of the absence of these gentlemen, Dr. J. H. Gibbon and Prof. W. B. Rogers were afterwards chosen to fill their places.

5. *Committee to memorialize the States to establish Meteorological Observations.*

Prof. Joseph Henry,
Prof. James H. Coffin,
Dr. F. B. Hough,
Major R. Lachlan.

6. *Committee on Dr. I. I. Hayes's Proposed Expedition to the North Pole.*

Prof. A. D. Bache,
Prof. Joseph Henry,
Prof. W. B. Rogers,
Prof. Edward Hitchcock,
Prof. Benjamin Peirce,
Prof. J. D. Dana,
Prof. Joseph Winlock,
Hon. Thomas Ewing,
Hon. D. M. Barringer,
Dr. J. L. Le Conte,
Prof. J. E. Hilgard,
Peter Force,
Dr. Joseph Leidy,
Dr. John Torrey,
Prof. S. S. Haldeman.

7. *Committee on American Ethnology.*

Dr. Wills De Haas,
G. C. Swallow, Esq.,
Philip T. Tyson,
Dr. J. H. Gibbon,
Prof. S. S. Haldeman.

8. *Committee on the Abbreviation of the Name of the Association.*

Lieut. E. B. Hunt,
Dr. S. L. Loomis,
Prof. W. B. Rogers.

OFFICERS OF THE SPRINGFIELD MEETING.

Prof. Stephen Alexander, *President.*
Prof. Edward Hitchcock, *Vice-President.*
Prof. Joseph Lovering, *Permanent Secretary.*
Prof. William Chauvenet, *General Secretary.*
Dr. A. L. Elwyn, *Treasurer.*

Standing Committee.

Prof. Stephen Alexander,
Prof. Edward Hitchcock,
Prof. Joseph Lovering,
Prof. William Chauvenet,
Dr. A. L. Elwyn,
Prof. Jeffries Wyman,
Prof. John E. Holbrook,
Prof. Alexis Caswell,
Prof. W. M. Gillespie.

Local Committee.

Hon. George Bliss, *Chairman.*
George A. Otis, Jr., *Secretary.*

R. A. Chapman, Esq.,
Rev. Francis Tiffany,
Capt. James Barnes,
Gen. James Whitney,
Dr. William Tully,
Josiah Hooker, Esq.,
Ansel Phelps, Jr.,
Hon. C. C. Chaffee,
Chester W. Chapin, Esq.,
Col. J. M. Thompson,
George Walker, Esq.

MEETINGS OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

Meeting.	Date.	Place.	President.	Vice-President.	General Secretary.	Permanent Secret'y.	Treasurer.
1st,	Sept. 20, 1848,	Philadelphia, Pa.,	W. C. Redfield, Esq.,		Prof. Walter R. Johnson,		Prof. J. Wyman.
2d,	Aug. 14, 1849,	Cambridge, Mass.,	Prof. Joseph Henry,		Prof. E. N. Horsford,		Dr. A. L. Elwyn.
3d,	March 12, 1850,	Charleston, S. C.,	Prof. A. D. Bache,*		Prof. L. R. Gibbes,*		Dr. St. J. Ravenel.*
4th,	Aug. 19, 1850,	New Haven, Ct.,	Prof. A. D. Bache,		E. C. Herrick, Esq.,		Dr. A. L. Elwyn.
5th,	May 5, 1851,	Cincinnati, Ohio,	Prof. A. D. Bache,		Prof. W. B. Rogers,	Prof. S. F. Baird,	Dr. A. L. Elwyn.
6th,	Aug. 19, 1851,	Albany, N. Y.,	Prof. L. Agassiz,		Prof. W. B. Rogers,	Prof. S. F. Baird,	Dr. A. L. Elwyn.
7th,	July 28, 1853,	Cleveland, Ohio,	Prof. Benj. Peirce,		Prof. J. D. Dana,	Prof. S. F. Baird,	Dr. A. L. Elwyn.
8th,	April 26, 1854,	Washington, D. C.,	Prof. J. D. Dana,		Prof. J. Lawrence Smith,	Prof. J. Lovering,	Dr. J. L. Le Conte.*
9th,	Aug. 15, 1855,	Providence, R. I.,	Prof. John Torrey,		Prof. Wolcott Gibbs,	Prof. J. Lovering,	Dr. A. L. Elwyn.
10th,	Aug. 20, 1856,	Albany, N. Y.,	Prof. James Hall,		Dr. B. A. Gould, Jr.,	Prof. J. Lovering,	Dr. A. L. Elwyn.
11th,	Aug. 12, 1857,	Montreal, C. E.,	Prof. J. W. Bailey,	Prof. Alexis Caswell,	Prof. John Leconte,	Prof. J. Lovering,	Dr. A. L. Elwyn.
12th,	April 28, 1858,	Baltimore, Md.,	Prof. A. Caswell,*	Prof. John E. Holbrook,	Prof. W. M. Gillespie,	Prof. J. Lovering,	Dr. A. L. Elwyn.

* In the absence of the regular officer.

CONSTITUTION OF THE ASSOCIATION.*

OBJECTS.

The Association shall be called "The American Association for the Advancement of Science." The objects of the Association are, by periodical and migratory meetings, to promote intercourse between those who are cultivating science in different parts of the United States; to give a stronger and more general impulse, and a more systematic direction, to scientific research in our country; and to procure for the labors of scientific men increased facilities and a wider usefulness.

MEMBERS.

Rule 1. Members of scientific societies or learned bodies having in view any of the objects of this Association, and publishing transactions, shall be considered members on subscribing these rules.

Rule 2. Collegiate professors, also civil engineers and architects who have been employed in the construction or superintendence of public works, may become members on subscribing these rules.

Rule 3. Persons not embraced in the above provisions may become members of the Association upon recommendation in writing by two members, nomination by the Standing Committee, and election by a majority of the members present.

---

* Adopted August 25, 1856, and ordered to go into effect at the opening of the Montreal Meeting.

OFFICERS.

RULE 4. The officers of the Association shall be a President, Vice-President, General Secretary, Permanent Secretary, and Treasurer. The President, Vice-President, General Secretary, and Treasurer, shall be elected at each meeting for the following one; — the three first-named officers not to be reëligible for the next two meetings, and the Treasurer to be reëligible as long as the Association may desire. The Permanent Secretary shall be elected at each second meeting, and also be reëligible as long as the Association may desire.

MEETINGS.

RULE 5. The Association shall meet, at such intervals as it may determine, for one week or longer — the time and place of each meeting being determined by a vote of the Association at the previous meeting; and the arrangements for it shall be intrusted to the officers and the Local Committee.

STANDING COMMITTEE.

RULE 6. There shall be a Standing Committee, to consist of the President, Vice-President, Secretaries, and Treasurer of the Association, the officers of the preceding year, the permanent chairman of the Sectional Committees, after these shall have been organized, and six members present from the Association at large who shall have attended any of the previous meetings, to be elected upon open nomination by ballot on the first assembling of the Association. A majority of the whole number of votes cast to elect. The General Secretary shall be Secretary of the Standing Committee.

The duties of the Standing Committee shall be, —

1. To assign papers to the respective sections.

2. To arrange the scientific business of the general meetings, to suggest topics and arrange the programmes for the evening meetings.

3. To suggest to the Association the place and time of the next meeting.

4. To examine, and, if necessary, to exclude papers.

5. To suggest to the Association subjects for scientific reports and researches.

6. To appoint the Local Committee.

7. To have the general direction of publications.

8. To manage any other general business of the Association during the session, and during the interval between it and the next meeting.

9. In conjunction with four from each Section, to be elected by the Sections for the purpose, to make nominations of officers of the Association for the following meeting.

10. To nominate persons for admission to membership.

11. Before adjourning, to decide which papers, discussions, or other proceedings shall be published.

SECTIONS.

Rule 7. The Association shall be divided into two Sections, and as many sub-Sections as may be necessary for the scientific business, the manner of division to be determined by the Standing Committee of the Association. The two Sections may meet as one.

SECTIONAL OFFICERS AND COMMITTEES.

Rule 8. On the first assembling of the Section, the members shall elect upon open nomination a permanent chairman and secretary, also three other members, to constitute, with these officers, a Sectional Committee.

The Section shall appoint, from day to day, a chairman to preside over its meetings.

Rule 9. It shall be the duty of the Sectional Committee of each Section to arrange and direct the proceedings in their Section; to ascertain what communications are offered; to assign the order in which these communications shall appear, and the amount of time which each shall occupy.

The Sectional Committees may likewise recommend subjects for systematic investigation by members willing to undertake the researches, and to present their results at the next meeting.

The Sectional Committees may likewise recommend reports on particular topics and departments of science, to be drawn up as occasion permits, by competent persons, and presented at subsequent meetings.

REPORTS OF PROCEEDINGS.

RULE 10. Whenever practicable, the proceedings shall be reported by professional reporters or stenographers, whose reports are to be revised by the secretaries before they appear in print.

PAPERS AND COMMUNICATIONS.

RULE 11. No paper shall be placed in the programme, unless admitted by the Sectional Committee; nor shall any be read, unless an abstract of it has been previously presented to the Secretary of the Section, who shall furnish to the chairman the titles of papers, of which abstracts have been received.

RULE 12. The author of any paper or communication shall be at liberty to retain his right of property therein, provided he declare such to be his wish before presenting it to the Association.

RULE 13. Copies of all communications, made either to the General Association or to the Sections, must be furnished by the authors; otherwise only the titles or abstracts shall appear in the published proceedings.

RULE 14. All papers, either at the general or in the sectional meetings, shall be read, as far as practicable, in the order in which they are entered upon the books of the Association; except that those which may be entered by a member of the Standing Committee of the Association shall be liable to postponement by the proper Sectional Committee.

RULE 15. If any communication be not ready at the assigned time, it shall be dropped to the bottom of the list, and shall not be entitled to take precedence of any subsequent communication.

RULE 16. No exchanges shall be made between members without authority of the respective Sectional Committees.

GENERAL AND EVENING MEETINGS.

RULE 17. The Standing Committee shall appoint any general meeting which the objects and interests of the Association may call for, and the evenings shall, as a rule, be reserved for general meetings of the Association.

These general meetings may, when convened for that purpose, give their attention to any topics of science which would otherwise come before the Sections.

It shall be a part of the business of these general meetings to receive the Address of the President of the last meeting; to hear such reports on scientific subjects as, from their general importance and interests, the Standing Committee shall select; also to receive from the chairmen of the Sections abstracts of the proceedings of their respective Sections; and to listen to communications and lectures explanatory of new and important discoveries and researches in science, and new inventions and processes in the arts.

ORDER OF PROCEEDINGS IN ORGANIZING A MEETING.

RULE 18. The Association shall be called to order by the President of the preceding meeting, and this officer having resigned the chair to the president elect, the General Secretary shall then report the number of papers relating to each department which have been registered, and the Association consider the most eligible distribution into Sections, when it shall proceed to the election of the additional members of the Standing Committee in the manner before described; the meeting shall then adjourn, and the Standing Committee, having divided the Association into Sections as directed, shall allot to each its place of meeting for the Session. The Sections shall then organize by electing their officers and their representatives in the Nominating Committee, and shall proceed to business.

PERMANENT SECRETARY.

RULE 19. It shall be the duty of the Permanent Secretary to notify members who are in arrears, to provide the necessary stationery and suitable books for the list of members and titles of papers, minutes of the general and sectional meetings, and for other purposes indicated in the rules, and to execute such other duties as may be directed by the Standing Committee or by the Association.

The Permanent Secretary shall make a report annually to the Standing Committee, at its first meeting, to be laid before the Association, of the business of which he has had charge since its last meeting.

All members are particularly desired to forward to the Permanent Secretary, so as to be received before the day appointed for the Association to convene, complete titles of all the papers which they expect to present during its meeting, with an estimate of the time required for reading each, and such abstracts of their contents as may give a general idea of their nature.

Whenever the Permanent Secretary notices any error of fact or unnecessary repetition, or any other important defect in the papers communicated for publication in the proceedings of the Association, he is authorized to commit the same to the author, or to the proper sub-committee of the Standing Committee for correction.

LOCAL COMMITTEE.

RULE 20. The Local Committee shall be appointed from among members residing at or near the place of meeting for the ensuing year; and it shall be the duty of the Local Committee, assisted by the officers, to make arrangements and the necessary announcements for the meeting.

The Secretary of the Local Committee shall issue a circular in regard to the time and place of meetings, and other particulars, at least one month before each meeting.

SUBSCRIPTIONS.

RULE 21. The amount of the subscription, at each meeting, of each member of the Association shall be two dollars, and one dollar in addition shall entitle him to a copy of the proceedings of the annual meeting. These subscriptions shall be received by the Permanent Secretary, who shall pay them over, after the meeting, to the Treasurer.

No person shall be considered a member of the Association until the subscription for the meeting at which he is elected has been paid.

RULE 22. The names of all persons two years in arrears for annual dues shall be erased from the list of members; provided that two notices of indebtedness, at an interval of at least three months, shall have been previously given.

ACCOUNTS.

RULE 23. The accounts of the Association shall be audited annually, by auditors appointed at each meeting.

ALTERATIONS OF THE CONSTITUTION.

RULE 24. No article of this constitution shall be altered, or amended, or set aside, without the concurrence of three fourths of the members present, and unless notice of the proposed change shall have been given at the preceding annual meeting.

RESOLUTIONS

OF A

PERMANENT AND PROSPECTIVE CHARACTER,

ADOPTED AUGUST 19, 1857.

---

1. No appointment may be made in behalf of the Association, and no invitation given or accepted, except by vote of the Association or its Standing Committee.

2. The General Secretary shall transmit to the Permanent Secretary for the files, within two weeks after the adjournment of every meeting, a record of the proceedings of the Association and the votes of the Standing Committee. He shall also daily, during the meetings, provide the chairmen of the two sectional committees with lists of the papers assigned to their Sections by the Standing Committee.

3. All printing for the Association shall be superintended by the Permanent Secretary, who is authorized to employ a clerk for that especial purpose.

4. The Permanent Secretary is authorized to put the proceedings of the meetings to press one month after the adjournment of the Association. Papers which have not been received at that time may be published only by title. No notice of articles not approved shall be taken in the published proceedings.

5. The Permanent Chairmen of the Sections are to be considered their organs of communication with the Standing Committee.

6. It shall be the duty of the Secretaries of the two Sections to

receive copies of the papers read in their Sections, all sub-sections included, and to furnish them to the Permanent Secretary at the close of the meeting.

7. The Sectional Committees shall meet not later than 9 A. M. daily during the meetings of the Association, to arrange the programmes of their respective Sections, including all sub-sections, for the following day. No paper shall be placed upon these programmes which shall not have been assigned to the Section by the Standing Committee. The programmes are to be furnished to the Permanent Secretary not later than 11 A. M.

8. During the meetings of the Association the Standing Committee shall meet daily, Sundays excepted, at 9 A. M., and the Sections be called to order at 10 A. M., unless otherwise ordered. The Standing Committee shall also meet on the evening preceding the first assembling of the Association at each annual meeting, to arrange for the business of the first day, and on this occasion three shall form a quorum.

9. Associate members may be admitted for one, two, or three years, as they shall choose at the time of admission, — to be elected in the same way as permanent members, and to pay the same dues. They shall have all the social and scientific privileges of members, without taking part in the business.

10. No member may take part in the organization and business arrangement of both the Sections.

MEMBERS

OF THE

AMERICAN ASSOCIATION

FOR THE

ADVANCEMENT OF SCIENCE.

NOTE. — Names of deceased members are marked with an asterisk (*). The figure at the end of each name refers to the meeting at which the election took place.

A.

Abbott, Gorham D., New York, New York [7].
Abernethy, William, Oregon City, California [12].
Abert, Col. J. J., Washington, District of Columbia [1].
*Adams, Prof. C. B., Amherst, Massachusetts [1].
Adamson, J. C., New York, New York [7].
Agassiz, Prof. Louis, Cambridge, Massachusetts [1].
Aiken, Prof. W. E. A., Baltimore, Maryland [12].
Albert, Augustus J., Baltimore, Maryland [12].
Alexander, Prof. Stephen, Princeton, New Jersey [1].
Allen, Prof. E. A. H., New Bedford, Massachusetts [6].
Allen, John H., Oxford, Maryland [6].
Allen, Nathaniel T., West Newton, Massachusetts [10].
Allen, Dr. R. L., Saratoga Springs, New York [10].
Allen, Zachariah, Providence, Rhode Island [1].
Allston, R. F. W., Georgetown, South Carolina [3].

Ames, Bernice D., Fort Edward, New York, New York [10].
*Ames, M. P., Esq., Springfield, Massachusetts [1].
Anderson, Pres. M. B., Rochester, New York [10].
Andrews, Alonzo, Lewiston, Maine [7.].
Andrews, Dr. E. H., Charlotte, North Carolina [3].
Andrews, Prof. E. B., Marietta, Ohio [7].
Andrews, Israel W., Marietta, Ohio [11].
Anthony, Charles H., Esq., Albany, New York [6].
Anthony, Henry, Providence, Rhode Island [9].
Anthony, J. G., Esq., Cincinnati, Ohio [1].
Appleton, Nathan, Esq., Cambridge, Massachusetts [1].
Appleton, Thomas G., Boston, Massachusetts [8].
Arden, Thomas B., Garrison's P. O., Putnam Co., New York [7].
Armour, A. H., Toronto, Canada [10].
Armsby, Prof. J. H., Albany, New York [6].
Astrop, R. F., Crichton's Store, Burns Co., Virginia [7].
Atkinson, Dr. Robert, Baltimore, Maryland [12].
Atterbury, Rev. John G., New Albany, Indiana [11].
Austin, Samuel, Providence, Rhode Island [9].

B.

Baby, G., Montreal, Canada [11].
Bache, Prof. Alexander D., Washington, District of Columbia [1].
Bache, Dr. Franklin, Philadelphia, Pennsylvania [1].
Bacon, Dr. John, Jr., Boston, Massachusetts [1].
Bacon, William, Richmond, Berkshire Co., Massachusetts [7].
Bagg, Dr. Moses M., Utica, New York [4].
*Bailey, Prof. J. W., West Point, New York [1].
Baird, Prof. S. F., Washington, District of Columbia [1].
Baird, Dr. Thomas D., Baltimore, Maryland [12].
Baldwin, F. H., Waverly, New York [10].
Barber, Edgar A., Albany, New York [10].
Barlow, Thomas, Canastota, New York [7].
Barnard, Pres. F. A. P., Oxford, Mississippi [7].
Barnard, Henry, Hartford, Connecticut [12].
Barnes, Capt. James, Springfield, Massachusetts [5].
Barnston, Dr. James, Montreal, Canada [10].

Barringer, Hon. D. M., Charlotte, North Carolina [12].
Barrows, George B., Fryeburg, Maine [7].
Barry, L. F., Baltimore, Maryland [12].
Bartlett, J. R., Providence, Rhode Island [8].
Bartlett, Prof. W. H. C., West Point, New York [9].
Barton, Dr. E. H., New Orleans, Louisiana [9].
Bassnett, Thomas, Ottawa, Illinois [8].
Batchelder, J. M., Cambridge, Massachusetts [8].
Baudry, J. H., Montreal, Canada [11].
Baylis, James, Montreal, Canada [11].
Beadle, Dr. E. L., New York, New York [1].
Beadle, Rev. E. R., Hartford, Connecticut [10].
Beck, Dr. C. F., Philadelphia, Pennsylvania [1].
*Beck, Prof. Lewis C., New Brunswick, New Jersey [1].
*Beck, Dr. T. Romeyn, Albany, New York [1].
Bell, Samuel N., Manchester, New Hampshire [7].
Belle, Charles E., Montreal, Canada [11].
Benedict, Erastus C., New York, New York [10].
Benedict, F. N., Parisippany, New Jersey [1].
Benedict, Dr. N. B., New Orleans, Louisiana [10].
Benedict, Thomas B., Kirk's Ferry, Louisiana [11].
Bent, Silas, U. S. N., New York, New York [10].
Berdon, Hiram, New York, New York [12].
Berezy, William, Daillebaut, Canada East [11].
Bernard, Dr. A., Montreal, Canada [11].
Bidwell, Walter H., New York, New York [11].
Bigelow, Artemas, Newark, New Jersey [9].
Billings, E., Montreal, Canada [11].
*Binney, Dr. Amos, Boston, Massachusetts [1].
Binney, Amos, Boston, Massachusetts [9].
*Binney, John, Boston, Massachusetts [3].
Blackie, Dr. George S., Nashville, Tennessee [10].
Blackmarr, Rev. Henry, Rochester, New York [1[illegible]].
Blake, Rev. C. M., Chili [11].
Blake, Eli W., New Haven, Connecticut [1].
Blake, J. R., LaGrange, Tennessee [10].
Blake, William P., Washington, District of Columbia [2].
*Blanding, Dr. William, Rhode Island [1].

Blaney, Prof. James V. Z., Chicago, Illinois [12].
Blatchford, Dr. Thomas W., Troy, New York [6].
Bledsoe, Prof. A. T., University of Virginia, Virginia [12].
Bolton, James, Richmond, Virginia [10].
*Bomford, Col. George, Washington, District of Columbia [1].
Bonar, Rev. J. B., Montreal, Canada [11].
Bonnycastle, Sir Charles, Montreal, Canada [1].
Botta, Prof. Vincenzo, New York, New York [9].
Bouve, Thomas T., Boston, Massachusetts [1].
Bowditch, Dr. Henry J., Boston, Massachusetts [2].
Bowman, Francis C., New York, New York [12].
Boyden, Uriah A., Boston, Massachusetts [2].
Boynton, John F., Chicago, Illinois [4].
Bradford, Dr. James C., Elyria, Ohio [11].
Bradford, George W., Homer, New York [10].
Braithwaite, Rev. Joseph, Chambly, Canada West [11].
Brant, James R., New York, New York [9].
Brevoort, J. Carson, Brooklyn, New York [1].
Brewer, Fisk P., New Haven, Connecticut [11].
Briston, William, Montreal, Canada [11].
Brocklesby, Prof. John, Hartford, Connecticut [4].
Bross, William, Chicago, Illinois [7].
Brown, Andrew, Natchez, Mississippi [1].
Brown, John C., Providence, Rhode Island [9].
Brown, Richard, Sydney, Cape Breton [1].
Brown, Robert, Jr., Cincinnati, Ohio [11].
Brunnow, Prof. F., Ann Arbor, Michigan [10].
Brush, George J., New Haven, Connecticut [11].
Brush, George S., Montreal, Canada [11].
Buchanan, Robert, Cincinnati, Ohio [2].
Buell, David, Jr., Troy, New York [6].
Bulkley, John W., Brooklyn, New York [10].
Bullard, Edward F., Waterford, New York [10].
Burnap, Rev. G. W., Baltimore, Maryland [12].
*Burnett, Waldo I., Boston, Massachusetts [1].
Burton, Dr. C. V. W., Lansingburg, New York [6].
Busher, James, Worcester, Massachusetts [9].
Butler, Hon. Thomas B., Norwalk, Connecticut [10].

C.

Cabell, Prof. James L., University of Virginia, Virginia [6].
Cady, Rev. Calvin B., Alburgh, Vermont [11].
Campbell, John, New York, New York [10].
Campbell, A. D., Montreal, Canada [11].
Campbell, Prof. George W., Montreal, Canada [11].
Campbell, W. D., Quebec, Canada [11].
Cartier, Hon. G. E., Montreal, Canada [11].
Carley, S. T., Cincinnati, Ohio [5].
Carpenter, Prof. J. N., Washington, District of Columbia [12].
*Carpenter, Thornton, Camden, South Carolina [7].
*Carpenter, Dr. William M., New Orleans, Louisiana [1].
Carr, E. S., Albany, New York [9].
Case, Hon. William, Cleveland, Ohio [6].
Cassells, Prof. J. L., Cleveland, Ohio [7].
Caswell, Prof. Alexis, Providence, Rhode Island [2].
Cavert, Michael P., Amsterdam, New York [10].
Chadbourne, Prof. P. A., Williamstown, Massachusetts [10].
Chamberlain, Frank, Albany, New York [10].
Chamberlin, B. C., Montreal, Canada [11].
Channing, Dr. William F., Boston, Massachusetts [2].
Chapin, L. C., New Haven, Connecticut [11].
*Chapman, Dr. N., Philadelphia, Pennsylvania [1].
Chapman, Prof. C. B., Madison, Wisconsin [11].
Chapman, Prof. Edward, Jr., Toronto, Canada [11].
Chase, Rev. Benj., Natchez, Mississippi [7].
Chase, Prof. George I., Providence, Rhode Island [1].
*Chase, Prof. S., Dartmouth, New Hampshire [2].
Chauveau, Pierre I. O., Montreal, Canada [10].
Chauvenet, Prof. William, Annapolis, Maryland [1].
Cherriman, J. B., Toronto, Canada [9].
Chesbrough, E. S., Chicago, Illinois [2].
Church, Prof. A. E., West Point, New York [10].
Clapp, Dr. Asahel, New Albany, Indiana [1].
Clark, Alvin, Cambridgeport, Massachusetts [4].
Clark, J. H., Philadelphia, Pennsylvania [12].
*Clark, Joseph, Cincinnati, Ohio [5].

Clark, Lester M., Canandaigua, New York [10].
Clark, Maj. M. Lewis, St. Louis, Missouri [5].
Clark, Prof. James, Georgetown, District of Columbia [8].
Clarke, Robert, Cincinnati, Ohio [7].
Cleaveland, Prof. C. H., Cincinnati, Ohio [9].
Cleghorn, Rev. E. B., New Orleans, Louisiana [11].
*Cleveland, Dr. A. B., Cambridge, Massachusetts [2].
Clum, Henry A., New York, New York [9].
Coakley, Prof. George W., Hagerstown, Maryland [5].
Cochran, J. W., New York, New York [11].
Coffin, Prof. James H., Easton, Pennsylvania [1].
Coffin, Prof. John H. C., Annapolis, Maryland [1].
Cogswell, Dr. Mason F., Albany, New York [4].
*Cole, Thomas, Salem, Massachusetts [1].
*Coleman, Rev. Henry, Boston, Massachusetts [1].
Collier, Prof. G. H., Wheaton, Illinois [11].
Collins, Dr. C., Carlisle, Pennsylvania [11].
Comfort, G. F., Middletown, Connecticut [10].
Comstock, J. C., Hartford, Connecticut [11].
Conant, Marshall, South Bridgewater, Massachusetts [7].
Congdon, Charles, New York, New York [1].
Conkling, Frederick A., New York, New York [11].
Cook, Prof. George H., New Brunswick, New Jersey [6].
Cooley, James E., New York, New York [10].
Cooper, Dr. J. G., Washington, District of Columbia [10].
Cooper, William, Hoboken, New Jersey [9].
Copes, Dr. Joseph S., New Orleans, Louisiana [11].
Cordner, Rev. John, Montreal, Canada [11].
Corning, Hon. Erastus, Albany, New York [6].
Cottle, Thomas J., Woodstock, Upper Canada [10].
Couper, J. Hamilton, Darien, Georgia [1].
Cowan, Dr. J. P., Montreal, Canada [11].
Cox, Dr. Christopher C., Easton, Maryland [12].
Craik, Dr. Robert, Montreal, Canada [11].
Cramp, Rev. J. M., Acadia College, Nova Scotia [11].
Crandall, Pardon S., Troy, New York [10].
Croft, Prof. Henry, Toronto, Canada [11].
Crosby, Alpheus, Hanover, New Hampshire [10].

Croswell, Edwin, Albany, New York [6].
Cruikshank, James, Albany, New York [10].
Cummings, William M., New York, New York [11].
Curley, Prof. James, Georgetown, District of Columbia [8].
Curry, Rev. W. F., Geneva, New York [11].
Cunynghame, Thurlow, Montreal, Canada [11].
Curtis, Jasper, St. Albans, Vermont [11].

D.

Dalrymple, Rev. E. A., Baltimore, Maryland [11].
Dalton, Prof. John C., Jr., New York, New York [11].
Dana, Prof. James D., New Haven, Connecticut [1].
Danforth, Edward Clarence, New York, New York [11].
Dascomb, Prof. James, Oberlin, Ohio [7].
Davenport, Rev. James R., Albany, New York [6].
Davidson, Robert, New Brunswick, New Jersey [10].
David, Dr. A. H., Montreal, Canada [11],
Davies, Prof. Charles, Fishkill Landing, New York [10].
Davies, W. H. A., Montreal, Canada [11].
Davies, W. H., Toronto, Canada [11].
Davis, James, Jr., Boston, Massachusetts [1].
Davis, Prof. N. K., Marion, Alabama [9].
Dawson, Prof. J. W., Montreal, Canada [10].
Dayton, Edwin A., Madrid, St. Lawrence Co., New York [7].
Day, Charles D., Montreal, Canada [11].
Day, John J., Montreal, Canada [11].
Dean, Prof. Amos, Albany, New York [6].
*Dearborn, Geo. H. A. S., Roxbury, Massachusetts [1].
*DeKay, Dr. James E., New York, New York [1].
Delafield, Joseph, New York, New York [1].
Delano, Joseph C., New Bedford, Massachusetts [5].
Delavan, Edward C., Albany, New York [10].
Denson, Claudius B., Portsmouth, Virginia [12].
Devol, Charles, Albany, New York [10].
Dewey, Prof. Chester, Rochester, New York [1].
Dexter, George, Albany, New York [10].
Dexter, James, Albany, New York [6].

Dexter, G. M., Boston, Massachusetts [11].
Dickinson, John W., Westfield, Massachusetts [10].
Dickson, Andrew, Kingston, Canada [11].
Dickson, Rev. Cyrus, Baltimore, Maryland [12].
Dield, Rev. Israel S., Sacramento, California [10].
Dinwiddie, Robert, New York, New York [1].
Disturnell, John, New York, New York [11].
Dixwell, Epes S., Cambridge, Massachusetts [1].
Donaldson, Dr. Francis, Baltimore, Maryland [12].
Doolittle, Rev. L., Lenoxville, Canada East [11].
Doremus, R. Ogden, New York, New York [10].
Douglass, Prof. Silas H., Ann Arbor, Michigan [6].
Dowie, J. Muir, Liverpool, England [11].
Downes, John, Washington, District of Columbia [10].
Doyle, John P., Montreal, Canada [11].
Drowne, Prof. Charles, Troy, New York [6].
*Ducatel, Dr. J. T., Baltimore, Maryland [1].
Duffield, George, D. D., Detroit, Michigan [10].
*Duncan, Lucius C., New Orleans, Louisiana [10].
Dunglison, Prof. Robley, Philadelphia, Pennsylvania [8].
Dunkin, Christopher, Montreal, Canada [11].
Dupuy, Prof. Charles H., Baltimore, Maryland [12].
Dwinelle, John H., Rochester, New York, New York [11].
Dwinelle, William H., New York, New York [10].
Dyer, David, Albany, New York [10].
Dyer, Elisha, Providence, Rhode Island [9].

E.

Easter, Prof. John D., Esq., Athens, Georgia [6].
Eaton, A. K., New York, New York [11].
Edmondstone, William, Montreal, Canada [11].
Edmundson, Thomas, Baltimore, Maryland [10].
Edwards, Richard, Salem, Massachusetts [10].
Elderhorst, Prof. William, Troy, New York [10].
Elin, Frederick, London, England [11].
Elliot, Ezekiel B., Boston, Massachusetts [10].
Elwyn, Dr. Alfred L., Philadelphia, Pennsylvania [1].

Ely, Charles Arthur, Elyria, Ohio [4].
Emerson, George B., Boston, Massachusetts [1].
Emmons, Dr. Ebenezer, Williamstown, Massachusetts [1].
Emory, Col. William H., U. S. A., Washington, D. C. [5].
Engelmann, Dr. George, St. Louis, Missouri [1].
Engstrom, A. B., Esq., Burlington, New Jersey [1].
Estes, Dr. D. C., Albany, New York [10].
Eustis, Prof. H. L., Cambridge, Massachusetts [2].
Eve, Prof. Paul F., Nashville, Tennessee [12].
Everett, Hon. Edward, Boston, Massachusetts [2].
Ewing, Hon. Thomas, Lancaster, Ohio [5].

F.

Fairchild, Charles, Madison, Wisconsin [11].
Fairchild, Prof. J. H., Oberlin, Ohio [5].
Fairfield, J. W., Hudson, New York [10].
Fairie, James, Bastross, Morehouse Parish, Louisiana [7].
Fairly, Dr. David, Glasgow, Scotland [11].
Farmer, Moses G., Boston, Massachusetts [9].
Farnham, Prof. J. E., Georgetown, Kentucky [7].
Farquhar, W. H., Fair Hill, Montgomerie Co., Maryland [12].
Fay, Rev. Charles, St. Albans, Vermont [11].
Fearing, Dr. E. P., Nantucket, Massachusetts [8].
Fellows, Joseph, Albany, New York [10].
Ferland, J. B. A., Quebec, Canada [11].
Ferrel, William, Nashville, Tennessee [11].
Ferris, Rev. Dr. Isaac, New York, New York [6].
Feuchtwanger, Dr. Louis, New York, New York [11].
Fillmore, Millard, Buffalo, New York [7].
Fisher, Mark, Trenton, New Jersey [10].
Fisher, Prof. George P., New Haven, Connecticut [11].
Fisher, Dr. N. A., Norwich, Connecticut [9].
Fisk, Prof. L. R., Ypsilanti, Michigan [10].
Fitch, Edward H., Ashtabula, Ohio [11].
Fitch, Alexander, Hartford, Connecticut [1].
Fitch, O. H., Ashtabula, Ohio [7].
Folsom, George, New York, New York [11].
Foote, Elisha, Seneca Falls, New York [10].

Force, Dr. Charles F., Washington, District of Columbia [12].
Force, Col. Peter, Washington, District of Columbia [4].
Ford, Richard, Columbia, South Carolina [12].
Forshey, Prof. Caleb G., Rutersville, Texas [7].
Fosgate, Dr. Blanchard, Auburn, New York [7].
Foster, J. W., Monson, Massachusetts [1].
Foster, Prof. John, Schenectady, New York [1].
Fowle, William B., Boston, Massachusetts [1].
*Fox, Rev. Charles, Grosse Isle, Michigan [7].
Francfort, Dr. E., Middletown, Connecticut [9].
Franklin, Danforth, Buffalo, New York [11].
Fraser, Hugh, Montreal, Canada [11].
Fraser, Prof. W., Montreal, Canada [11].
Frazer, Prof. John F., Philadelphia [1].
Freeman, Dr. Samuel H., Albany, New York [10].
French, J. H., Syracuse, New York [11].
French, Rev. J. W., West Point, New York [11].
Fristoe, Edward T., Washington, District of Columbia [11].
Frothingham, Rev. Frederick, Portland, Maine [11].
Frothingham, George H., Montreal, Canada [11].

G.

Gale, Hon. Samuel, Montreal, Canada [11].
Gale, L. D., Washington, District of Columbia [8].
Gardner, James S., Whitestown, New York [10].
Garrigues, Dr. S. S., Philadelphia, Pennsylvania [10].
Garth, Charles, Montreal, Canada [11].
Gavit, John E., Albany, New York [1].
Gay, A. M., Charlestown, Massachusetts [10].
*Gay, Dr. Martin, Boston, Massachusetts [1].
Geddes, Charles, Montreal, Canada [11].
Geddings, Prof. E., Charleston, South Carolina [3].
Gibbes, Prof. L. R., Charleston, South Carolina [1].
Gibbes, Prof. Robert W., Columbia, South Carolina [1].
Gibbon, Dr. J. H., Charlotte, North Carolina [3].
Gibbs, Dr. Wolcott, New York, New York [1].
Gibbs, J. Campbell, Montreal, Canada [11].
Gifford, J. P. S., Albany, New York [10].

Gillespie, Prof. W. M., Schenectady, New York [10].
Gilman, Charles, Baltimore, Maryland [12].
Gilman, Daniel C., New Haven, Connecticut [10].
Gilman, Dr. Judson, Baltimore, Maryland [12].
*Gilmor, Robert, Esq., Baltimore, Maryland [1].
Gladstone, T. H., London, England [10].
Gluck, Isidor, New York, New York [10].
Glynn, Com. James, U. S. N., New Haven, Connecticut [1].
Gold, Theodore S., West Cornwall, Connecticut [4].
Goodwin, William F., Concord, New Hampshire [10].
Gould, Dr. Augustus A., Boston, Massachusetts [11].
Gould, B. A., Boston, Massachusetts [2].
Gould, Dr. B. A., Jr., Albany, New York [2].
Graham, Col. James D., U. S. A., Washington, D. C. [1].
Grant, John M., Montreal, Canada [11].
Grant, S. H., New York, New York [11].
Gray, Dr. Henry C., Washington, District of Columbia [12].
Gray, Prof. Asa, Cambridge, Massachusetts [1].
*Gray, Dr. James H., Springfield, Massachusetts [6].
Green, Horace, New York, New York [10].
Green, Dr. John W., New York, New York [9].
Green, Dr. Traill, Easton, Pennsylvania [1].
Greene, Dr. Benjamin D., Boston, Massachusetts [1].
Greene, David B., New York, New York [10].
Greene, Everett W., Madison, New Jersey [10].
Greene, Prof. B. Franklin, Troy, New York [1].
Greene, Dr. F. C., East Hampton, Massachusetts [11].
Greene, Samuel, Woonsocket, Rhode Island [9].
Greene, Prof. Samuel S., Providence, Rhode Island [9].
Greene, Thomas A., New Bedford, Massachusetts [9].
Griffin, Prof. Nathaniel H., Williamstown, Massachusetts [10].
*Griffith, Dr. Robert E., Philadelphia [1].
Grinnan, A. G., Madison Court House, Virginia [7].
Groneweg, Lewis, Dayton, Ohio [7].
Grosvenor, A. C., Cincinnati, Ohio [5].
Grosvenor, Dr. William, Providence, Rhode Island [9].
Grunow, Julius, New Haven, Connecticut [10].
Guerin, Thomas, Albany, New York [11].

Guest, William E., Ogdensburg, New York [6].
Gulick, John T., Williamstown, Massachusetts [10].
Gunmere, Samuel J., Burlington, New Jersey [7].
Guyot, Prof. Arnold, Princeton, New Jersey [1].

H.

Hackley, Prof. Charles W., New York, New York [4].
Hager, Albert D., Proctorsville, Vermont [11].
Haines, William S., Providence, Rhode Island [9].
Haldeman, Prof. S. S., Columbia, Pennsylvania [1].
Hale, Albert W., Albany, New York [10].
*Hale, Dr. Enoch, Boston, Massachusetts [1].
Hall, Archibald, Montreal, Canada [10].
Hall, Prof. James, Albany, New York [1].
Hall, Joel, Athens, Illinois [7].
Hall, N. K., Buffalo, New York [7].
Hallowell, Benjamin, Alexandria, Virginia [7].
Hamlin, A. C., Bangor, Maine [10].
Hammond, Ogden, Charleston, South Carolina [3].
Hance, Ebenezer, Morrisville, Bucks Co., Pennsylvania [7].
Hand, Thomas J., Baltimore, Maryland [12].
Handy, Rev. Isaac W. K., Portsmouth, Virginia [10].
*Hare, Dr. Robert, Philadelphia, Pennsylvania [11].
*Harlan, Prof. Joseph G., Haverford, Pennsylvania [8].
*Harlan, Dr. Richard, Philadelphia, Pennsylvania [1].
Harman, Rev. Henry M., Baltimore, Maryland [12].
Harrequi, Prof. Jose Salazar, Minerra, Mexico [10].
Harris, Ira, Albany, New York [11].
*Harris, Dr. Thaddeus W., Cambridge, Massachusetts [1].
Harris, Prof. Chapin A., Baltimore, Maryland [12].
Harrison, B. F., Wallingford, Connecticut [11].
Harrison, Edwin, St. Louis, Missouri [11].
Harrison, Joseph, Jr., Philadelphia, Pennsylvania [12].
*Hart, Simeon, Farmington, Connecticut [1].
Hart, Theodore, Montreal, Canada [11].
Hartshorne, Dr. Henry, Philadelphia, Pennsylvania [12].
Harvey, Hon. Matthew, Concord, New Hampshire [1].
Harvey, Prof. William H., Trinity College, Dublin, Ireland [3].

Hathaway, Charles, Delhi, New York [10].
Haven, Samuel F., Worcester, Massachusetts [9].
Hayden, Dr. F. V., Washington, District of Columbia [12].
*Hayden, Dr. H. H., Baltimore, Maryland [1].
Hayward, James, Boston, Massachusetts [1].
Hayes, Dr. Isaac I., Philadelphia, Pennsylvania [12].
Hazard, Rowland, Peace Dale, Rhode Island [9].
Headlam, William, Jr., Albany, New York [11].
Heffron, Daniel S., Utica, New York [10].
Helme, W. H., Providence, Rhode Island [9].
Heneker, William R., Sherbrooke, Canada East [11].
Henry, Prof. Joseph, Washington, District of Columbia [1].
Herring, William A. Duplin, Sampson Co., North Carolina [12].
Hickok, Rev. M. J., Scranton, Pennsylvania [11].
Hicks, Levi I., Walworth, New York [10].
Higgins, Dr. James, Baltimore, Maryland [12].
Hilgard, Eugene W., Oxford, Mississippi [11].
Hilgard, Julius E., Washington, District of Columbia [4].
Hilgard, Dr. T. C., St. Louis, Missouri [8].
Hill, Benjamin, Montreal, Canada [11].
Hill, Nathaniel P., Providence, Rhode Island [10].
Hill, S. W., Eagle Harbor, Lake Superior [6].
Hill, Rev. Thomas, Waltham, Massachusetts [3].
Hincks, Rev. William, Toronto, Canada West [11].
Hingston, Dr. W. P., Montreal, Canada [10].
Hitchcock, Charles H., Amherst, Massachusetts [11].
Hitchcock, Prof. Edward, Amherst, Massachusetts [1].
Hitchcock, Edward, Jr., Amherst, Massachusetts [4].
Hoblitzell, J. H., Baltimore, Maryland [12].
Hodgins, George, Toronto, Canada West [11].
Hodgson, W. B., Savannah, Georgia [10].
Holland, Joseph B., Monson, Massachusetts [9].
Holmes, Prof. A. F., Montreal, Canada [11].
Holmes, Benjamin, Montreal, Canada [11].
Holmes, Prof. Francis S., Charleston, South Carolina [3].
Holton, L. H., Montreal, Canada [11].
Holwell, W. A., Quebec, Canada [11].

Homans, J. S., Jr., New York, New York [12].
Homans, Sheppard, New York, New York [10].
Homes, Henry A., Albany, New York [11].
Hopkins, James G., Ogdensburg, New York [10].
Hopkins, Rev. John H., New York, New York [11].
Hopkins, T. O., Williamsville, New York [10].
Hopkins, Prof. William, Lima, New York [5].
Horan, E. J., Quebec, Canada [10].
Horsford, Prof. E. N., Cambridge, Massachusetts [1].
Horton, C. V. R., Chaumont, New York [10].
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Hough, Dr. Franklin B., Albany, New York [4].
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Howard, Dr. R. P., Montreal, Canada [11].
Howe, Prof. Henry A., Montreal, Canada [11].
Howell, Robert, Nichols, Tioga Co., New York [6].
Hubbard, Prof. Oliver P., Hanover, New Hampshire [1].
Humphrey, Frederic, Iowa City, Iowa [11].
Humphrey, S. D., New York, New York [6].
Humphreys, Capt. A. A., U. S. A., Washington, D. C. [11].
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Hunt, George, Providence, Rhode Island [9].
Hunt, Lieut. E. B., U. S. A., Bristol, Rhode Island [2].
Hunt, Thomas S., Montreal, Canada [1].
Hunter, Andrew W., New Orleans, Louisiana [11].
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Huson, Calvin, Jr., Rochester, New York, New York [11].
Hyatt, James, New York, New York [10].

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Ives, Moses B., Providence, Rhode Island [9].
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Jackson, Dr. Charles T., Boston, Massachusetts [1].
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James, Prof. Charles S., Lewisburg, Pennsylvania [10].
James, M. P., Cincinnati, Ohio [5].
James, John, Alton, Illinois [10].
Janes, D. P., Montreal, Canada [11].
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Joseph, J. H., Montreal, Canada [11].
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O'Leary, Prof. Charles, Emmetsburg, Maryland [10].
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Opdyke, George, New York, New York [8].
Ordway, John M., Roxbury, Massachusetts [9].
Ormiston, Rev. William, Hamilton, Canada [10].
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Parker, Hon. Amasa J., Albany, New York [6].
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Parker, Prof. William H., Middlebury, Vermont [10].
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Parsons, Dr. Usher, Providence, Rhode Island [9].
Parvin, Theodore S., Muscatine, Iowa [7].
Paton, George, Galt, Canada West [11].
Paton, John, Toronto, Canada [11].
Patterson, Prof. J. W., Hanover, New Hampshire [11].
Peale, Titian R., Washington, District of Columbia [1].
Peckham, Dr. F. H., Providence, Rhode Island [12].
Peirce, Prof. Benjamin, Cambridge, Massachusetts [1].
Peirce, James W., Cambridge, Massachusetts [11].
Perkins, Prof. G. R., Utica, New York [1].
Perkins, J. A., Montreal, Canada [11].
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Peters, Dr. C. H. F., Clinton, New York [9].
Phelps, Philip, Jr., Hastings, New York [10].
Phelps, Prof. William F., Trenton, New Jersey [11].
Pierce, Prof. William M., Geneva, Illinois [10].
Piggot, A. Snowden, Baltimore, Maryland [10].
Pitcher, Dr. Zina, Detroit, Michigan [1].
Pitman, Benn, Cincinnati, Ohio [10].
Plant, I. C., Macon, Georgia [3].
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Pope, Prof. Charles A., St. Louis, Missouri [12].
Porter, Prof. John A., New Haven, Connecticut [4].
Porter, Prof. Thomas C., Lancaster, Pennsylvania [12].

Potter, Rev. Dr. H. B., Albany, New York [6].
Pourtales, L. F., Washington, District of Columbia [1].
Powers, A. E., Lansingburg, New York [1].
Pratt, J. D., Baltimore, Maryland [12].
Prentice, E. P., Albany, New York [2].
Prescott, Dr. William, Concord, New Hampshire [1].
Priest, Rev. J. A., Homer, New York [10].
Prince, W. R., Flushing, Long Island [10].
Prout, Dr. H. A., St. Louis, Missouri [7].
Pruyn, J. V. L., Albany, New York [1].
Pruyn, Robert H., Albany, New York [10].
Putnam, F. W., Cambridge, Massachusetts [10].
Pynchon, Prof. Thomas R., Hartford, Connecticut [11].

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Quincy, Edmund, Jr., Dedham, Massachusetts [11].

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Ramsay, Andrew C., London, England [11].
Ramsay, J. K., Montreal, Canada [11].
Rankin, Robert G., New York, New York [10].
Rauch, Dr. John H., Burlington, Iowa [11].
Ravenel, Dr. Edmund, Charleston, South Carolina [1].
Read, D. B., Toronto, Canada [11].
Redfield, Charles B., Albany, New York [11].
Redfield, John H., New York, New York [1].
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Reed, Lyman, Baltimore, Maryland [12].
Reid, D. B., London, England [10].
Renwick, Prof. James, New York, New York [1].
Resor, Jacob, Cincinnati, Ohio [8].
Reuben, Levi, New York, New York [10].
Rice, Henry, North Attleborough, Massachusetts [7].
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Richards, Rev. William C., Providence, Rhode Island [9].
Richardson, Dr. Horace, Boston, Massachusetts [12].

Riddell, Dr. John L., New Orleans, Louisiana [1].
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Robb, Prof. James, Fredericton, New Brunswick [4].
Robertson, Thomas D., Rockford, Illinois [10].
Robertson, Rev. W. H. C., Stamford, Connecticut [11].
Roberts, W. M., Carlisle, Pennsylvania [5].
Robinson, W. B., Toronto, Canada [11].
Rockwell, Alfred P., Chicago, Illinois [10].
Rockwell, John, Chicago, Illinois [11].
Rockwell, John A., Norwich, Connecticut [10].
Rodman, William M., Providence, Rhode Island [9].
Rogers, Prof. Fairman, Philadelphia, Pennsylvania [11].
Rogers, Prof. Henry D., Glasgow, Scotland [12].
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Rogers, Prof. Robert E., Philadelphia, Pennsylvania [1].
Rogers, Prof. W. B., Boston, Massachusetts [1].
Rogers, William F., Philadelphia, Pennsylvania [10].
Roome, Martin R., New York, New York [10].
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Russell, Andrew, Toronto, Canada [11].
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Ruttan, Dr. Allan, Newburgh, Canada West [11].
Ryan, Thomas, Montreal, Canada [11].
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Schoolcraft, Henry R., Washington, District of Columbia [7].
Schott, Arthur C. V., Washington, District of Columbia [8].
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Scott, Rev. Joseph, Dunham, Canada East [11].
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Selden, George M., Troy, New York [6].
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V.

Vail, Prof. Hugh, Haverford, Pennsylvania [8].
Van Benschoten James C., Oxford, Chenango Co., New York [12].
Vancleve, John W., Dayton, Ohio [1].
Van Derpool, S. Oakley, Albany, New York [9].
Van Pelt, Dr. William, Williamsville, Erie Co., New York [7].
*Vanuxem, Lardner, Bristol, Pennsylvania [1].

Van Vleck, J. M., Middletown, Connecticut [9].
Vaughan, Daniel, Cincinnati, Ohio [5].
Vaux, William S., Philadelphia, Pennsylvania [1].
Verreau, H. A. B., Montreal, Canada [10].

W.

Wadsworth, Charles F., Genesee, New York [11].
Wadsworth, James S., Genesee, New York [2].
Wagner, Tobias, Philadelphia, Pennsylvania [9].
Walker, Rev. James B., Mansfield, Ohio [7].
Walker, Joseph, Oxford, New York [10].
*Walker, Sears C., Washington, D. C. [1].
*Walker, Hon. Timothy, Cincinnati, Ohio [4].
Wallbridge, T. C., Belleville, Canada West [11].
Walworth, Reuben H., Saratoga, New York [10].
Warder, Dr. J. A., Cincinnati, Ohio [4].
Warner, H. G., Rochester, New York [11].
Warren, G. K., U. S. A. Top. Engineers [12].
*Warren, Dr. John C., Boston, Massachusetts [1].
Watertown, Charles, Wakefield, England [1].
Watson, William, Cambridge, Massachusetts [12].
Wayne, Rev. Benjamin, New Orleans, Louisiana [10].
*Webster, H. B., Albany, New York [1].
*Webster, Dr. J. W., Cambridge, Massachusetts [1].
*Webster, M. H., Albany, New York [1].
Webster, Nathan B., Portsmouth, Virginia [7].
Weed, Monroe, Wyoming, New York [6].
Weinland, D. F., Cambridge, Massachusetts [10].
Welch, John, Newark, New Jersey [10.]
Wells, David A., New York, New York [2].
Wells, Dr. Thomas, New Haven, Connecticut [4].
West, Charles E., Buffalo, New York [1].
Wethered, Charles E., Baltimore, Maryland [12].
Wetherell, Prof. L., Legrange, Kentucky [2].
Weyman, G. W., Pittsburg, Pennsylvania [6].
Wheatland, Dr. Henry, Salem, Massachusetts [1].
Wheatley, Charles M., New York, New York [1].
Wheeler, Dr. T. B., Montreal, Canada [11].

Whipple, A. B., Nantucket, Massachusetts [10].
Whipple, J. E., Lansingburg, New York [10].
Whipple, W., Adrian, Michigan [7].
Whitcomb, Joseph M., Salem, New York [10].
White, Aaron, Cazenovia, New York [10].
White, Charles, Crawfordsville, Indiana [10].
White, Horace, Chicago, Illinois [10].
White, Rev. Robert J. P., Chambly, Canada East [11].
Whitney, Asa, Philadelphia, Pennsylvania [1].
Whitney, H. H., Montreal, Canada [11].
Whitney, J. D., Northampton, Massachusetts [1].
Whitney, John R., Philadelphia, Pennsylvania [11].
Whitney, Prof. William D., New Haven, Connecticut [12].
Whittlesey, Charles, Cleveland, Ohio [1].
Whittlesey, Charles C., St. Louis, Missouri [11].
Wilder, Alexander, New York, New York [10].
Wilder, L., Hoosick Falls, New York [1].
Wilgress, George, Montreal, Canada [11].
Wilkes, Capt. Charles, U. S. N., Washington, D. C. [1].
Wilkes, Rev. Henry, Montreal, Canada [11].
Willard, Samuel D., Cayuga, New York [10].
Williams, Dr. Henry W., Boston, Massachusetts [11].
Williams, Samuel Wells, Canton, China [10].
Williams, Thomas H., Portsmouth, Virginia [12].
Williams, W. F., Mosul, Turkey [10].
Williams, William W., Keokuk, Iowa [12].
Williamson, Prof. James, Kingston, Canada [11].
Williamson, Capt. R. S., U. S. A., San Francisco, Cal. [12].
Wilson, Prof. Daniel, Toronto, Canada [10].
Wilson, Prof. W. C., Carlisle, Pennsylvania [12].
Winchell, Prof. Alexander, Ann Arbor, Michigan [3].
Winlock, Prof. Joseph, Cambridge, Massachusetts [5].
Winslow, C. F., Newton, Massachusetts [10].
Winslow, John F., Troy, New York [10].
Wood, William, Portland, Maine [10].
Woodall, John W., Scarborough, England [11].
Woodbridge, George A., Nashville, Tennessee [10].
*Woodbury, Hon. L., Portsmouth, New Hampshire [1].

Woodman, John S., Hanover, New Hampshire [11].
Woolworth, Hon. S. B., Albany, New York [10].
Worthen, A. H., Springfield, Illinois [5].
Wright, Charles, Wethersfield, Connecticut [10].
Wright, Chauncey, Cambridge, Massachusetts [9].
*Wright, Dr. John, Troy, New York [1].
Wurtele, Louis C., Lennoxville, Canada East [11].
Wurtz, Prof. Henry, Washington, District of Columbia [10].
Wyman, Prof. Jeffries, Cambridge, Massachusetts [10].
Wyman, Dr. Morrill, Cambridge, Massachusetts [11].
Wynne, Dr. James, New York, New York [8].
Wynne, Thomas H., Richmond, Virginia [8].

Y.

Youmans, E. L., Saratoga Springs, New York [6].
Young, Prof. Ira, Hanover, New Hampshire [7].
Young, Hon. John, Montreal, Canada [11].
Young, William H., Baltimore, Maryland [12].

The above list contains ten hundred and thirty-four names, of which seventy-two are of deceased members

MEMBERS ELECTED

AT

THE BALTIMORE MEETING.

Abernethy, William, Oregon City, Cal.
*Abrahams, Woodward, Baltimore, Md.
Aiken, Prof. W. E. A., Baltimore, Md.
Albert, Augustus J., Baltimore, Md.
Atkinson, Dr. Robert, Baltimore, Md.

Baird, Dr. Thomas D., Baltimore, Md.
†Baker, George B., Providence, R. I.
*Barnard, Henry, Hartford, Connecticut.
Barringer, Hon. D. M., Charlotte, N. C.
‡Barry, L. F., Baltimore, Maryland.
†Beck, Dr. M. B., Fredericksburg, Va.
‡Berdan, Hiram, New York, New York.
*Blaney, Prof. James V. Z., Chicago, Ill.
*Bledsoe, Prof. A. T., University of Virginia, Va.
Bowman, Francis C., New York, N. Y.
Burnap, Rev. G. W., Baltimore, Md.

Carpenter, Prof. J. N., Washington, D. C.
Clark, J. H., Philadelphia, Penn.
Cox, Dr. Christopher C., Easton, Md.
†Crozet, Prof. Claudius, Washington, D. C.

†Davis, Prof. E. H., New York, N. Y.
Denson, Claudius B., Portsmouth, Va.
Dickson, Rev. Cyrus, Baltimore, Md.
Donaldson, Prof. Francis, Baltimore, Maryland.
Dupuy, Prof. Charles H., Baltimore, Maryland.

Eve, Prof. Paul F., Nashville, Tenn.
§Ewbank, Thomas, New York, N. Y.

Farquhar, W. H., Fair Hill, Montgomery Co., Maryland.
Force, Dr. Charles F., Washington, D. C.
Ford, Richard, Columbia, S. C.

Gilman, Charles, Baltimore, Maryland.
Gilman, Dr. Judson, Baltimore, Md.
Gray, Dr. Henry C., Washington, D. C.

Hand, Thomas I., Baltimore, Maryland.
Harman, Rev. Henry M., Baltimore, Maryland.
*Harris, Prof. Chapin A., Baltimore, Md.
*Harrison, Joseph, Jr., Philadelphia, Pa.
*Hartshorne, Dr. Henry, Philadelphia, Pennsylvania.
*Hayes, Dr. Isaac I., Philadelphia, Penn.
Herring, William A., Duplin, Sampson Co., North Carolina.
Higgins, Dr. James, Baltimore, Md.

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*Those marked with an asterisk paid the assessment, and signed the Constitution without being formally elected.

† Have not been heard from.

‡ Did not sign the Constitution.

§ Decline.

Hoblitzell, J. H., Baltimore, Maryland.
† Hoffman, Hon. S. Owings, Baltimore, Maryland.
‡ Homans, J. S. Jr., New York.

† Jackson, Hon. W. L., Parkersburg, Va.
Johnston, Dr. Christopher, Baltimore, Maryland.

Kennicott, Robert, West Northfield, Ill.
Kimmel, Col. Anthony, Lingamore, Maryland.
Kloman, Dr. W. C., Baltimore, Md.

*La Roche, Dr. C. P., Philadelphia, Pennsylvania.
*La Roche, Dr. R., Philadelphia, Pennsylvania.
† Lawrence, Dr. G.W., Cockeysville, Md.
Lea, Robert M., Aransas, Texas.
*Leidy, Prof. Joseph, Philadelphia.
*Lincoln, Dr. N. L., Washington, D. C.
*Litton, Prof. A., St. Louis, Missouri.
‡ Loosey, Charles F., New York.

‡ McCabe, Rev. James D., Baltimore, Maryland.
‡ McCauley, James, Elkton, Cecil Co., Maryland.
McCron, Rev. John, Baltimore, Md.
McKew, Dr. Dennis, Baltimore, Md.
*Martin, Prof. W. J., Chapel Hill, N. C.
† Mayer, Charles F., Baltimore, Md.
*Meigs, Prof. James A., Philadelphia.
† Meredith, Jonathan, Baltimore, Md.
Merryman, Dr. M., Baltimore, Md.
* ‡ Meyers, Prof. A. M., Baltimore, Md.
Minifie, William, Baltimore, Maryland.
Mittag, J. F. G., Hagerstown, Maryland.
† Morris, Dr. John, Baltimore, Maryland.
Morris, Rev. John G., Baltimore, Md.
Murdock, Charles N., Baltimore, Md.
‡ Murdock, John, Baltimore, Maryland.

† Nelson, Rev. Cleland K., Annapolis, Maryland.

‡ Paine, Cyrus F., Rochester, New York.
*Palmer, Dr. A. B., Detroit, Michigan.
† Pearce, Dr. George A., Providence, R. I.
Peckham, Dr. F. H., Providence, Rhode Island.
*Pope, Prof. Charles A., St. Louis, Mo.
Porter, Prof. Thomas C., Lancaster, Penn.
Pratt, J. D., Baltimore, Maryland.

Reed, Lyman, Baltimore, Maryland.
Richardson, Dr. Horace, Boston, Mass.
† Rives, Alfred L., Virginia.
† Roberts, Dr. George C. M., Maryland.
*Rogers, Prof. Henry D., Glasgow, Scotland.

Sands, Samuel, Baltimore, Maryland.
† Sangston, Lawrence, Baltimore, Maryland.
Schaff, Rev. Philip, Mercersburg, Penn.
*Slack, J. H., Philadelphia, Penn.
Staley, Rev. George L., Mount Washington, Maryland.
‡ Stuart, Hon. A. H. H., Staunton, Va.
† Swann, Hon. Thomas, Baltimore, Md.

*Taylor, W. I., Worcester Co., Md.
† Thompson, Hon. George W., Wheeling, Virginia.
Tilghman, Gen. Tench, Baltimore, Md.
Tuttle, David K., Charlottesville, Va.
Tyson, Phillip T., Baltimore, Maryland.

Uhler, P. R., Baltimore, Maryland.

Van Benschoten, James C., Oxford, Chenango Co., New York.
*Volck, Dr. Adolbert, Baltimore, Md.

*Warren, G. K., Top. Engineers, U. S. A.
Watson, William, Cambridge, Mass.
Wethered, Charles E., Baltimore, Md.
† White, Dr. E. H.
*Whitney, Prof. W. D., New Haven, Conn.
Williams, Thomas H., Portsmouth, Virginia.
Williams, William W., Keokuk, Iowa.
Wilson, Prof. W. C., Carlisle, Penn.
‡ Williamson, Capt. R. S., San Francisco, California.
† Wright, Prof. R. N., Baltimore, Md.

Young, William H., Baltimore, Md.

The following paid the assessment and signed the Constitution, but were not formally elected.

Austen, P. H., Baltimore, Maryland.

Corse, James M., Philadelphia, Penn.

De Hass, Wills, Marshall Court House, Virginia.

Field, Hickson W., New York, N. Y.

Hamilton, Dr. Thomas, Rochester, New York.
Hayden, Dr. F. V., Washington, D. C.

Lamborn, Robert H., Trenton, New Jersey.

Monnier, Alfred, Camden, New Jersey.

† Severson, B., Baltimore, Maryland.
Stimpson, William, Washington, D. C.

Tiffany, Rev. O. H., Baltimore, Md.

Williams, Charles P., Philadelphia, Pa.

The following signed the Constitution, but was not formally elected, and did not pay the assessment.

Kohl, T. G., Washington, District of Columbia.

The following have voluntarily withdrawn from the Association, or declined their election.

William C. Bond.
Edward Daniels.
A. O. Dayton.
Thomas Ewbank.
Edward C. Herrick.
W. F. Hopkins.
Alfred Mordecai.
Rufus P. Stebbins.
A. H. H. Stuart.

PROCEEDINGS

OF THE

BALTIMORE MEETING, 1858.

COMMUNICATIONS.

A. MATHEMATICS AND PHYSICS.

I. MATHEMATICS.

1. Further Remarks upon Systems of Coördinates. By Rev. Thomas Hill, of Waltham.

At the Cambridge and the Montreal meetings of the Association, I made some remarks upon systems of coördinates in a plane, and mentioned the existence of twenty-two systems (Proc. Am. As. Ad. Sc., 11th Meeting, p. 43), and I now wish to make a few remarks on each.

1. Of the usual Cartesian coördinates I need say little. It is easy to demonstrate that every equation $y = F.x$ represents a curve,— and it is natural to infer that every curve formed by a law can be represented by $y = F.x$. Yet we find that in many very simple curves, the form of the function $F.$ in this equation becomes far too complicated to be easily, or usefully, discussed. The comical advice of the Clouds,—

> *Ταχὺς δ’, ὅταν εἰς ἄπορον πέσης,*
> *Ἐπ’ ἄλλο πήδα νόημα φρενός,* —

is to the geometer sound and sensible; — "quickly, when you find the problem insoluble, fly to some other problem," — that is, attack it in an entirely different way. In attempting to treat spirals by means of rectangular coördinates, one would be immediately driven to resort to polar equations.

2. It is also easy to show that $r = F . \varphi$ will always represent a curve, and it is natural to infer that every curve could be defined by $r = F . \varphi$. But there are simple curves and problems of curvature which defy both polar and rectangular coördinates.

3. Professor Peirce has, in all his writings for more than twenty years, been accustomed to represent certain curves by defining the radius of curvature, ϱ, in terms of its direction, ν; that is, by putting $\varrho = F . \nu$. I gave many examples of these expressions, at the Cambridge meeting, and afterwards (Gould's Astr. Journ. Vol. II. p. 84) showed that the curves included in the equation $\varrho = A \sin^n \nu$ includes a great variety of curves, such as the catenary, parabola, cycloid, and other curves, not usually shown to have any geometrical properties in common.

4. An analogous system of coördinates gives what I am told is called by Whewell the intrinsic equation of a curve. It consists in putting the direction of a curve equal to some function of the arc; or $\tau = F . s$. The differential of this equation leads us at once into Peirce's circular coördinates, so that an equation in the third system will express the curves expressed in this fourth system.

5. A system which has been partially investigated by James Edward Oliver, and by myself, and which I hold in high esteem, consists in putting $\varrho = F . s$. The differential of this equation relatively to ν will give us the means of determining its evolute in the same system.

An interesting point in this fifth system is the ease with which a curve expressed in it may be produced by the deformation or metamorphoses of a curve in rectangular equations. The curve, for example, $\varrho = F . s$ may be produced from the curve $y = F . x$, by conceiving the ordinates of the latter curve to be fastened at right angles to the axis of x, and the axis of x to be afterwards curved until the ordinates become tangents at the other end to a new curve thus generated. The ordinates thus become radii of curvature to the axis, and the axis becomes the curve s. This can be done, rudely, by drawing $y = F . x$ upon a card, bending it on the axis of x to a right angle, cutting it on the ordinates into strips down to the axis, and then laying these strips over each other so as to make the points of the curve become points of tangency to the evolute of the new curve $\varrho = F.s$. Thus the straight line $y = Ax$ will give birth to a logarithmic spiral $\varrho = As$; the parabola $y^2 = 4Px$ will give the involute of a circle $\varrho^2 = 4Ps$; the hyperbola $xy = A$ produces beautiful volute or double finite spiral, $\varrho s = A$; the parabola $y = Ax^2$ produces the spiral $\varrho = As^2$; the ellipse $Ax^2 + By^2 = 1$, produces the epicycloids $A\varrho^2 + Bs^2 = 1$ or hypocycloids $As^2 + B\varrho^2 = 1$, which are cycloids when $A = B$. For if we put a and b for the semi axes of the ellipse, we have for the radius of the stationary circle, $R = \frac{ab^2}{a^2 - b^2}$, and for the rolling circle, $R^1 = \frac{ab}{2(a+b)}$.

6. The angle of the curve with the radius vector may be represented as a function of the polar angle, that is, we may write $\varepsilon = F.\varphi$. These curves may be readily reduced in many cases, to $r = f.\varphi$, and investigated in that form. Thus $\varepsilon = -\varphi$ is a straight line; $\varepsilon = \varphi$ a circle; $\varepsilon = -\frac{1}{2}\varphi$ a parabola; $\varepsilon = -\frac{1}{2}(\varphi - \pi)$ a cardioid; $\varepsilon = -2\varphi$ an equilateral hyperbola; $\varepsilon = 2\varphi$ a curve of the fourth degree, &c.

7. If we examine the equations $\tau = f.\varphi$ we obtain similar results. The equation $\tau = a\varphi$ is equivalent to $\varepsilon = (a-1)\varphi$. Hence $\varepsilon = b$ or $\tau = \varphi + b$ are either of them equations of a logarithmic spiral.

8. The system in which $r = F.x$ may be reduced to rectangular coördinates by putting $r = \sqrt{x^2 + y^2}$. A curve in rectangular coördinates may be deformed or metamorphosed into a curve of this system, by dropping each point of the curve, until the radius vector (the abscissa being unchanged) equals the former ordinate. The effect thus produced in changing $y = \sec x$, $y = \tan x$, $y = \log x$ into $r = \sec x$, $r = \tan x$, $r = \log x$ is singular and beautiful.

9. The system $x = F.\varphi$ is easily reduced to polar coördinates, by putting $x = r\cos\varphi$. The equation $x = a\varphi$ gives a curious system of curves in which the distance of the branches measured parallel to the axis of x is a constant $2a\pi$.

10. The system $x = F.\varepsilon$, is too difficult for my scanty algebra. I only know that $x = B\cot\varepsilon$ and $x = A\sqrt{\cot\frac{1}{2}\varepsilon}$ are two equations of an equilateral hyperbola.

11. If we put $\tau = F.x$ we have a somewhat more natural and easy system. In this scheme a parabola would be represented by $\tan\tau =$

$A\,x$; the involute of a catenary by $\cos\tau = A\,x$; a circle by $\sin\tau = A\,x$; and the equation $\tau = a\,x$ would represent a series of equal arches, of which only the alternate arches can be represented in rectangular co-ordinates.

12. The radius of curvature in terms of the ordinate or abscissa is a very interesting mode of defining curves. The curve $\varrho = F.x$ may be approximately drawn, by finite differences, as follows: Draw first the curve $y = F.x$. Next draw on the same sheet of paper a curve in which, for any abscissa, the radius of curvature in the second curve shall be equal to the ordinate of the first. The point of starting is arbitrary, and the direction of the radius of curvature at starting is arbitrary. Hence the forms of $\varrho = F.x$ are more varied than those of $y = F.x$. Thus you may deform or metamorphose the straight line $y = A\,x$ into the beautifully varied forms of $\varrho = A\,x$; the equilateral hyperbola $x\,y = A$ into the elegant and multiform elastic curve $\varrho\,x = A$.

13. The thirteenth system, in which $x = F.s$ is beyond the reach of my algebra. I have only seen that $x = a\,s^2$ is a cycloid, and $x = s$ a straight line.

14. The system in which $\varepsilon = F.r$ may be readily metamorphosed with the 11th, $\tau = F.x$. Thus the singular curve $\tau = a\,x$ (or $\varrho = A \sec\tau$) may be deformed into the spiral $\varepsilon = a\,r$, which runs asymptote to a series of concentric circles.

15. The fifteenth system $\tau = F\,r$ has proved too difficult for the limited time which I have been able to give to it, but it promises interesting results.

16. The radius of curvature may be defined as a function of the radius vector, giving the equation $\varrho = F.r$. These curves bear a general resemblance to those of the equation $\varrho = F.x$.

The equation $\varrho = Ar$ includes the logarithmic spirals. When it represents a log. spiral, we have, $\cot\varepsilon = \sqrt{A^2 - 1}$.

If then $A > 1$, and if for any assumed value of ϱ we determine the angle ε by the equation $\cot\varepsilon = \sqrt{A^2 - 1}$ we shall have a log. spiral. But if we assume a greater or a less value of ε, we shall have a double spiraloid looping near the origin, and approximating to a log. spiral as the curve recedes from the origin. The nearer to the origin is the point at which ε becomes $\frac{1}{2}\pi$ or $\frac{3}{2}\pi$, so much the smaller is the scale on which the spiraloid is drawn. But when the point at which ε takes this value is the origin itself, then the curve is a log. spiral. Hence the log. spirals may be defined as the spiraloid $\varrho = A\,r$ drawn (with A greater than unity) on an infinitely small scale. When $A = 1$ the general form of the spiraloid resembles an involute of a circle, and on reducing it to an infinitely small scale, it becomes a series of concentric circles filling up the entire plane to infinity. The curves when A is less than unity are more like those of $\varrho = A\,x$; and the curve of $\varrho = \frac{A}{r}$ resembles the elastic curve. The equilateral hyperbola may be defined in this system by $\varrho = A\,r^3$.

17. The system in which the radius vector is a function of the arc is somewhat difficult, but is interesting.

The equation $r = a\,s$ gives by differentiation a constant value to ε, and must therefore be included in the log. spirals. $r = a\,s^2 + b$ gives a series of arches included between two concentric circles. When r is made equal to a trigonometric function of s the variety of form is greater.

18. When $\varrho = F.\varphi$ is taken as the expression of the curve, it is difficult to detect its form, but so far as I have examined this system, I have felt assured that it would yield beautiful curves.

19. The nineteenth system expresses the polar angle as a function of the arc. Thus $\varphi = a\,s$ is the equation of a circle. The equation $\varphi = a\,s^2 + b\,s + c$ gives the curve, which at the Cincinnati meeting I offered to the consideration of railroad engineers as perhaps useful in laying out a track between unequal tangents. I then remarked, from too hasty an examination, that it returned to the origin after leaving. But I soon found that the returning portion is imaginary, and that φ and s are limited by the relation between their differentials. At an infinite distance from the origin the curve degenerates into a straight line composed of infinitesimal cycloids. Professor Peirce has been led from an investigation of this curve into the invention of a new system of coördinates, much more general than any mentioned in this paper, and will himself explain them at the present meeting.

20. The equation $r = a\,\varepsilon$ is equivalent to $r = \frac{a}{a-1}\,\varphi$, or to $\varepsilon = \frac{1}{a-1}\,\varphi$, which have been already noticed.

21. The system in which $\varrho = F.\varepsilon$ is too difficult for me, and I have had time only for the briefest examination.

22. Of the twenty-second system, $\varepsilon = F . s$, I must also confess myself as yet ignorant.

Additional systems may be made by introducing the tangent, subtangent, normal, subnormal, perpendicular let fall from the origin upon the tangent or normal, &c. &c., as variables.

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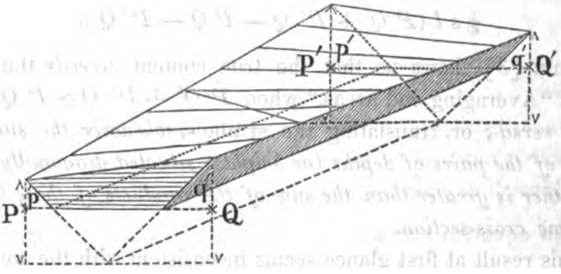
2. ON THE CALCULATION OF THE EARTHWORK OF ROADS ON SIDELONG AND IRREGULAR GROUND.

By Prof. W. M. GILLESPIE, Schenectady, N. Y.

IN a paper read at the last meeting of this association, the writer examined the nature of the warped surfaces which usually form the upper or lower faces of the solids removed or added in road excavations and embankments, and demonstrated that the volumes of these solids could be determined with perfect precision by the familiar prismoidal formula.*

It is now proposed to compare these results with those given by the methods in common use, and to establish rules by which the nature and the amount of the errors which these latter involve can be determined in advance.

A type of the solids in question is shown in the figure, which represents a road excavation in perspective. Inverted, it will equally represent an embankment.



To simplify the investigation, we will conceive the side slopes to be prolonged till they meet, as shown in the figure by broken lines. The conclusions at which we may arrive respecting the new solid thus produced, will apply equally well to the original one; since the triangular prism which we imagine added is common to both the solids discussed, whatever hypothesis we may adopt respecting their upper surfaces.

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* Proceedings of the Amer. Assoc., Eleventh meeting, page 45.

We will designate the original depths or heights (which terms we will use indifferently) of the solid at its angles, by p, q, p', q', and the new depths by P, Q, P', Q'. These last are obtained by simply adding to the former the quotient obtained by dividing the base of the excavation by twice the ratio of the side-slopes, usually $1\frac{1}{2}$ or 2.

The end areas of the new solid will be respectively $s\,P\,Q$ and $s\,P'\,Q'$,* and the true content, on the hypothesis of the former paper, will be

$$[1] \qquad \tfrac{1}{6}\,s\,l\,(2\,P\,Q + 2\,P'\,Q' + P\,Q' + P'\,Q).$$

We are now prepared to examine the usual methods of calculating contents.

I. The most common method is that of "*Averaging end areas.*" It considers the content of the solid in question to equal the product of the arithmetical mean of the two end areas by the perpendicular distance between them. Using the same symbols as before, the volume, by this method, is

$$[2] \qquad \tfrac{1}{2}\,s\,l\,(P\,Q + P'\,Q').$$

The excess, if any, of the true content above this is obtained by subtracting [2] from [1]. After a little reduction it becomes

$$[3] \qquad \tfrac{1}{6}\,s\,l\,(P\,Q' + P'\,Q - P\,Q - P'\,Q').$$

Formula [3] shows us that the true content *exceeds* the content given by "Averaging end areas," when $P\,Q' + P'\,Q > P\,Q + P'\,Q'$ and *vice versâ;* or, translating the symbols, *whenever the sum of the products of the pairs of depths (or heights) situated diagonally opposite to each other is greater than the sum of the products of those belonging to the same cross-section.*

But this result at first glance seems inconsistent with the well-known fact, that the common method, now under discussion, *always* gives more than does the prismoidal method, in such cases as tapering sticks of timber, mill-hoppers, and similar figures. The answer to this is that the warped-surface-solid under consideration is not a prismoid, although the rule by which a prismoid is calculated does apply to it also. A somewhat analogous case is that of a sphere, to which the prismoidal rule also applies, as was first noticed by Mr. Ellwood Morris, C. E.

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* $\tfrac{1}{2}\,(P+Q) \times (s\,P + s\,Q) - \tfrac{1}{2}\,P \times s\,P - \tfrac{1}{2}\,Q \times s\,Q = s\,P\,Q.$

II. The second usual method is that of "*Middle areas.*" This assumes the content to be equal to the product of the area of the middle cross-section of the solid by its length.

Using the same symbols as before, the content is expressed thus: —

$$[4] \qquad \tfrac{1}{4}\, s\, l\, (P + P') + (Q + Q').$$

The excess of the true content [1] over this is then found to be

$$[5] \qquad \tfrac{1}{12}\, s\, l\, (P\,Q + P'\,Q' - P\,Q' - P'\,Q).$$

Comparing this expression with [3], we see that we have merely to *reverse* the deductions there established: and that this method will give results too small where the preceding method gave them too great, and *vice versâ.*

The absolute error however will be only half as great; the coefficient of [5] being half that of [3], one twelfth instead of one sixth.

III. The method of "*Equivalent mean heights*" (or depths) is next to be considered. This consists, as is well known to engineers, in conceiving the solid to be transformed in such a way that its top surface shall be a plane, everywhere level cross-ways at right angles to the length, and that the areas of the cross-sections at each end shall be equivalent to the original areas there. The method then assumes that the content of this new solid, which is a true prismoid, is equal to the original content of the real sidelong warped-surface-solid.

This assumption we are now prepared to examine.

We have first to obtain the equivalent mean height of each end section; i. e., the height of a trapezoidal section, level at top and equivalent in area to the original section.

This investigation will be much simplified by the same conception as before, namely, prolonging the sloping sides till they meet, and calling the new depths at the corners, P, Q, P', Q'.

The end equivalent mean heights will then be found to be respectively $\sqrt{P\,Q}$ and $\sqrt{P'\,Q'}$.*

The middle equivalent mean height is then $\frac{1}{2}\,(\sqrt{P\,Q} + \sqrt{P'\,Q'})$, and the corresponding middle area is $\frac{1}{4}\,s\,(\sqrt{P\,Q} + \sqrt{P'\,Q'})^2$. Using this middle area and the given end areas in the prismoidal rule, we find the content of the new solid to be

$$[6] \qquad \tfrac{1}{3}\,s\,l\,(P\,Q + P'\,Q' + \sqrt{P\,Q\,P'\,Q'}).$$

But the true content of the original solid is expressed by [1]. Subtracting [6] from [1] we have for the excess of the true content

$$[7] \qquad \tfrac{1}{6}\,s\,l\,(\sqrt{P\,Q'} - \sqrt{P'\,Q})^2.$$

This expression is positive for every possible value of P, Q, P', Q'; with the single exception of the case in which $P\,Q' = P'\,Q'$, when it is zero.

We therefore arrive at this conclusion: —

The true content is always greater than that given by the method of "Equivalent mean heights;" with a single exception in the case in which the products of the pairs of heights diagonally opposite to each other are equal.

A simple numerical example will illustrate the nature and amount of these errors. Let an excavation, such as that shown in the figure on page 6, be 100 feet long and eighteen wide at base, with side-slopes of one to one, and outside depths of six feet and fifteen feet at one end, and of eighteen feet and twelve feet at the other. Its true content will be 38,850 cubic feet. But the first of the methods above discussed gives 38,250 cubic feet, or 600 too little; the second gives 39,150 cubic feet, or 300 too much; and this last method gives 37,860 cubic feet, or 990 too much; more than $2\frac{1}{2}$ per cent. of the whole amount — a very serious difference in work which on many railroads is counted by millions of yards.

This method of "Equivalent mean heights" seems to have been introduced by Telford, and has since been adopted without question by most writers on the subject (the present one included) when *perfect*

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* Putting $x =$ the "Equivalent mean height" of the triangle, we have its area $= \frac{1}{2}\,x \times 2\,s\,x = s\,x^2$. Equating this with the area $s\,P\,Q$, we get $x = \sqrt{P\,Q}$. Similarly, we have $x' = \sqrt{P'\,Q'}$.

accuracy was desired, and most of the tables and diagrams for side-long ground are constructed on this hypothesis. The difficulty has been the want of any better standard with which to compare its results. But if the positions which the former paper endeavored to establish be correct, it seems that the employment of the method of "Equivalent mean heights" should be at once abandoned, since it causes such serious errors (which do not even balance each other in the long run, but always err on the same side), and since it does this, too, under the assumption of *perfect accuracy*.

II. ASTRONOMY.

1. On the Mean Distances of the Periodic Comets. By Prof. Daniel Kirkwood, of Bloomington, Indiana.

It was noticed, several years since, that comets of *known* periodicity were arranged in two groups; the *first* including the comets of Encke, De Vico, Brorsen, D'Arrest, Biela, Faye, &c., whose periods are nearly the same with those of the asteroids; the *second* including the comets of Halley, Olbers, Westphal, and some others, whose calculated periods are all nearly equal to that of Uranus. Apart from the speculation of Professor Stephen Alexander, in regard to the origin of the former group, I am not aware that any explanation of such arrangements has ever been attempted. Perhaps our data may be still insufficient to warrant the suggestion of any theory on the subject.

The following facts, however, seem to indicate the existence of a *third group, the periods of which are nearly equal to that of Jupiter*:—

1. The comet which passed its perihelion on the 28th of November, 1793, has, according to Burckhardt, a period of twelve years. Its perihelion distance is somewhat greater than that of Mars; inclination, 47°; eccentricity, 0.73: motion, *direct.*

2. Peters' comet, discovered June 26, 1846, has a period of thirteen years. Perihelion distance slightly greater than the former; inclination, 13°; eccentricity, 0.76; motion, *direct.*

3. The first comet of 1858, discovered January 4, by Mr. H. P. Tuttle, of the Cambridge Observatory, has been found to be identical with the second comet of 1790. Its period is 13½ years; inclination, 54°; eccentricity, 0.82; motion, *direct.*

There is, then, a tendency to coincidence in the cases above mentioned, between the *cometary* and *planetary* periods. Is a similar relationship to be found in regard to any other planets?

I.

1. The comet which passed its perihelion, July 12, 1683, was estimated by Clausen to have a period of 189 years; exceeding that of Neptune by about one seventh of the latter. Inclination, 84°; eccentricity, 0.98; motion, *retrograde.*

2. The period of the comet which passed its perihelion, December 10, 1825, is, according to Rumcker, 152 years, or twelve thirteenths of Neptune's period. Inclination, 33°; eccentricity, 0.956; motion, *retrograde.* It is proper to remark that Hansen assigns this comet a period of more than four thousand years.

3. The great comet of 1843 has been thought by some astronomers to be identical with one which appeared in 1668, and to have a period of 175 years; a little greater than that of Neptune. Inclination, 36°; eccentricity, 0.9998; motion, *retrograde.*

4. The first comet of 1853 has been thought to be identical with the comet of 1664, and to have a period of 189 years. Inclination, 21°; perihelion distance, 1.026; motion, *retrograde.*

5. The fourth comet of 1857, discovered by Dr. C. H. F. Peters, of the Dudley Observatory, July 25, and called by him the Olcott comet, has, according to Mr. James C. Watson, a period of 175 years. Inclination, 32°; motion, *direct.*

II.

The comet of April, 1845, has a period, according to Clausen, of thirty-three years; exceeding that of Saturn by about one ninth of the latter. Inclination, 54°; perihelion distance, 1.23; eccentricity, 0.88; motion, *direct.*

The following table includes *all* the comets of Peirce's Catalogue (American Almanac, 1847, pp. 82–92) whose assigned periods are less than two hundred years; Peirce's No. 86 being the comet of Biela. Those not found in Peirce's table are distinguished by an asterisk. All whose periods exceed two hundred years are omitted, because, 1st, their times of revolution are very uncertain; and, 2dly, we know not what planets may exist beyond the orbit of Neptune.

TABLE I.—PERIODIC COMETS WHOSE TIMES OF REVOLUTION ARE LESS THAN TWO HUNDRED YEARS.

No.	Date Per. Pass.	Incl.	Per. Dist.	Eccent.	Period.	Dir.	Name of Computer.	Remarks.
		°			y			
1	1531, Aug.	17	0.5799	0.9673	75.	R	Halley	
2	1585, Oct.	5	1.0648	0.6439	5.17	D	Laugier and Mauvais	15 ys. acc. to Hind.
3	1683, July	84	0.5534	0.9832	189.8	R	Clausen	
4	1743, Jan.	2	0.8616	0.7213	5.435	D	Clausen	Period not stated in Peirce's table. Other periods have been assigned.
5	1766, April	8	0.3990	0.8640	5.013	D	Burckhardt	
6	1770, Aug.	2	0.6745	0.7863	5.60	D	Clausen	
7	1783, Nov.	45	1.4544	0.5395	5.61	D	Burckhardt	
8	1786, Jan.	14	0.3239	0.8484	3.123	D	Encke	
9	1793, Nov.	48	1.4000	0.7347	12.126	D	Burckhardt	
10	1806, Jan.	14	0.9068	0.7458	6.737	D	Gambart	
11	1812, Sept.	74	0.7771	0.9545	70.70	D	Encke	
12	1815, April	45	1.2134	0.9331	77.34	D	Gauss	
13	1819, July	11	0.7736	0.7552	5.618	D	Encke	
14	1819, Nov.	9	0.8926	0.6867	4.810	D	Encke	
15	1825, Dec.	34	1.0458	0.9562	152.	R	Rumcker	Per. acc. to Hansen over 4,000 years.
16	1843, Feb.	36	0.0056	0.9998	189.†	R	Gould	
17	1843, Oct. (Faye)	11	1.6921	0.5501	7.293	D	Petersen	
18	1844, Sept. (De Vico)	3	1.1862	0.6172	5.459	D	Nicolai	
19	1844, Dec.	46	0.2351	0.9804	41.46	D	Hind	
20	1845, April	54	1.2335	0.8803	33.07	D	Clausen	
21	1846, Feb. (Brorsen)	31	0.6508	0.7892	5.421	D	Goujon	
22	1846, March	85	0.6637	0.9622	73.715	D	Peirce	
23*	1846, June	31	1.5	0.75	13.	D		
24*	1847, Sept.	19	0.4879	0.9726	74.97	D	D'Arrest	
25*	1851, July	14	1.1740	0.6609	6.641	D	Villarceaux	
26*	1852, Oct.	41	1.2510	0.9248	68.	D		
27*	1853, March	20	1.0913		189.	R	Tuttle	
28*	1857, Aug.	33	0.7459	0.9764	175.	D	Watson	
29*	1858, Jan.	54	1.0260	0.8231	13.6	D	Watson	

On comparing the periods in this table with those of the planets, we find that No. 19 is the only exception to an approximative coincidence. They may be arranged in their respective groups as follows:—

TABLE II.—ASTEROIDAL COMETS.

No.	Inc.	Per. Dist.	Eccent.	Period.	Dir.	Name of Computer.	Remarks.
	°			y.			
1	14	0.3239	0.8484	3.123	D	Encke	Encke's.
2	9	0.8926	0.6867	4.810	D	Encke	
3	8	0.3990	0.8640	5.013	D	Burckhardt	
4	5	1.0648	0.6439	5.17	D	Laugier	
5	31	0.6508	0.7892	5.421	D	Goujou	Brorsen's.
6	2	0.8616	0.7213	5.435	D	Clausen	
7	3	1.1862	0.6172	5.459	D	Nicolai	De Vico's.
8	2	0.6745	0.7863	5.60	D	Clausen	
9	45	1.4544	0.5395	5.61	D	Burckhardt	Pigott's.
10	11	0.7736	0.7552	5.618	D	Encke	
11	14	1.1740	0.6609	6.641	D	Villarceaux	D'Arrest's.
12	14	0.9068	0.7458	6.737	D	Gambart	Biela's.
13	11	1.6921	0.5501	7.293	D	Petersen	Faye's.

TABLE III.—JOVIAN COMETS.

No.	Inc.	Per. Dist.	Eccent.	Period.	Dir.	Name of Computer.	Remarks.
	°			y.			
1	48	1.4000	0.7347	12.126	D	Burckhardt	
2	31	1.5	0.75	13.	D	Peters	
3	54	1.0260	0.8231	13.6	D	Watson	

TABLE IV.—SATURNIAN COMET.

	Inc.	Per. Dist.	Eccent.	Period.	Dir.	Name of Computer.	Remarks.
	°			y.			
	54	1.2335	0.8803	33.07	D	Clausen	

TABLE V.—URANIAN COMETS.

No.	Inc.	Per. Dist.	Eccent.	Period.	Dir.	Name of Computer.	Remarks.
	°			y.			
1	17	0.5799	0.9673	75.	R	Halley	Halley's comet.
2	74	0.7771	0.9545	71.	D	Encke	Pons'.
3	45	1.2134	0.9331	77.	D	Gauss	Olbers'.
4	85	0.6637	0.9622	74.	D	Peirce	DeVico's, of 1846.
5	19	0.4879	0.9726	75.	D	D'Arrest	Brorsen's, of 1847.
6	41	1.2510	0.9248	68.	D		Westphal's, of 1852.

TABLE VI. — NEPTUNIAN COMETS.

No.	Inc.	Per. Dist.	Eccent.	Period.	Dir.	Name of Computer.	Remarks.
	°			y.			
1	84	0.5534	0.9832	189.8	R	Clausen	Comet of 1683.
2	34	1.0458	0.9562	152.	R	Rumcker	Comet of Dec., 1825.
3	36	0.0056	0.9998	189.	R	Gould	Great comet of 1843.
4	20	1.0913	?	189. ?	R	Tuttle	Comet of March, 1853.
5	33	0.7459	0.9764	175.	D	Watson	Comet of Aug., 1857.

Of the twenty-nine periodic comets, therefore, whose times of revolution are less than two hundred years, the mean distances of twenty-eight are nearly equal to those of the planets; No. 19 being the only exception. It is worthy of note, also, that while the only instances of retrograde motion occur in the Uranian and Neptunian groups, Uranus and Neptune are the only planets whose satellites move in a backward direction. It will be observed, moreover, that in general the comets of shorter periods have less eccentric orbits.

The coincidences here pointed out are merely approximative. Were they exact they could not always remain so, as some of the cometary orbits might be wholly changed by perturbations. Are they not sufficient, however, to demand consideration, and to justify the conjecture that they have resulted from a general cause?

2. ON A SUPPOSED PERSONAL EQUATION IN THE USE OF THE ZENITH TELESCOPE FOR DETERMINING LATITUDES BY TALCOTT'S METHOD. By A. D. BACHE, Superintendent U. S. Coast Survey.

[Communicated by authority of the Treasury Department.]

THE use of the Zenith Telescope to determine latitudes for the Coast Survey by Talcott's method, has been explained in a paper read before the Association at the Cincinnati meeting in 1851. The difference of zenith distance of two stars passing the meridian nearly at the same time, one north and the other south of the zenith, and at nearly the same distance from the zenith, is measured by a micrometer. The sum of the same distances is given by the star catalogue, whence the zenith distance of each star of the pair becomes known, and from its declination the latitude of the place.

Observations at Mt. Sebattis station, Maine, in 1853, seemed to show that this method was not free from personal equation, either of observer or instrument. The same twenty-three pairs of stars observed with zenith telescopes, No. 1 by Assistant J. E. Hilgard, and No. 2 by Sub-assistant J. G. Oltmanns, gave results differing by 1″.12, the probable accidental error of one value of the difference being ± 0″.77, and that of the mean ±0″.16. The probable accidental error of one observation was ± 0″.81 with the first, and ± 1″.14 with the second observer, hence for the mean of each pair, with an average number of three observations on each, the probable errors would be ± 0″.48 and ± 0″.66 respectively, and for the difference of two such means ± 0″.81, scarcely differing from the value above stated, which is derived from a comparison of the differences among themselves.

The residual difference of 1″.12 appears therefore in the light of a constant quantity, and there being thus cause to suspect that identical results might not be obtained by two observers, or by two different instruments, a plan of observation was laid out for settling the question.

At the telegraph station, Columbia, S. C., in 1854, Assistants B. A. Gould, Jr., and G. W. Dean, observed with Zenith Telescopes No. 2 and No. 5. The latitude found by Mr. Dean with both instruments was almost identically the same, being with No. 2, 33° 59′ 58″.14, and with No. 5, 33° 59′ 58″.10, with probable errors for twenty-one pairs of stars, with the two instruments respectively of ± 0″.14 and ± 0″.16. With the same instruments and from the same stars Dr. Gould obtained 33° 59′ 58″.30 ± 0″.18, and 58″.13 ± 0″.18.

The inquiry was continued at Ragged Mt. Station, Maine, in 1854, by Assistant G. W. Dean and Sub-assistant Stephen Harris, under my direction, using the same instrument and observing the same pairs of stars. The resulting latitude from observations on thirty-two pairs of stars was absolutely identical, with a probable error of ± 0″.11 for the result by either observer.

A further trial with Zenith Telescopes No. 10 and No. 2 at Mt. Harris Station, Maine, by Assistant Dean and Sub-assistant Goodfellow, under my direction, in which Mr. Dean used both instruments, and Mr. Goodfellow No. 10, gives latitudes differing very slightly. Mr. Dean obtained, using Zenith Telescope No. 10, 44° 39′ 54″.49, and by No. 2, 54″.76, while Mr. Goodfellow using No. 10 obtained 54″.83. The corresponding probable errors of the mean latitudes from thirty-three pairs of stars were ± 0″.14, 0″.13 and 0″.13.

Finally at Mt. Desert Station, Maine, a comparison was made by Sub-assistants Edward Goodfellow and Stephen Harris, using Zenith Telescope No. 5, and observing the same pairs of stars. The latitudes obtained from observations on thirty pairs of stars differed but two hundredths of a second, with probable errors of ± 0″.13, and ± 0″.14.

The investigation has thus clearly proved that there is no personal equation of observer or instrument in the use of the Zenith Telescope by Talcott's method, but that identical results can be obtained by different observers, with the same or different instruments, using the same pairs of stars for observing.

The accuracy, therefore, of the final result depends upon the accuracy of the declinations of the pairs of stars, upon the use of the proper number of pairs, and upon the care taken in determining the values of the micrometer and level divisions, and in using the instrument.

A probable error in observing of from ± 0″.25 to ± 0″.80 may be expected for one observation, according to the size and quality of the instrument and the ability of the observer. The probable error of the result by a single pair of stars as depending on the catalogue errors of the stars' places, will range from ± 0″.60 to ± 1″.00, which will give from ± 0″.11 to ± 0″.18 for the latitude as resulting from thirty pairs.

The following table contains a recapitulation of these results. The first column gives the name of the station at which the observations were made; the second, date; the third, instrument; the fourth, observer's name; the fifth, number of pairs observed; the sixth, resulting latitude; the seventh, probable error of the result; the eighth, probable error of the result by a single pair of stars, derived from a comparison of the mean latitude with the result by each pair; and the ninth, the probable accidental error of one observation, derived from a comparison of the individual results for each pair of stars with their respective means.

TABLE SHOWING RESULTS OF OBSERVATIONS FOR PERSONAL EQUATION IN DETERMINING LATITUDES BY TALCOTT'S METHOD.

Station.	Date.	Zenith Telescope.	Observer.	No. of pairs of Stars.	Latitude.	Prob. error of resulting latitude.	Prob. error of result by single pair.	Prob. accidental error of one obs'n.
					° ′ ″	±	±	±
Mt. Sebattis	June, 1853	No. 1	J. E. Hilgard	23	44 08 37.82	0.16	0.90	0.81
		2	J. G. Oltmanns	23	38.94	0.22	1.08	1.14
Columbia	Feb. 1854	No. 2	G. W. Dean	21	33 59 58.14	0.14	0.62	0.36
		5	" "	21	58.10	0.16	0.75	0.25
		2	B. A. Gould	21	58.30	0.18	0.83	0.
		5	" "	21	58.13	0.18	0.83	0.
Ragged Mt.	Oct. 1854	No. 5	G. W. Dean	32	44 12 42.79	0.11	0.62	0.25
		5	Stephen Harris	32	42.79	0.11	0.65	0.47
Mt. Harris	Aug. 1855	No. 10	G. W. Dean	33	44 39 54.59	0.14	0.79	0.29
		2	" "	33	54.76	0.13	0.73	0.37
		10	E. Goodfellow	33	54.83	0.13	0.74	0.44
Mt. Desert	Aug. 1856	No. 5	E. Goodfellow	30	44 21 06.37	0.13	0.71	0.41
		5	S. Harris	30	06.39	0.14	0.76	0.46

III.—PHYSICS.

1. ON THE PENDULUM. By F. A. P. BARNARD, President of the University of Mississippi. WITH A DESCRIPTION OF AN ELECTRIC CLOCK, constructed by E. S. RITCHIE, of Boston, for the University of Mississippi, under the direction of President BARNARD.

THE importance of the pendulum as an instrument for the measurement of time, is sufficient to justify any amount of effort which may be made to secure the regularity of its performance. The causes which disturb this regularity exist partly in the nature of things, and are partly introduced by the contrivances employed to maintain the motion of the instrument. Among these, the effect of varying temperature in altering the distance between the centre of oscillation and the point of suspension, is one which has given occasion to many ingenious inventions; yet, however effectual some of these may have been in removing the irregularity due to this cause, it is probably true that no plan of compensation has been found in practice to be entirely satisfactory. It is an opinion entertained by the writer, though it is proposed with some diffidence, that the problem of compensation cannot be experimentally studied with results to be perfectly relied upon, so long as the pendulum has any *work to do;* and this must be the case whenever the maintaining power is derived, directly or indirectly, from a train of wheel-work. The different forms of anchor or pallet escapement involve friction upon the pallets, which, however nearly constant it may be, cannot be wholly so, and however slight it may be, either absolutely or in its variations, cannot be altogether insensible as a disturbing cause. For small as may be the amount of fluctuation in this resistance, it is to be considered that all the quantities concerned in the question of maintaining the motion of the pendulum are small, and that every minute variation is multiplied thousands of times. But a more serious cause of irregularity in the pendulum directly driven by a train, is to be looked for in the varying condition of the train itself, and of its lubricants; in consequence of which the power of the prime mover is to some extent absorbed, and is at different times unequally communicated to the pendulum.

When we attempt to study, in the actual performance of the pendulum, the efficacy of any plan of compensating the effects of temperature, it is impossible entirely to distinguish the irregularities due to one cause from those which may proceed from another. Cold, for instance, by stiffening the lubricants may cause the clock to gain, and this effect may be erroneously ascribed to an under-compensation of the contraction of the pendulum-rod. And though, in such a case, if the clock should run more slowly than in warmer weather, we might justly infer an over-compensation to exist, we should be unable to determine exactly the excess.

The partial or total failure in practice of plans of compensation theoretically perfect, has been sometimes attributed to the unequal rapidity with which changes of temperature take place in different parts of the same pendulum. Every plan of compensation is necessarily founded on the supposition that, under all alterations of temperature, the temperature throughout the entire instrument is simultaneously the same. It is easy to see, for example, that if the mercury employed to compensate the pendulum of a common astronomical clock were to be wholly inclosed in a cylinder of some material entirely impervious to heat (supposing such a material to exist), it would be altogether useless for the purpose intended. And that which would thus be true, on the supposition that the mercury could not change its temperature at all, must be measurably so, if its changes of temperature lag behind those of the rod. Glass jars for containing the mercury in pendulums of this construction have been objected to, on the ground that they do introduce an irregularity of this sort; and accordingly Mr. Dent, the distinguished practical horologist of London, introduced in place of them cylinders of iron. The objection has been founded, I believe, rather upon the observed performance of the pendulums, than upon actual observation of the relative temperatures of the mercury and the rod. It would seem not to be difficult to arrange a pendulum with thermometers which should show constantly the true temperature both of the mercury and of the rod. And considering the importance of the question involved, such an experiment would appear to be well worth making, before pronouncing the mercurial compensation to be unsatisfactory, or even condemning the glass cylinders.

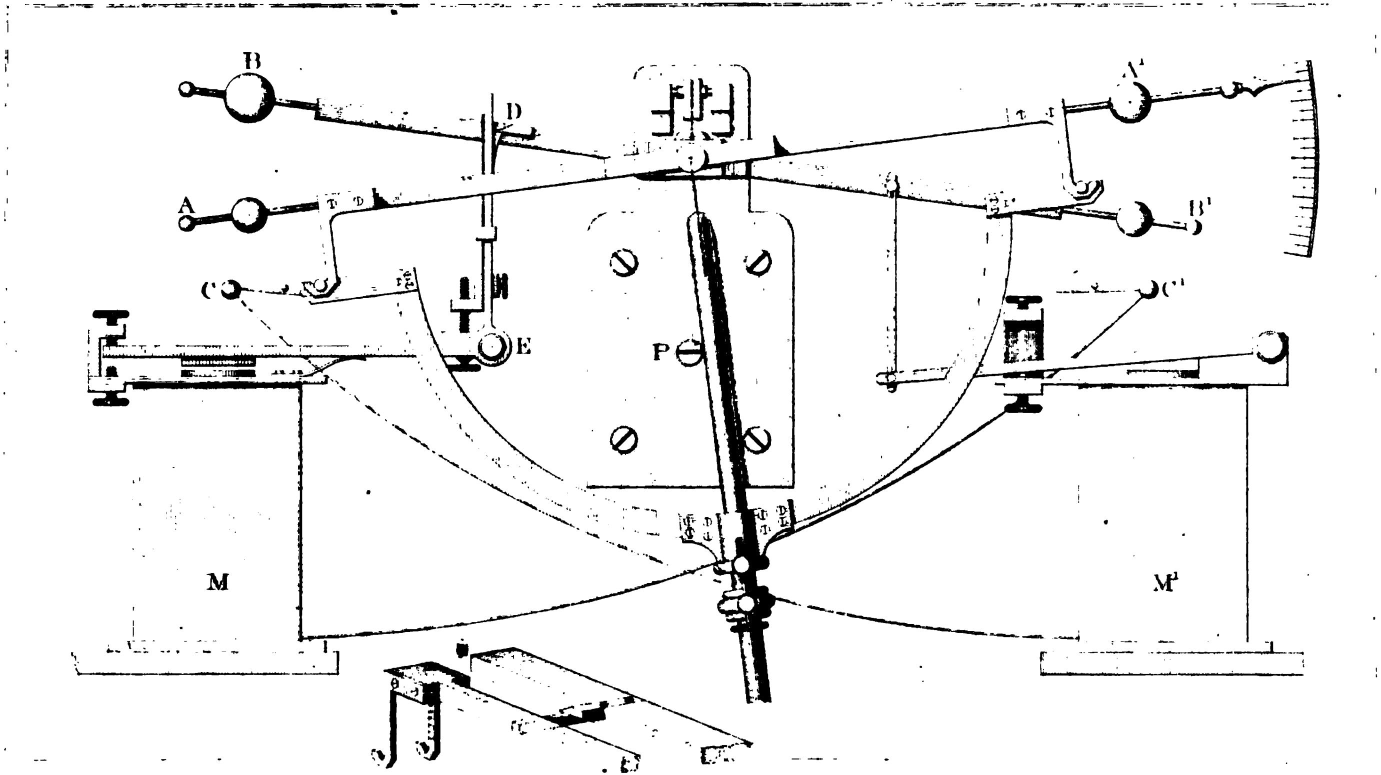
The escapements called remontoirs apparently set the pendulum free from most of the liabilities to disturbance which the train introduces. In clocks provided with these escapements, the force of the train itself is exerted not in impelling the pendulum, but in raising a small weight, or bending a slight spring, which subsequently acts — the former in its descent or the latter in its recoil — in moving the pendulum. The gravity-remontoir apparently furnishes an impelling force which is perfectly uniform, there being nothing but the very slight friction on the pivots of the arms carrying the remontoir weights to disturb this uniformity. The spring-remontoir is free from even this source of disturbance, but is open to a more serious objection, in consequence of the varying elasticity of the spring occasioned by change of temperature. Both of these contrivances, however, impose a certain duty on the pendulum, which is, to unlock the train at the moments when it is necessary that the remontoir motor should be raised. This cannot be done without friction, although the friction is less than in the case of pallet escapements. It is also true that, in proportion as the friction required to unlock is reduced, these escapements become liable to the accident of failing to lock — or of *tripping*, as it is technically called — whereby error is introduced into the time shown upon the dial.

Of all these escapements, remontoir escapements and pallet escapements alike, it may finally be said that they require the pendulum to swing through a certain arc larger than would be necessary if the maintaining power could be applied to it from without, leaving it subject to no disturbances whatever beyond those occasioned by the varying temperature and density of the air. The usual extent of the arc of vibration is 2° on each side of the vertical. About one quarter of this distance is required merely to unlock the train of the remontoir. If now, at a definite point of the swing, a light weight could be deposited upon an arm projecting from the pendulum rod, allowed to remain there during the descent, and then removed, and if this could be repeated on one side and on the other alternately, with perfect regularity and at precisely the same distance always from the centre of motion, and if this could be done without any friction or concussion, we should have a pendulum subject to no forces accelerating or retarding but such as may be accurately estimated in their amount, and in their effects upon the time of vibration.

I do not overlook the fact, that the manner of suspending the pendulum may have some influence on its performance. But as the suspension is almost invariably by means of a very flexible but also very elastic spring, the effect due to the resistance of this spring in the ascent may be considered as neutralized, so far as regularity of vibration is concerned, by its recoil in the descent; and for the variations of its elasticity with change of temperature, a special compensation may be made.

The problem which is here proposed, seems to present a condition difficult to be fulfilled. The impulse is to come at the proper moment, but the pendulum is to do no work in order to induce it. Yet it can be nothing but the pendulum itself which is to determine the moment of application; since if we possessed any independent mechanism sufficiently regular to do this, we should have no need of the pendulum. It is believed that the difficulty which this consideration presents has been overcome.

DIAGRAM OF AN ELECTRICAL CLOCK DESIGNED BY PROF F A P BARNARD



The clock which is herewith presented for the examination of the Association is one in which electricity is made to work a remontoir apparatus, by which very slight weights are made to impel the pendulum, alternately, on either side. There is nothing new in the idea of an electric clock; but there is something sufficiently novel in a clock in which the pendulum does absolutely no work at all (not even in making battery connections), to deserve attention. There have probably been as many varieties of electric clocks as of escapement clocks — conceived, at least, if not constructed. All of these known to the writer, involve as much friction as the dead-beat escapement — most of them a great deal more — or else are otherwise objectionable. That which seems to be least so, is a contrivance described in the third volume of Becquerel's Electricity, and attributed to Mr. Vérité, in which two light balls are suspended by metallic threads to a horizontal lever oscillating on pivots placed just above the point of suspension of the pendulum. Two arms from the pendulum alternately touch these balls, closing a battery circuit by the contact. The corresponding end of the oscillating lever is thereupon depressed, relaxing the suspending thread, so that the ball presses upon the pendulum arm until the latter is carried by the swing out of its reach; when, the circuit being broken, the lever rights itself again. Ingenious as this arrangement is, the objections to it are too obvious to require enumeration. It furnishes a force which fails in the three essential requisites, — perfect uniformity in quantity, uniformity in duration, and uniformity in the point of application. The suspending thread, however flexible, must interfere with the first of these conditions, and the agitation produced by the sudden tilting of the lever must affect the other two.

The construction of the clock herewith exhibited, may be explained by reference to the accompanying diagram, which shows the upper portion of the pendulum rod with the contrivance employed to apply the impelling weight. Two levers are represented, marked $A\ A'$ and $B\ B'$.

The first of these, from its office, is called the remontoir lever — the second, which is employed to control the first, is called the governor. The former is provided at each of its extremities with two hooks (as shown in perspective at the bottom of the diagram). These hooks are designed to carry a very light weight, formed of an exceedingly thin disk of metal (designed to make it conspicuous), having a small metallic axis passing through its centre. This axis rests on the hooks, and is slightly indented in the lathe at the points on which it rests, to prevent liability to displacement.

The pendulum-rod is furnished with two semicircular arms, to which, at their extremities, are attached two parallel plates of metal which pass between the hooks of the remontoir lever, and receive the weights, at the proper moments, from the hooks, being also slightly notched, or indented, to secure uniformity in regard to the point of application of the force.

Both the levers are pivoted in agates, their pivots being in the same horizontal line as the centre of motion of the pendulum. They are also provided with adjustable weights at their extremities, designed to make them tilt slowly in one direction or the other, as may be necessary in order to apply the impulse on either side of the pendulum at the proper moment.

The remontoir lever, when free from control by the other, tends to preponderate toward the right. The governor has a sufficient preponderance in the opposite direction to carry the remontoir with it, when in action, by means of a projection seen at *D*, which overlaps a corresponding projection on the other, as shown in perspective at the foot of the diagram. This lever in the position exhibited, is caught by a detent at *D*, and the remontoir lever is free to tilt toward the right, in doing which, it will carry the impulse weight of the extremity *A* along with it, while it will leave that at the extremity *A′* on the higher arm of the pendulum. In its swing toward the left, the pendulum will then be impelled by this small weight until it reaches a corresponding inclination on that side, when it will deposit the weight *A′* upon the hooks of the remontoir, and will take up the other upon the opposite arm.

This mechanism is so simple as to require no further explanation. It only remains to point out in what manner the levers are controlled by the electric battery.

The remontoir lever is insulated by the agates in which it turns. By a tangent spring at its axis it is put into the battery circuit. It has no other contact with the mechanism except where it is acted on by the governor; at which point insulation is also effected; and likewise through the springs shown at *C* and *C′*, upon which the hooks remotest from the observer alternately rest. These springs are fixed in insulated pins to which are soldered the extremities of the enveloping wires of the magnets *M* and *M'*. The pin *C* is connected with the magnet *M'*, and the pin *C'* with the magnet *M*. The continuation of these wires beyond the magnets is not shown; but they are united into a single one, which, after enveloping a third magnet in the time-register (of which no drawing is given), returns to the battery.

From what has thus far been said, it would appear that the moment the hook of the remontoir lever touches either of the springs, *C* or *C'*, the battery circuit would be complete. But this is not the case: for the hook in contact with the spring is insulated from the lever, and the circuit is only completed at the moment when the pendulum, in its swing, deposits the impulse weight upon the two hooks.

In the position shown, the weight *A* is supposed to have just been deposited. The magnet *M'* has acted and has raised the extremity *B* of the governor to the detent, *D*. The remontoir lever will now slowly tilt, the gentle motion being necessary to prevent the impulse weight from being thrown off; and the balance weights being so adjusted as to secure the necessary change of position within the second. A stop prevents the spring *C* from following the hook as it rises.

When the impulse weight *A'* is deposited on the hooks, it is the magnet *M* which acts; and the effect of this, through the bent lever pivoted at *E*, is to release the governor, which, by its preponderant weight, will cause the remontoir to tilt again.

Whenever either magnet acts, the magnet of the time-register simultaneously acts, and advances the second-hand one division on the dial.

The remaining parts of the mechanism it is hardly necessary to describe. Adjusting screws are provided to secure the exact position of the pendulum arms, and to cause the impulse to be precisely equal in duration on opposite sides. In adjusting for this latter purpose, the graduated arc on the right, and the index attached to the remontoir lever, are employed. The manner of making the adjustment is ob vious.

A pendulum impelled in this manner is subject to the action of no forces which cannot be definitely appreciated. The impelling power is constant and known. The mean resistance of the air may be computed, and even its fluctuations may, if necessary, be taken into account. The irregularities, therefore, which cannot be ascribed to these causes, must be due to imperfect compensation.

There is a possibility that a steel pendulum rod may, to some extent, be affected by the vicinity of the electro-magnets employed in this contrivance. In order to guard against this danger, if it be one, the rod of the pendulum of the clock, here exhibited, is made of brass; and the compensation, which is mercurial, is adjusted accordingly.

The effect of the impelling and resisting forces acting upon a pendulum is to alter its rate of motion; but this circumstance is of no importance, so long as these forces are invariable, like gravity. If, however, any variation occurs, either in the impulse or in the resistance, the time of vibration will be altered. The kind of alteration which occurs, in consequence of a given change of arc, is not, nevertheless, the same, with pendulums impelled on different plans. The recoiling escapement, for instance, accelerates the rate for an increase of arc, while the dead beat retards it beyond the amount which would be due to circular motion, as compared with motion in the cycloid.

The remontoir escapement has the advantage that, though it accelerates the rate of going of the pendulum, it applies invariably the same amount of impelling force at every swing; so that if the pendulum had no work to do in the unlocking of the train, it would be subject to no disturbance of its regularity except such as may be consequent upon fluctuations of atmospheric density, and upon changes in its own temperature. In studying experimentally the subject of compensation, it would not be difficult to eliminate the effects of the first of these causes, so as to exhibit truly the merit or defect of any given mode of compensation for the expansion and contraction of the materials of the pendulum.

The remontoir escapement does not perfectly fulfil these conditions; but it is believed that the electric clock herewith presented does so completely.

This pendulum has an additional advantage over an escapement remontoir; which arises from the fact that its arc of vibration may be reduced much lower than is at all practicable with a clock driven by a train. All the errors of the pendulum, except those arising from the varying temperature of the rod, increase with the arc. It is believed that this pendulum may be run with so small a motion as to make such errors quite insensible. The degree, moreover, to which external forces affect the rate without altering the arc, is proportional to the forces themselves; and these, in the present case, must necessarily be less as the arc is less.

In order to show in what manner a pendulum of this description differs in its rate of going from one entirely free and vibrating in vacuo, we may take the ordinary differential equation of the angular motion of an oscillating body, and introduce into it terms expressing the forces which in this case are in action, besides gravity. This equation then becomes (φ being the variable arc, measured from the vertical, t the time of one vibration, g the force of gravity, l the length of the simple pendulum, m the maintaining and r the resisting forces, or rather their constant coefficients as compared with gravity)

$$\frac{d^2\varphi}{dt^2} = -\frac{g}{l}\Big(\sin\varphi - rf(\varphi) + mf'(\varphi)\Big).$$

The maintaining force, in the present case, is a weight applied at the extremity of an arm of the pendulum at the height of the centre of motion. Represent the weight by w, the length of the arm by a, and the total mass of the pendulum by M, and we have the value of m equal to $\frac{wa}{Ml}$. The function of φ on which its effect depends is obviously the cosine.

The resistance is, in this case, nothing but the atmospheric inertia, so long as the impulse lasts; after this, the maintaining weight becomes itself a resisting force, and its sign must be changed. The resistance of the atmosphere may be computed on the supposition that the velocity with which a falling body, of equal weight with the pendulum, and presenting an equal surface of resistance, ceases to be accelerated by gravity, is known. This velocity may be represented by k. Then $rf(\varphi) = \frac{v^2}{k^2}$. For the value of $f(\varphi)$, we consider the velocity to be that which a body would acquire in falling vertically through the height the pendulum has descended from the commencement of its motion, which (if α represent the limit of vibration) is $l(\cos\varphi - \cos\alpha)$. Then $v^2 = 2gl(\cos\varphi - \cos\alpha)$, and

$$rf(\varphi) = \frac{2gl}{k^2}(\cos\varphi - \cos\alpha).$$

The equation then becomes

$$\frac{d^2\varphi}{dt^2} = -\frac{g}{l}\Big(\sin\varphi - \frac{2gl}{k^2}(\cos\varphi - \cos\alpha) + \frac{wa}{Ml}\cos\varphi\Big).$$

If we suppose the impulse to continue to the distance β on the other side of zero, and integrate between the limits α and $-\beta$, we shall have (employing the simpler symbols for the sake of convenience)

$$\frac{d\varphi^2}{dt^2} = 2\frac{g}{l}\Big[\cos\varphi - \cos\alpha + (m - r)(\sin\alpha - \sin\varphi) + r\cos\alpha\,(\alpha - \varphi)\Big].$$

Replacing the trigonometrical functions by their values in terms of the arcs, and rejecting minute terms of higher orders than the second, we shall obtain, after reduction,

$$\frac{d\varphi^2}{dt^2} = \frac{g}{l}\Big[\alpha^2 + 2m\alpha - 2m\varphi - \varphi^2\Big].$$

Hence,

$$dt = \sqrt{\frac{l}{g}}\,\frac{-d\varphi}{\sqrt{\alpha^2 + 2m\alpha - 2m\varphi - \varphi^2}},$$

$$t = \sqrt{\frac{l}{g}} \times -\text{arc sin}\,\frac{\varphi + m}{\alpha + m} + C.$$

And, between the limits α and $-\beta$,

$$t = \sqrt{\frac{l}{g}}\left(\frac{\pi}{2} - \text{arc sin}\,\frac{-\beta + m}{\alpha + m}\right).$$

In like manner, if t' express the time during which the impulse weights oppose the motion,

$$t' = \sqrt{\frac{l}{g}}\left(\frac{\pi}{2} - \text{arc sin}\,\frac{\beta + m}{\alpha + m}\right).$$

Putting $\tau = t + t'$, the total time of vibration,

$$\tau = \sqrt{\frac{l}{g}}\left(\pi + \text{arc sin}\,\frac{\beta - m}{\alpha + m} - \text{arc sin}\,\frac{\beta + m}{\alpha + m}\right).$$

Developing these arcs in terms of their sines, according to the ordinary series, taking their difference, and expressing by $\Delta\tau$ the difference between τ and the time of vibration of a free pendulum,

$$\Delta\tau = -\sqrt{\frac{l}{g}}\Big[\frac{(\beta + m) - (\beta - m)}{(\alpha + m)} + \frac{1\,[(\beta + m)^3 - (\beta - m)^3]}{2.3\,(\alpha + m)^3} + \frac{1.3\,[(\beta + m)^5 - (\beta - m)^5]}{2.4.5\,(\alpha + m)^5}, \text{\&c.}\Big];$$

which becomes, if we neglect insignificant terms,

$$\varDelta\tau = -\frac{2m}{\alpha+m}\sqrt{\frac{l}{g}}\left[1+\frac{\beta^2}{2.\alpha^2}+\frac{\beta^4}{2.4\,\alpha^4}+\frac{\beta^6}{2.4.6\,\alpha^6}, \&c.\right] \quad (1)$$

If $\sqrt{\frac{l}{g}}=\frac{1}{\pi}$, as for the free pendulum beating seconds, then, putting $\varDelta T$ for the total acceleration of the clock in making the number of beats made by the seconds pendulum in a day, and calling the entire remaining value of the second member of the last equation S,

$$\varDelta T = \frac{86400\,S}{\pi}. \quad (2)$$

The terms containing r having disappeared from these expressions, it would seem that the resistance of the air does not affect the time of vibration. These terms, however, have not been eliminated, but only neglected, in consequence of being connected with powers of the arc higher than the square. By preserving them, it may be shown that the resistance of the air produces an effect which is not altogether insensible; rather, however, by consuming some of the disturbing power of m, than by its direct influence. The reason of this is, that the resistance of the air opposes gravity during the descent of the pendulum, but favors it during the ascent.

The value of the foregoing series depends upon the impelling power, and on the ratio between the arc of impulse and the arc of vibration. The necessary impelling force itself, when the ratio just mentioned is fixed, depends upon the absolute magnitude of the arc α. If we assume this arc at two degrees, which is .035 of the radius taken as unity, and make the ratio of β to $\alpha = .7071$, as recommended by Mr. Denison, in his rudimentary treatise on clock and watch work, we may compute the value of m by the following process:—

Assume the pendulum to weigh twenty pounds, which is not far from the weight of that of the clock exhibited; and suppose it to expose a resisting surface to the air of thirty square inches. A column of air of equal base and weight would be about 1,250 feet in height, and the velocity with which a fluid of this altitude would issue from a vessel in consequence of the superincumbent weight, is determined by the formula

$$v^2 = 2\,g\,h.$$

Were the pendulum therefore to move with a velocity equal to the square root of 2 × 32 × 1,250 (= 80,000), the resistance would be equal to its weight. Hence,

$$k = \sqrt{80{,}000}.^{*}$$

And $r\,(\cos\varphi - \cos\alpha) = \frac{2gl}{k^2} \times \frac{\alpha^2 - \varphi^2}{2} = \frac{gl(\alpha^2 - \varphi^2)}{80{,}000}$.

This being the coefficient of the variable resistance, the total effect of the resisting force may be found by integrating the expression,

$$\frac{gl\,(\alpha^2 - \varphi)^2}{80{,}000}\,g\,dt,$$

between the limits α and $-\alpha$. Employing the symbol r, instead of the fractional coefficient, and taking the value of dt as given above, we have, calling the total resistance, R,

$$R = \int \frac{rg}{2}(\alpha^2 - \varphi^2)\sqrt{\frac{l}{g}}\,\frac{-d\varphi}{\sqrt{\alpha^2 + 2m\alpha - 2m\varphi - \varphi^2}}.$$

Integrating between the limits α and $-\alpha$, and putting $\sqrt{\frac{l}{g}} = \frac{1}{\pi}$,

$$R = \frac{rg}{2\pi}\left[\left(\frac{\alpha^2 - 2m\alpha}{2}\right)\left(\frac{\pi}{2} + \text{arc sin}\,\frac{\alpha - m}{\alpha + m}\right) + (\alpha + 3m\sqrt{m\alpha})\right].$$

Since m is very minute in comparison with α, we may make $\frac{\alpha - m}{\alpha + m} = 1$, and also neglect $-2m\alpha$. The small positive term at the end becomes insensible, when multiplied by the general coefficient, in which k^2 is a divisor — the term itself being insignificant compared with π, with which it is connected by the sign +. The errors thus introduced, besides being insensible, are in opposite directions, and nearly balanced. The simplified expression is then,

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* This computation supposes the resisting surface to be plane. The actual value of k will vary with the form of the pendulum; and will ordinarily be considerably greater than it is here found to be. The disturbing effects upon the pendulum, deduced further on, will therefore be materially less than represented; since a less impelling force will be required to maintain the motion than the calculation exhibits. The actual value of k may be pretty nearly ascertained for bodies of regular shape, by considering the inclination of their surfaces to the direction of motion.

$$R = \frac{r g \alpha^2}{4} = \frac{g^2 l \alpha^2}{2 k^2}.$$

This resistance extends over the whole arc of vibration; but the maintaining power acts effectively only between the limits β and $-\beta$, or during the time found by integrating the expression already given for $d t$ between the same limits. Then,

$$m g (t - t') = \frac{2 m g}{\pi} \left[\frac{\beta}{\alpha} + \frac{1. \beta^3}{2. 3 \alpha^3} + \frac{1. 3 \beta^5}{2. 4. 5 \alpha^5} + \frac{1. 3. 5 \beta^7}{2. 4. 6. 7 \alpha^7}, \text{\&c.} \right] \quad (4)$$

Or, putting S for the sum of the series within the brackets,

$$m g (t - t') = \frac{2 m g S}{\pi}, \text{ which must equal } R.$$

Therefore,

$$\frac{2 m g S}{\pi} = \frac{g^2 l \alpha^2}{2 k^2}; \text{ and } 4 S k^2 m = g l \alpha^2 \pi,$$

Whence,

$$m = \frac{w a}{M l} = \frac{g l \alpha^2 \pi}{4 S k^2}; \text{ and } w = \frac{M g l^2 \alpha^2 \pi}{4 a S k^2}. \quad (5)$$

The foregoing series rapidly converges, and if $\beta = .707 \alpha$, its sum is $\frac{1}{4} \pi$. Putting a, the length of the pendulum arm, measured at right angles to the pendulum rod from the centre of motion, $=3$ inches, and employing for the other symbols, the values heretofore given, we shall obtain for m and w the numerical values,

$$m = .000001597.$$

$$w = 2.914 \text{ grains, or 3 gr. nearly.}$$

Returning to the expressions (1) and (2), with the value of m thus determined, and still employing for β the value $.707 \alpha$, the sum of the series within the brackets in (1) will be found to be 1.384. And therefore ΔT, or the daily acceleration will be,

$$\Delta T = \frac{2 \times 86400 \times .000001597 \times 1.384}{3.14159 \times .035001597} = 3.473 \text{ seconds.}$$

Whence it appears that this pendulum, in order to beat seconds, must be about three one hundredths of an inch longer than one entirely free.

In order to investigate the liability of this pendulum to change of rate, we must observe that, at a constant temperature, it is impossible that there should be a change of rate without a change of the arc of vibration; and further, that there is no cause in operation to change the arc, except variations of density in the air. In expression (5) we observe that α^2 varies as k^2; but it is evident that k^2 varies inversely as the density of the atmosphere. Or, putting D for the density,

$$\alpha^2 \propto k^2 \propto \frac{1}{D}.$$

$$2\,\alpha\,d\,\alpha \propto 2\,k\,d\,k \propto -\frac{d\,D}{D^2}.$$

Hence,

$$\frac{2\,d\,\alpha}{\alpha} \propto \frac{2\,dk}{k} \propto -\frac{d\,D}{D}.$$

And,

$$\frac{d\,\alpha}{\alpha} \propto -\frac{d\,D}{2\,D}.$$

Putting the mean density of the air $=1$, and substituting a finite difference for $d\,D$, we shall find that the corresponding finite difference of α will be but half as great in proportion to the entire arc, as the fractional change of density. If, therefore, under a constant temperature, the mercury in the barometer rise or fall one inch, or a change of density occur equal to one thirtieth of the mean, the arc of vibration will change one sixtieth part of the whole; that is to say, if the value of α is 2°, the arc will fall off, or increase to the amount of $2'$.

To compute the effect of such a change upon the quantity $\varDelta T$, we may regard the series in equation (1) as being sensibly constant, and then, representing the whole expression, except the denominator of the coëfficient fraction, by Q, and omitting the insignificant term m from the denominator, we shall have,

$$\varDelta\,T = -\frac{Q}{\alpha}, \text{ and } d\,\varDelta\,T = \frac{Q\,d\,\alpha}{\alpha^2}.$$

Substituting $-\alpha\,\varDelta\,T$ for Q and reducing, we have,

$$d\,\varDelta\,T = \frac{\varDelta\,T\,d\,\alpha}{\alpha},$$

which, in the extreme case supposed above, gives a diminution of the daily acceleration equal to .058 sec. This change is, unfortunately, in the same direction as that of the circular error: but it is proportional to the quantity ΔT itself, which is directly as the maintaining power; which, again, as appears from equation (5), is as the square of the arc. Hence, therefore, the importance of reducing the arc of vibration, and the near approach to insensibility of the errors arising from its variations, when it is small. Were the arc only 1° on each side of the vertical, the error would be between one and two hundredths of a second per day. Were it half a degree, the clock, from this cause, would not be an entire second in error in nine months.

The chief object had in view in the construction of the electric clock herewith exhibited, has been to secure the reduction of the arc of vibration. The work having just been completed, opportunity has not yet been allowed for experimentally deciding the question how great a reduction of arc is practicable; but the principle of the mechanism exacts no larger motion than may be necessary to make and break electric contacts. The reduction of the arc α may be equally effected by either of two methods — either by reducing the impulse weights, or by shortening the duration of the impulse. When the arc is considerable, the former method appears preferable; when it is very small, there is not much to choose between the two; except that, by constantly reducing the impulse weights, they may perhaps become inconveniently small.

To return to the subject of compensation for variations of temperature, it may be observed that, while every pendulum is liable to be disturbed by the forces other than gravity acting upon it, and while these forces are not all of them subject to law, so that their effects can be exactly predicted and allowed for, it is not surprising that methods of compensation theoretically good should have failed to satisfy in practice. In the electric clock here presented, should its performance accord with expectation, and should it be found practicable to reduce the arc of vibration as far as it is at present believed to be, there will evidently be no sensible change of rate arising from any cause whatever, except expansion or contraction. If then the rate does actually change, the cause of error will be directly indicated; and the proper mode of correction may be made a subject of intelligent study.

A doubt has been already intimated above, whether the complaint made of the performance of the mercurial compensation, and of the glass jars as connected with it, is well founded. If the pendulum rod descends into the mercury, it would seem that there could be no great difference in the fluctuations of the temperature of the two metals. As the changes come from without, it will be the rod which will be most directly exposed to them; but the capacity of the mercury for heat is so much less than that of steel, that its changes take place with correspondingly greater rapidity. If the smaller bulk of the rod in proportion to its surface, be in its favor, the remedy would be to make the rod larger, or to dispose the mercury in an annular vessel. But, at any rate, it is easy to make the containing vessel of iron, according to the plan of Mr. Dent; and if this is done, and the expedient last suggested is adopted, of introducing the mercury into the annular space between two cylinders, it would seem that the mercurial compensation might be made quite perfect. As a final security against irregularities in the receiving of heat or parting with it, the entire surface both of the rod and of the containing vessel might be made uniform in character: which is done in the present clock by gilding. For a comparison of the performance of the compensation in glass and in iron, different jars are provided, which will be substituted for each other at intervals of several months. A brass cover, externally gilded, is also provided, to be placed over either the iron or the glass jar, for the purpose of observing the effect of change of external surface.

These are some of the arrangements which have been made for future use in the experimental examination of the question under consideration. They would not have been brought to the notice of the Association until after having been instrumental in securing some results, were it not for the fact that no other opportunity will occur of exhibiting the clock itself — its completion having taken place just as the Association are meeting — and the constructor being on the point of forwarding it to the University of Mississippi, where it belongs. The observatory of the university is now in progress of erection, and it will be some time before the large transit instrument which is to be provided, will be set up. It will be practicable, however, with less perfect facilities, to make some of those observations for which this clock is designed; and the conclusions to which such observations may lead will be communicated hereafter.

2. On the Electrical Phenomena observed in certain Houses in New York. By Prof. Elias Loomis, of New York.

At the meeting of the American Association for the Advancement of Science, held at New Haven in August, 1850, I gave an account of some remarkable electrical phenomena observed in certain houses in New York. That account was received at the time with some surprise; but it soon appeared that similar phenomena had been observed in a much larger number of houses than had been supposed, and every one who was at first sceptical about the facts, had an opportunity of convincing himself by personal observation. At present I am not aware that there is any person in the United States who doubts the truth of the statements which I made at New Haven in 1850, or who charges them with exaggeration.

The winter of 1856–7 I spent in Berlin, and I mentioned the leading particulars of these phenomena to some of the professors of the Berlin University. They all expressed very great surprise at my statements, and some of them took no pains to conceal their incredulity. In August, 1857, I attended the meeting of the British Association at Dublin, and made a short communication on the same subject to that Association. The facts were considered very remarkable, and I could not learn that any thing similar had ever been observed in any part of Europe. I then decided to improve the first opportunity on my return to New York, to verify my former observations, and to procure the concurrent testimony of a second witness to corroborate my statements. The house in which my principal observations in 1850 had been made, was the house of Mr. D. W. Catlin, a merchant of New York, who resides in Fourteenth street. Upon my stating the case to Mr. Catlin, he gave me full liberty to make such additional observations and experiments in his house as I might desire.

The past winter has proved unusually mild, and for many weeks it seemed doubtful whether a favorable opportunity would be presented for witnessing these electrical phenomena, as it is only in extremely cold weather that they are at all remarkable. About the middle of February, 1858, the first opportunity presented itself. On the morning of February 11th, the external thermometer stood at 12°; on the morning of the 12th it stood at 16°; on the morning of the 13th it stood at 18°. On the evening of the 12th, the thermometer then standing at 24°, I called at Mr. Catlin's in company with Professor Samuel St. John, professor of chemistry in the College of Physicians and Surgeons of this city. We found the parlors at a temperature of 70°, and the electricity was quite noticeable, although by no means so abundant as at my visit in 1850. We first measured the *length* of the spark, by employing two brass balls supported on glass rods in such a manner that the distance between the balls could be regulated at pleasure. The diameter of one ball was $\frac{2}{3}$ inch; that of the other was $\frac{8}{10}$ inch. When Mrs. C. walked briskly across the room and touched one of the balls with her finger, the other ball being in communication with the gas pipes, the spark would not pass from one ball to the other, if the distance much exceeded *one quarter* of an inch. Yet this spark was quite intense, and was accompanied by a good snap. When I skipped across the room in the same manner, I gave a bright spark, but it would not pass through a distance quite as great as that furnished by Mrs. C.

We next attempted to light the gas from the chandelier by means of the spark. One of the burners was extinguished; and when its temperature had fallen to about blood heat, Mrs. C. attempted to light the gas by a spark from her finger. Three or four of the first attempts were unsuccessful; perhaps because the gas was not flowing freely at the time; perhaps because the spark passed on one side of the gas jet. At length Mrs. C. took a pocket key in her hand, and skipping briskly across the room, gave a spark from the handle of the key which ignited the gas.

We next poured a small quantity of ether into a shallow metallic cup, and attempted to ignite it by a spark from the finger. The first attempts were unsuccessful; generally because the spark was received on the side of the dish, and did not pass through the ether. After a few trials, the ether was fired by a spark from Mrs. C.'s finger.

We next received a succession of sparks upon the knob of a Leyden jar, and I received the charge through my body. The shock was sufficient to convulse my arms somewhat unpleasantly.

We next suspended a pith ball by a silk thread, and having rubbed a stick of sealing-wax upon the carpet, electrified the pith ball. After skipping across the room, I presented my finger to the pith ball and found it violently repelled, showing that the electricity of my body was of the same kind as that of the sealing-wax, which conforms to the result which I published in 1850. It should be remarked that before commencing the preceding experiments, I exchanged my boots for a pair of dry slippers with thin soles.

On the evening of March 5, 1858, Professor St. John and myself made a second visit to the house of Mr. Catlin. The sky was free from clouds, but the stars shone somewhat dimly. The external thermometer stood at + 7°. On the morning of the 5th, the thermometer stood at + 8°; and on the morning of the 6th at + 12°. We found the temperature of Mr. Catlin's parlor 69°, and the electricity was more abundant than at our previous visit. We first measured the length of the spark by means of the same apparatus we employed in our former experiments. The balls were set at a distance of one third of an inch. I again exchanged my boots for a pair of dry slippers, and after skipping briskly across the room and touching one of the balls with my finger, the spark passed to the other ball. I succeeded in passing a spark when the distance of the balls slightly exceeded one third of an inch, and the spark from Mrs. C.'s finger passed a little further than my own; but even this would scarcely pass a distance of four tenths of an inch.

We next attempted to light the gas from a burner which was quite cool. We allowed the gas to flow for a time, in order to be sure that the atmospheric air was all expelled. Professor St. John then held his finger over the opening, and I took in my hand a brass rod terminated with a small ball. I skipped across the room and applied the ball to the burner, but the gas was *not* ignited. The spark evidently passed a little on one side of the gas-jet. A second trial failed, and evidently for the same reason. The third time I presented the ball to the burner with more caution, the spark passed through the gas, and *the gas was ignited.*

We next repeated the experiment with ether in a metallic cup. Professor St. John held the cup in his hand, and after shuffling across the room I presented the brass ball to the surface of the ether, but it was not ignited. A second time the experiment also failed; but the third time, after skipping a little more actively across the carpet, I succeeded in firing the ether.

What are the circumstances most favorable to the success of the preceding experiments?

1. A low temperature of the external air. I have never heard that any phenomena of this kind have been observed, except during that period of the year when the houses are warmed by fires; and the phenomena are most remarkable during the coldest weather. The electricity is not abundant unless the external temperature is below 32°; and it is most abundant when the thermometer sinks to the neighborhood of zero. I have never heard of a single fact at variance with this statement.

2. A high temperature of the interior of the house, accompanied by considerable dryness of the air. In a cold room, even when the external thermometer is at zero, but little electricity can be obtained by friction upon a carpet. A high temperature is needed, and the higher the temperature the better; and in those houses in which the electricity has been most abundant, a high temperature is preserved almost uninterruptedly during the winter season. In all my observations at Mr. Catlin's house, the thermometer in the parlors stood at about 70°. From all the facts which I have been able to collect, I infer that a furnace placed in the cellar, and sending its heat through flues to the parlors and other portions of the house, is more favorable to electrical excitement than any other mode of heating, whether by means of a stove, or an open grate. Perhaps the only reason for this difference is that when a furnace is used, the entire house is more sure to be kept thoroughly warm and dry. On many of the furnaces in New York, perhaps on most of them, a dish of water is placed to supply moisture to the heated air; but it is also true that in all the modern and well-built houses in New York, so far as I have observed or heard, particularly in those which are warmed by furnaces, the doors and wainscot, together with the furniture, during winter, habitually shrink and crack, indicating a very dry atmosphere.

3. A thick and substantial woollen carpet. My own observations and information lead me to the following conclusions: that without a carpet, the phenomena I have been describing are wholly unknown; that with an ordinary ingrain carpet but feeble electricity is obtained; a second carpet or a drugget spread upon the first improves its electrical qualities. A Brussels carpet is much better than an ingrain;

and a heavy velvet carpet is decidedly the best of all. The carpet unquestionably serves as an insulator, to preserve the electricity from being dissipated; and I also regard it as performing a still more important function.

4. A dry slipper, with a sole not very thick, appears to be favorable to the success of these experiments. A boot or shoe with a moist sole succeeds but poorly. Ladies are generally more successful in these experiments than gentlemen, chiefly I think from the fact that the soles of their shoes are ordinarily thin and dry. In my experiments at Mr. Catlin's house, I gave but a very feeble spark when dressed in boots; but after I had put on a pair of dry slippers, I gave a spark nearly equal to that given by Mrs. Catlin. A shoe with a thin sole is preferable to a boot with a thick sole, partly because the latter is less likely to be thoroughly dry, and therefore not so well adapted to exciting electricity, and also because, even if it were perfectly dry, its great thickness would interfere with the communication of the electricity to the body as fast as it was excited.

What is the source of the electricity observed in the preceding experiments?

In my paper published in 1850, I gave it as my opinion, that the electricity was excited by the friction of a leather shoe upon a woollen carpet. I proved by direct experiment that by such friction, electricity was actually excited; and I still believe that this is the principal, if not the only, source of the electricity. Some have thought that the electricity was due, at least in part, to the dress of the experimenter. I have satisfied myself that electricity may be excited in shuffling across a carpet, by any person, no matter what his dress may be, provided only that he have shoes with a dry leather sole. Children of all ages, boys and girls, as well as persons of mature years, excite electricity in the same way. I have never heard of an individual who had tried the experiment in a house similar to that of Mr. Catlin's, without exciting considerable electricity. No particular material or quality of dress is essential to the success of the experiment.

It is a very common impression, and one which is countenanced by my own observations, that a silk dress is favorable to the electrical excitement. A silk dress saves the electricity which is accumulated upon the person from being dissipated as fast as it is excited; but I do not think the silk dress performs any important active part in the electrical excitement. I believe it is true that ladies generally succeed better in these experiments than gentlemen; and ladies generally succeed best, when they wear a silk dress; but from the statement of Professor St. John which accompanies this paper, it appears that small boys in their ordinary woollen dress may be equally electrical; and I am inclined to think that the uniform success of ladies in these experiments is due to the fact, that they are more habitually confined to the house; and their dress, particularly the shoe, is more thoroughly dry.

Some have imagined that a silk wrapper worn next to the skin was important to the success of these experiments; but this is by no means essential, for one of the most electrical cases of which we have any knowledge, was that of a boy described in Professor St. John's communication, whose dress contained not a particle of silk, and who wore cotton next to his skin.

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3. Notice of some Electrical Phenomena witnessed in Houses in the Cities of New York, and Cleveland, Ohio. By Prof. Samuel St. John, of New York.

By invitation of Prof. Loomis, of the New York University, I accompanied him on the evening of the 12th of February, 1858, to witness some experiments on electricity, exhibited in a house in Fourteenth street in the city of New York. It was his purpose to investigate such electrical phenomena as he had observed in the same place in the winter of 1850, an account of which he read at the meeting of the American Association for the Advancement of Science, held at New Haven, August, 1850. The rooms in which the experiments were performed, have upon the floors thick velvet carpets, and the usual furniture of elegant houses; are warmed by furnaces, and are kept at a nearly uniform temperature of 70° F. The experiments were performed by the gentleman and lady of the house, and Prof. Loomis, who had drawn his boots and put on dry slippers. After walking rapidly through the parlors with a shuffling motion, very bright electrical sparks were exhibited when the hand was presented to the chandeliers or other good conductors communicating with the ground. Gas was ignited at one of the burners by a spark from a key in the hand of the lady, and sulphuric ether inflamed by the spark passing from her finger to the liquid which I held in a metallic cup in electrical connection with the earth. The spark was made to pass between two small insulated brass balls with a view to measure its length. The greatest length attained was one fourth of an inch. The spark exhibited a beautiful appearance in a darkened room, when the fingers were brought near to the wall paper, dispersing itself through the space of a foot or more, over the gilded ornaments of the paper. On the evening of the 5th of March, the coldest day of the season, the experiments were repeated in the same rooms, when a sensible increase of electrical intensity was discerned. The gas and ether were inflamed by Professor Loomis holding a brass ball in his hand; and the length of the spark attained a little more than one third of an inch.

These phenomena were similar to such as I often witnessed during the winters of 1854–5 at the Cleveland Female Seminary, located in the south-east quarter of the city of Cleveland, Ohio. The building is three stories high, of brick with a sandstone basement, and is warmed by three furnaces, supplied with the ordinary bituminous coal of South-eastern Ohio, the fires declining but not becoming extinct during the night. The temperature of the rooms varied considerably, sometimes rising above 80°, but rarely fell below 60° even during the night. The rooms, in which the electrical manifestations were conspicuous, were the parlors on the first floor above the basement. The floors of these rooms were covered with substantial Brussels carpets. The seasons when they attracted especial attention were periods of severely cold weather — the thermometer on one occasion indicating 23° below zero; the electrical excitement diminished during mild weather, and ceased entirely when it rained. The carpets upon the halls and other rooms were thinner fabrics than Brussels carpeting, or composed partly of cotton or linen; and upon these the electrical phenomena were barely discernible. In the parlors, electricity was manifested during dry cold weather at all hours of the day, but much more strikingly in the evening after the young ladies had spent an hour in recreation and dancing. On such occasions the intensity of the spark was such as readily inflamed ether and pulverized resin, and measured repeatedly one half inch, passing between insulated balls to the furnace register which was in good electrical communication with the earth. All persons remaining in the room were enabled to communicate sparks to conductors, but the longest sparks were given by two boys of the ages of nine and eleven years, after running and sliding upon the carpets; this we attributed to the friction evolved by their unconstrained freedom of motion. These boys wore dry slippers, and were clad in woollen, one of them wearing flannel next to his person, and the other cotton; the latter, who was of more vigorous constitution and active habits, giving the more vivid spark. A difference of electrical accumulation was also discernible among the young ladies, which we were inclined to ascribe to diversities in their dress, silk, woollen, cotton — the silk and woollen appearing more favorable to success. In one instance the different degrees of moisture upon the skin seemed to affect the amount of electricity communicated. These phenomena attracted the attention of all the inmates and visitors of the seminary, many persons expressing surprise, and some consternation, on receiving a shock as they entered the room, and took the hand presented to welcome them, preceded by a vivid spark. The passage of the spark over glass by bits of tinfoil disposed in letters and in the "spiral tube," together with the usual experiments on electric light, were repeatedly exhibited as a source of amusement.

Having listened to Professor Loomis's paper read before the Association at New Haven, and deeming his explanations satisfactory, I was accustomed to cite these phenomena, as exhibitions of electricity excited upon thick carpets, in a dry, insulating state of the atmosphere.

4. ON THE APPLICATION OF THE ROTATING MIRROR TO THE ANEROID BAROMETER. By JOSEPH SAXTON, of the United States Office of Weights and Measures, of Washington, D. C.

MR. SAXTON first applied a mirror movable around an axis passing through its plane for determining small differences in lengths of bars as early as 1825. He first employed it in adjusting the parts of a compensating pendulum. For this purpose the pendulum rod was suspended in a vertical tube, through the bottom of which the lower end projected water-tight and rested against the upper end of a slide which gave angular motion to the mirror placed below. A lamp being adjusted so as to throw a beam of light on the mirror and thence reflected through a lens of suitable curvature to a focus on the opposite wall, the place of the point of light on the wall being marked while the tube containing the pendulum rod was filled with pounded ice, the deviation was noted when the same tube was afterwards supplied with boiling water. The relative lengths of the bars of the pendulum were then altered until the change of temperature produced no motion in the reflected image from the mirror. In another application a fixed mirror was known to reflect part of the same line as the rotation mirror, as the ridge of the roof of a house, and the continued parallelism of the two mirrors at different temperatures was tested by the unbroken line presented by reflection.

While in London, about 1834, he exhibited an arrangement of the same kind for illustrating the expansion of short bars of different kinds of metals.

Since his connection with the coast survey he has applied the same principle in the construction of a pyrometer, with the addition of a telescope, to observe the deviations of the marks on a graduated scale for the purpose in the adjustment of the compensation of the measuring rods of the base apparatus, and also for the adjustment of the lengths of bars of standard measures.

This instrument, which exhibits distinctly an elongation which does not exceed the one hundred thousandth part of an inch, is described in the annual reports of Dr. Bache, the superintendent of the survey. It is supplied with a compensating frame for supporting different objects of which the expansion is required. Mr. Saxton has lately applied it, at the request of Capt. Meigs, in determining the relative expansion of different specimens of marble cut to the form of rectangular prisms of the same length and cross section. The same principle is evidently applicable to all cases where the measure of changes of length or of angular position is required. It was applied by Mr. Saxton in the Girard College magnetic observatory to indicate changes of magnetic dip.

The latest application which has been made of it by Mr. Saxton is to magnify the motion of the axis of the aneroid barometer. For this purpose the case of the instrument is removed and a mirror of about an inch square is attached to the first axis of motion. The aneroid thus furnished is fastened to a bracket on a wall, with the axis to which the mirror is attached placed horizontally. At the distance of about fifteen feet from the mirror, a telescope is permanently adjusted, so that the image of a divided scale placed on one side of the object-glass can be seen magnified in the mirror.

The slightest change in the pressure of the air is sufficient to produce an apparent motion of the scale as seen through the telescope. The opening and closing of a window, a gust of wind over the house, produce marked disturbances in the pressure of the atmospheric column. The value of this contrivance is of special interest in observing the changes of pressure during thunderstorms.

Mr. Saxton has frequently watched the indications of the instrument during the occurrence of these storms, and has been surprised to find that they seldom produce any great effect in the pressure of the atmosphere. The application by Gauss of a mirror to a magnetic bar to magnify angular variations was subsequent to the first use of the rotating mirror by Mr. Saxton.

5. ON THE EXTINCTION OF LIGHT REFLECTED FROM TERRESTRIAL OBJECTS. By L. W. MEECH, of Preston, Conn.

THE design of the following investigation is to generalize and represent in a single algebraical formula, the experiments made by W. S. Jacob, astronomer, in the trigonometrical survey of India. Of which experiments, the subjoined account was published in Silliman's Journal of Science, for the year 1850: —

"On commencing work," says Mr. Jacob, "with heliotropes in 1837, I soon found that for long distances it was necessary to enlarge the apertures *more* than in the simple ratio of the distances; and before the end of the first season I had formed a scale of apertures, which, when finally corrected, stood as follows: —

Aperture. Inches.	Maximum Distance. Miles.	Maximum Distance without absorption.
0.5	15	15
1.0	23	30
2.0	33	60
4.0	45	120
8.0	60	240

"At the distances stated in the middle column, the light was just visible to the naked eye in clear weather. The distinctness was much obscured by the rays passing in close proximity to any intervening object.

"The mean of the whole shows a loss of .0610 in passing through one mile of atmosphere, with the barometer at 27.0 inches; but reduced to 30.0 inches, the quantity will be .0671. Hence the loss of light in passing through a homogeneous atmosphere of 5.2 miles will be .303 or only about one per cent. less than Prof. Forbes' result. And as my air was considerably drier than his (the mean humidity being not much above .30 instead of .56), this will probably account for the difference."

Such is the account given in the words of their author of the experiments, which appear to have been made under favorable circumstances. For a first criterion of accuracy, they are found on examination to show the increasing facility of light for penetrating the successive strata of a medium, to which attention has been specially directed by the experiments of Delaroche and Melloni. For another test, their derived loss by absorption in passing through the whole atmosphere vertically .303, differs but slightly from .306, the proportion found by trials with the painted circles termed the diaphanometer of Saussure. (*Gehler's Wörterbuch*, II. 710.)

Presuming, therefore, that the experiments are valid and authentic, let us proceed to determine the physical law which they represent. Their author has referred them to the empiric law published by Prof. Forbes in the Philosophical Transactions of 1842, and which was ostensibly based upon experiments made with Herschel's actinometer. But the intensity of the light in India was estimated by the eye, a different instrument, and the results on closer examination are found to manifest a peculiar law of their own.

Thus regarding the intensities as inversely proportional to the apertures in the first preceding column, we perceive that the intensities diminish in a uniform geometric series. The corresponding distances in the second column give the differences in miles, 8, 10, 12, 15, respectively; and these, with the unimportant discrepancy of a single mile in the last, form a regular arithmetical series. Hence the following joint relation prevails through an extent of at least sixty miles of horizontal atmosphere.

As the first differences of distance increase in arithmetical progression, the intensity of light diminishes in geometrical progression.

This rate of diminution will readily be seen to be similar in species to the logarithmic law of Bouguer and others, as well as to that which connects altitudes above the earth's surface, with the natural decrease of atmospheric density. Under an analytic form, it is represented by the subjoined quadratic equation,

$$\log^2 I + C \log I = S\,C' + C'';$$

Where I denotes the intensity at the distance S from the origin, and C, C', C'' are constants to be determined from the observations; the logarithms employed may be either common or hyperbolic. As applied to the experiments in India before recited, this equation, after determination of the constants, and resolving the quadratic, is conveniently expressed as follows: —

$$\log.\, I = \left(\tfrac{9}{2} - \sqrt{S - \tfrac{11}{4}}\right) \log.\, 2.$$

To insert here the analytical derivation of this formula is perhaps unnecessary, since the reproduction of the values given by observation affords decisive evidence of its accuracy. Thus, being inversely as the apertures before stated, the *intensities* are,

From observation, 2, 1, $\frac{1}{2}$, $\frac{1}{4}$, $\frac{1}{8}$;
From the formula, 2, 1, $\frac{1}{2}$, $\frac{1}{4}$, $\frac{1}{8} \times \frac{21}{22}$.

The slight variation in the last case is quite unimportant, in connection with the perfect agreement in all the other values.

The inquiry, in conclusion, may arise whether this law is applicable to the experiments made by Melloni with the thermo-multiplier. In reply, a comparison has been instituted with the results of lamp heat, heat from incandescent platinum, and dark heat, transmitted through glass, which shows that in these cases it is not applicable. Hence the inference that the law is peculiar to the circumstances from which it has been derived, that is, to the extinction under the visual angle, of light coming from terrestrial objects.

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6. On the Australian Weapon called the Boomerang.[*]

By Professor Joseph Lovering, of Cambridge, Mass.

I shall arrange the remarks which I have to offer on the boomerang under these five heads: — 1. Its History and Description; 2. Its Shape; 3. Its Use; 4. Its Mechanical Theory; 5. Its Experimental Illustration.

1. What appears to be the Boomerang is delineated, among other weapons, &c., by Leseur and Petit, under the name of *sabre à ricochet* in Plate XXII. Fig. 6, of Peron's *Voyage de Découvertes aux Terres Australes*, published in 1807. But no description is given of it, and no allusion made to its peculiarities of shape and motion. This remarkable Australian weapon was first brought to European notice, so far as I know, by Captain Philip P. King. His description runs as follows:[†] —

"The boomerang is a very formidable weapon; it is a short, curved piece of heavy wood, and is propelled through the air by the hand in so skilful a manner, that the thrower alone knows where it will fall. It is generally thrown against the wind, and takes a rapid rotatory motion. It is used by the natives with success in killing the kangaroo, and is, I believe, more a hunting than a warlike weapon. The size varies from eighteen to thirty inches in length, and from two to three inches in breadth. The shape is that of an obtuse angle, rather than a crescent; one in my possession is twenty-six inches long, its greatest breadth is two and one half inches, one half an inch thick, and the angle formed from the centre is 140°. Boomerang is the Port Jackson term for this weapon, and may be retained for want of a more descriptive name."

In the "Picture of Australia," published anonymously in 1829, I find another description,[‡] thus: —

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[*] Sometimes spelt *Boomereng*. American Journal of Science, Vol. XXXVI. p. 164.

[†] Narrative of a Survey of the Intertropical and Western Coast of Australia. 1818–1822. Vol. I. p. 355.

[‡] Page 246.

"These people have, indeed, another missile weapon, and one which is equally curious in the structure and the use: that weapon is the boomerang, which is a piece of very hard and heavy timber, rendered more so by exposure to the fire. Its length is about twenty inches, and its greatest breadth about three. Its form is something like that of a lunette, or rather of two arms slightly curved, forming a large angle at the centre. It is formed of a single piece, however; both sides are made sharp, and they stand a better edge in consequence of their direction being obliquely across the grain of the timber. The boomerang is thrown in a direction, slanting upward, and both its form and the peculiar jerk with which it is thrown give it a whirling and whizzing motion as it passes through the air; and when thrown high, it *cants over* and hits with the force of its fall in returning. To throw this missile with certainty requires a great deal of practice; but those against whom it is directed are just as apt to run into its way as out of it, as none but the thrower knows where it is likely to fall. It is used in the chase as well as for hostile purposes."

The next extract is taken from the "Excursion into New South Wales and Western Australia," by Lieutenant Breton, and published in 1833*: —

"The boomerang is of different forms; the most common, however, are those in the plate. This is one of the most curious weapons of war ever invented, at least by a barbarous people, nor is it easy to comprehend by what law of projectiles it is made to take the singular direction that it frequently does. I have seen a native throw one so as to make it go forty or fifty yards horizontally, and not more than three or four feet from the ground. It would then suddenly dart into the air to the height of fifty or sixty yards, describe a very considerable curve, and finally fall at his feet! From numerous attempts, I am induced to think those only of a peculiar shape can be made invariably to return; as, for instance, No. 4. The others, although they may often do so, are more designed for throwing straight, and at the legs of the enemy, in which case they are made to go either horizontally or hoop-fashion; that is to say, the points or extremities alternately striking the ground. In all cases, no matter how thrown, the boomerang keeps turning with great rapidity, like a piece of wood revolving on a pivot, and with a whizzing noise. It is always made of hard wood, is thirty or forty inches in length, two and a half to three inches wide at the broadest part, and tapers away at each end nearly to a point. The concave part is from one eighth to one quarter of an inch thick, and the convex quite sharp. It is a dangerous weapon, and should be very cautiously used by those who do not understand it, as no person, save a native, can ever be certain where it will fall.

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* Page 238.

"I have thrown, or rather pretended to throw, a boomerang, formed like No. 4, at an object on the ground, and about ten yards distant. If cleverly managed, it would go within a few inches (sometimes two, perhaps three) of the object, and rise at once, vertically; or otherwise proceed many yards close to the ground, and then ascend; almost invariably describing a long curve, and frequently returning to within three or four yards of my feet. Even if it strikes the ground it will often move along hoop-fashion, and then rise to a great height, and return to the thrower. Of course, when thrown in this way, the blacks mean only to amuse themselves, for, when in earnest, they always make it proceed horizontally or vertically, and in either case it inflicts serious wounds."

Major T. L. Mitchell (Surveyor-General), who made an expedition to the rivers Darling and Murray in 1836, after observing "that the weapons used by the natives are not more remarkable and peculiar in their construction than general in their use on every shore of New Holland," describes the *boomerang* and its use in the following words: — *

"The boomerang, a thin, curved missile, can be thrown by a skilful hand, so as to rise upon the air, and thus to deviate from the ordinary path of projectiles, its crooked course being, nevertheless, equally under control. It is of the form here represented, being about two feet four inches long. These weapons are cut according to the grain, from the curved parts of acacia, or other standing trees of compact, hard wood. They usually weigh about nine and a half ounces. One side, which is the uppermost in throwing, is slightly convex, and is sometimes elaborately carved. The lower side is flat and plain. The boomerang is held, not as a sabre, but sicklewise, or concave towards the thrower; and as a rotary motion is imparted to it when sent off, the air presents so much resistance to the flat side, and so little to the sharp edge as it cuts forwards, that the long-sustained flight of the whirling missile seems independent of the common effect of gravitation.

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* Three Expeditions into the Interior of Eastern Australia, &c. Second Edition. Vol. II. p. 348.

"The native, from long practice, can do astonishing things with this weapon. He seems to determine, with great certainty, what its crooked and distant flight shall be, and how and where it is to end. Thus he frequently amuses himself in hurling the formidable weapon to astonishing heights and distances, from one spot, to which the missile returns, to fall beside him. Sometimes the earth is made a *fulcrum*, to which the boomerang descends only to resume a longer and more sustained flight, or to leap, perhaps, over a tree, and strike an object behind it.

"The contrivance probably originated in the utility of such a missile for the purpose of killing ducks, where they are very numerous, as on the interior rivers and lagoons, and where, accordingly, we find it much more in use than on the sea-coast, and better made, being often covered with good carving."

A more recent description is found in Edward John Eyre's "Journal of Expedition of Discovery in Central Australia," &c., in 1840-41:—*

"The wango (the boomerang of Eastern and the kiley of Western Australia) is another simple but destructive weapon in the hands of the native. It consists of a thin, flat, curved piece of hard wood, about two feet long, made out of the Acacia pendula or gum shrub, the raspberry-jam wood, or any other of a similar character. A branch or limb is selected which has naturally the requisite curve (an angle of 100° to 130°), and is dressed down to a proper shape and thickness, and rounded somewhat at the bend. Those whose angles are slightly obtuse are usually thrown with the sharp edge against the wind, and go circling through the air with amazing velocity and to a great height and distance, describing nearly a parabola and descending again at the foot of the person who throws them. Those which have the largest obtuse angles are thrown generally against the ground, from which they bound up to a great height and with much force. With both, the natives are able to hit distant objects with accuracy, either in hunting or in war; in the latter case, this weapon is particularly dangerous, as it is almost impossible, even when it is seen in the air, to tell which way it will go, or where descend. I once nearly had my arm broken by a wango, whilst standing within a yard of the native who threw it, and looking out purposely for it."

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* Vol. II. pp. 307, 308.

Captain Charles Wilkes, of the U. S. Antarctic Exploring Expedition, writes thus of the boomerang:—*

"But the most extraordinary weapon is the boomerang. This is a flat stick, three feet long and two inches wide by three quarters of an inch thick, curved or crooked in the centre, forming an obtuse angle. At first sight, one would conclude it was a wooden sword, very rudely and clumsily made; indeed, one of the early navigators took it for such.† It is an instrument useful both for war and in the chase. In the hands of the native, it is a missile efficient for both, and is made to describe some most extraordinary curves and movements.

"It is grasped at one end in the right hand, and is thrown sicklewise, either upwards into the air, or downwards, so as to strike the ground at some distance from the thrower. In the first case it flies with a rotary motion, as its shape would indicate; after ascending to a great height in the air, it suddenly returns in an elliptical orbit to a spot near its starting-point. The natives in its use are enabled to strike objects which lie behind others with great precision, and to reach those near, as if by a back stroke, by throwing it at a particular angle. The diagram at the end of the chapter exhibits the curves at the angles of 22°, 45°, and 65°, respectively, which I have obtained in making experiments with it. Some facts which are spoken of in its use are remarkable. On throwing it downwards on the ground, it rebounds in a straight line, pursuing a ricochet motion until it strikes the object at which it is thrown. Birds and small animals are killed with it, and it is also used in killing ducks. The most singular curve described by it is when thrown into the air above the angle of 45°: its flight is always then backwards, and the native who throws it stands with his back instead of his face to the object he is desirous of hitting. The diagram also exhibits its fall in case it loses its rotary motion. It is a favorite weapon with the natives, and is frequently seen ingeniously carved."

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* Narrative of the United States Exploring Expedition, during the years 1838–42. 1845. Vol. II. p. 191.

† Note by J. L. — That Dampier saw this weapon on the western coast, in latitude 16° 50′, is evident from the following observation: "These *swords* were afterwards found to be made of wood, and rudely shaped, *something like a cutlass.*" Mitchell, Vol. II., note to page 369. This mistake in the name may account for the omission of Cook, Perouse, and Flinders to mention the boomerang.

Robert Dawson, in "The Present State of Australia," speaks of a curious weapon of war, called *bomerang*, which has the shape of two *straight lines* forming an obtuse angle. (Page 108.) J. Lert Stokes, in the "Discoveries in Australia in the Beagle, 1837–43" (Vol. I. p. 117), describes the *kileys* of King Sound and Swan River thus: "One of the natives seen to-day had with him a kiley so different in shape to any we had previously seen, that I preserved a sketch of it. The sketch is one twenty-fourth the natural size." Hodgkinson, in his "Australia," published in 1845, *shows* the weapon in the hands of the natives, but does not describe it. The boomerang is not mentioned either by George Gray, in his "Journals of Two Expeditions of Discovery into Northwest and West Australia, 1847–49," or by Lieut.-Colonel T. L. Mitchell, in his "Journal of an Expedition into the Interior of Tropical Australia," published in 1848, or in "Rambles and Observations in New South Wales," by J. P. Townsend, published in 1849.

In 1837 some specimens of this curious Australian weapon from Swan River were sent to Mr. Joseph S. Moore. In a letter to Professor MacCullagh, read by him before the Royal Irish Academy, May 22, Mr. Moore says:* — "The natives throw them with the convex edge against the air: their movement is then from left to right. But the way in which I have succeeded was by taking the missile by one end, with the concave edge inward, and the plane side undermost, the plane making an angle of about 40° with the horizon; throwing as if to strike the ground at the distance of about thirty yards, and giving it, on leaving the hand, a rapid rotatory as well as progressive motion.

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* Proc. Roy. Irish Acad. I. p. 73.

Instead of striking the ground at which it was aimed, its plane becomes horizontal at the distance of about twenty-five yards, and so continues for about fifteen yards, when it commences rising in the air and moving towards the left; its plane then becomes inclined, and continues at an angle of from thirty to forty degrees, whilst it describes apparently a segment of a circle to the left. Having at the distance of sixty or seventy yards attained an altitude of from forty to sixty feet, the projectile returns, descending to the point from which it was projected, when, its plane becoming once more horizontal, it skims along within a few feet of the ground, and passes close by the right hand of the person who threw it. On passing, its plane becomes elevated once more, it rises a second time, and performs another smaller curve (fifteen or twenty yards behind the projector), in like manner as the first, with this singular exception, that the second curve is described from left to right, contrary to the course of its rotation, and of the first curve, which is invariably from right to left."

The discussions on the archæology and the mechanical theory of this instrument which followed in the Irish Academy, and which were copied into Poggendorff's *Annalen*, Vol. XLV. pp. 474–480, attracted popular attention to the subject. The interest in it was so great in Dublin, that the boomerang was manufactured there, and became a popular scientific toy, as the top, the kite, the kaleidoscope, or the balloon previously, and the stereoscope or the gyroscope since. At Oxford and Cambridge it came to be extensively used in gymnastic exercises. Although it is sold there in the shops as a plaything, it is not a very safe plaything in the hands of children.

2. The boomerang is popularly described as resembling, in the shape of its principal section, a sickle, though less curved; and in that of its cross-section, a plano-convex lens. Perhaps the suggestion of some similarity of form, at least in the cross-section, between it and the wing of the bird, is not wholly without significance. A more scientific description of the boomerang represents it as of a hyperbolical form (the vertex further from the handle than the middle is), from two to three feet in length, and about three inches broad, flat beneath, and slightly convex upon the upper side. The model (exhibited by me to the American Academy of Arts and Sciences, before whom this paper was also read) carries a label asserting that it was "manufactured from the original pattern, as presented to the Royal Irish Society."

This model measures two feet and ten inches in the curve from one extremity to the other, nearly one inch and three quarters in breadth, and three eighths of an inch in thickness on the central longitudinal line. Specimens of the instrument, Professor Snell informs us,* exist in the collections of the Smithsonian Institution, and in the National Gallery in the building of the Patent-Office at Washington. In the figure which Mr. Snell has given, the angle is more obtuse than in the model just mentioned, which represents perhaps the figure of an improved or ideal boomerang, rather than the savage instrument itself. The latter is often of rude construction, and varies much in shape, and in use also in different cases. Science may perhaps be able one day to teach the savage how to improve on his own instrument, in the shape, if not in the management of it. The illustrations on the next page, copied from the delineations of travellers who have seen the boomerang in the hands of the natives of Australia, and who have given us the descriptions of its shape and use already quoted, will exhibit to the reader the variety in its form; to explain a no less surprising variety in the motions of different specimens.

3. The following directions are attached to the model of the boomerang which I have. "By holding the missile at one end, the plane side undermost, the plane making an angle of 40° with the horizon, and throwing it as if to strike the ground at thirty yards distant, and giving it, on leaving the hand, a rapid rotatory as well as progressive motion, instead of striking the ground, it rises in the air horizontally for sixty feet, comes round from right to left, and falls many yards behind the projector."

Mr. Moore did not succeed with the specimens sent from Swan River, though he did with others of the same general figure, but more curved, which were manufactured for him. Poggendorff remarks: "When I spent some days this summer in Dublin, I had an opportunity to learn, by my own observation, the manipulation of the boomerang by a person well practised in throwing it, and, I must confess, to my great astonishment; for the path of the instrument, in its incessant vortices, at first shooting forward horizontally, then quickly ascending and retrograding, is so strange, and, according to the direction of the

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* Smithsonian Report for 1855, p. 178.

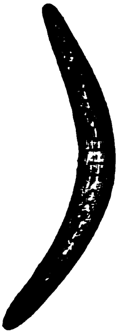
Fig. 1.

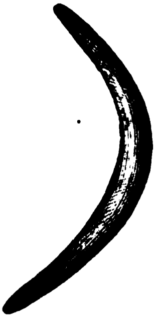
Fig. 2.

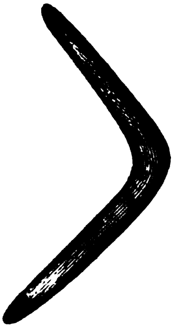
Fig. 3.

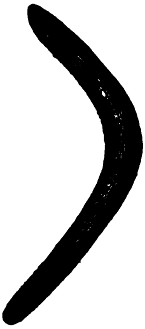
Fig. 4.

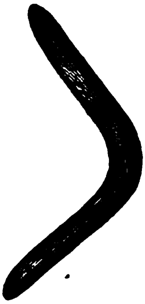
Fig. 5.

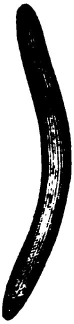
Fig. 6.

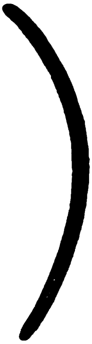
Fig. 7.

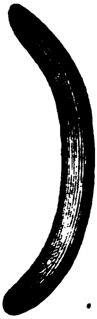
Fig. 8.

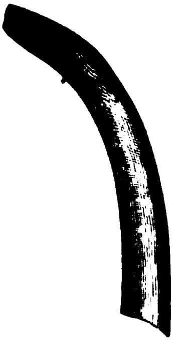
Fig. 9.

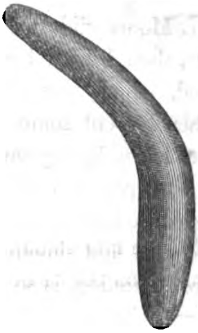
Fig. 10.

wind or other contingencies in its projection, so manifold, that no one, without seeing it, can easily form a vivid idea of it." He adds, that great experience is required to know beforehand what course it will pursue. Therefore, the Australians, he is told, use it to strike down a bird in a flock, or to throw their enemies into confusion before attacking them with the javelin, rather than to single out any one remote object, which seems almost impossible. He concludes that the boomerang is no substitute for bows and arrows, with which the Australians are not acquainted. If the Australians succeed better than Europeans with this missile, the cause must be found in superior skill in its use, and not in superior strength; for Peron states, in the *Voyage de Découvertes aux Terres Australes, pendant les Années* 1800–4,* that it was found by Regnier's dynamometer† that the physical force of the hand of the New-Hollander was 51.8 on the same scale in which that of the Frenchman amounted to 69.2 and that of the Englishman to 71.4.

The following illustration, copied from Wilkes's History of the United States Antarctic Exploring Expedition, indicates the strange and manifold flights of the boomerang, according to the manner in which it is projected. It will help the reader to understand, better than he otherwise would, the descriptions of the orbit of the missile which precede, and the dynamical explanations which follow.

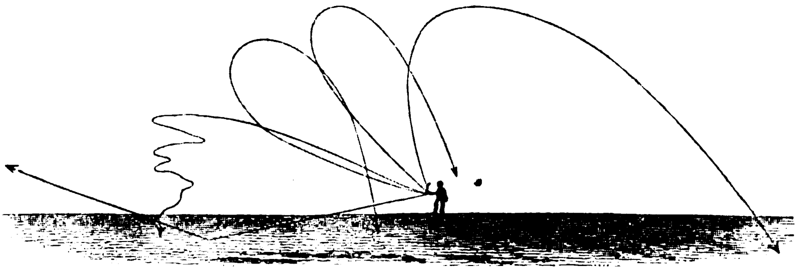

Fig. 11.

4. The late eminent mathematician, MacCullagh, made the following remarks on the theory of the boomerang, at the meeting of the Royal

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* Page 456.

† Journ. Ecole Polytech., Vol. III. p. 160.

Irish Academy on the 22d of May, 1837: * — "If a body of any form is projected *in vacuo*, we know that its centre of gravity describes a parabola in a vertical plane, while the body rotates about an axis passing through the centre of gravity. It is clear, therefore, that the deviation from the vertical plane must be ascribed to the influence of the air. To calculate the mutual action of the air and of a body to which is communicated at the same time a rotating and a progressive motion, is a problem which far transcends the present powers of mechanics. The problem can only be solved approximately, and, however much we may simplify it, the calculations still remain very laborious. Even the supposition of a resistance proportional to the square of the velocity (which is usually considered in questions of this kind as an approximation) will lead to complicated results."

At a later meeting of the Irish Academy,† a paper by Mr. Carroll was read, in which the author sought an explanation of the peculiar flight of the weapon in a resistance of the air similar to that which acts upon a flat, circular disc. This paper gave occasion to Professor H. Lloyd to observe, that the peculiar motion of this projectile was only an extraordinary case of well-known laws. When a body moves in a resisting medium, and when the resultant of all the resisting forces which act upon the single points of its surface is not contained in the vertical plane of its projection, then the body must deviate from this plane. This is generally the case when a body moves in a resisting medium. It may be shown that the influence of the air's resistance is unusually great in a body which, like the boomerang, consists of two straight arms joined at an obtuse angle, and which is thrown with a rotating motion. Hence arises the great deviation in this case, which, it is known, rises even to 180°. But this anomalous deviation is by no means peculiar to this figure; there are other forms which manifest it to a still more wonderful extent. The second peculiarity in the flight of the boomerang, namely, its alternating motion *up* and *down*, Mr. Lloyd ascribes to a *nutation* of its axis of rotation, while the instrument, on account of its flat figure, is forced to move in its own plane, which is also the plane of rotation. The *progressive* and *rotatory* motions of a heavy body in a resisting medium are not independent of each other, as they are *in vacuo*, and therefore changes in the progressive motion will produce corresponding changes in the *velocity*, as well as the *direction*, of the *rotatory* motion.

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* Pogg. Ann. XLV. pp. 476, 477, and Proc. Roy. Irish Acad., I. p. 73.

† Proceedings, I. p. 145.

A writer in the Philosophical Magazine has attempted a labored explanation of the motions of the boomerang, but neither he nor Mr. Carroll, to whom reference has already been made, places sufficient stress on the importance of the *inertia of rotation* in bringing about the singular motions under consideration. Professor E. S. Snell, of Amherst College, in his lecture before the Smithsonian Institution * on *Planetary Disturbances*, states the mechanical peculiarity of the boomerang very clearly, and assigns distinctly the true cause of its extraordinary path; but he does not wander from his principal subject, or go into any details in regard to this case; which is only introduced as an illustration. I have thought, therefore, it might not be without interest to the Association to hear some theoretical remarks upon the subject, though not presuming to exhaust the problem, the solution of which, in all its generality, so eminent a mathematician as the late Mr. MacCullagh had declared impossible. An elucidation of the motions of the boomerang, not so popular as to be insufficient, neither so analytical as to be inexpressive except to those initiated in the higher mathematics, is all that is now attempted.

The most general peculiarity of motion in the boomerang is this. Instead of describing a parabolic path, as projectiles usually do, it ascends and advances for some distance, then descends retrograding to fall at the feet of him who threw it, or, with still greater retrograde motion, to pass over his head and strike the ground behind him, sometimes leaving the original vertical plane and wheeling round to the right or to the left, passing the flanks of the experimenter. If there were no resisting medium, the path of a projectile, as determined by inertia and gravity, would be a parabola. In a resisting medium its path is not a parabola or any known curve; and when the elevation is 45°, the vertex is not in the middle, but more remote from the point of projection than from the other extremity of its orbit, and the path by which it descends is less curved than that by which it ascends; for, in ascending, the resistance diminishes greatly the horizontal motion and increases the downward motion, while in descending it diminishes both the downward and horizontal motions. Here it is supposed that the projectile is spherical, or that the resistance acts freely upon the body, turning it round so that it may present the surface of *least resistance* to the front; and offer no large oblique surface to the resistance, by which it is made to deviate to leeward. In the boomerang there is the same gravity, the same inertia of progression, the same general resistance of the air, and, in addition, the *flat figure* and the *inertia of rotation*, peculiar to this missile. When the boomerang is thrown by the hand, as the force does not pass through the centre of gravity, the missile acquires a rapid rotation in the plane of its principal section, around the short axis passing through the centre of gravity. This inertia of rotation forces the plane of rotation to preserve its parallelism during the flight of the missile, in opposition to the tendency to shift its equator, which comes from the influence of the air. How efficient this inertia of rotation is, in enabling a body to resist disturbances, is illustrated in the celebrated pendulum experiment of Foucault, and more recently in the apparently paradoxical motions of the gyroscope. If it were not for the inertia of rotation, the plane of the principal section might turn so as to remain tangential to the path of progression; which in this case, in spite of the resistance of the air, would be nearly a parabola. Again, were it not for the resistance of the air, the path of progression would be a parabola in spite of the inertia of rotation. The next question is, What will be the path of a projectile which is rapidly rotating in its own plane, and which, on account of the inertia of rotation, will present its principal surface of resistance at a nearly constant angle with the vertical.

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* Report for 1855, p. 178.

I will suppose, first, that the boomerang has reached the vertex of its path, that its projectile force is spent, and that it begins to descend vertically. How will the air act upon a descending body with its principal surface inclined by the angle (x) to the vertical? The pressure of the air is *proportional* to the frontage of the falling body, which is equal to its principal surface [S] multiplied by sin x. This pressure, being exerted obliquely against the surface, is decomposed into two; one parallel to the surface and inefficient except through friction, the other perpendicular to the surface. The latter (the only effective part of the pressure) is to the whole as sin x to 1. Hence the efficient pressure is proportional to $S \times (\sin x)^2$. Again, the efficient pressure, tending to move the body *obliquely* upwards, may be decomposed into two others: one urging the body vertically upwards, and so retarding its fall, the other pressing it *backwards*, and therefore particularly claiming our attention. The latter is to the whole effective pressure as $\cos x$ to 1. Hence the pressure directly backward is proportional to $S (\sin x)^2 \cos x$. To find the angle (x) which gives the greatest value to this backward pressure, we differentiate the formula in relation to (x), the variable, and make the differential equal to zero.

$$S\, 2 \sin x\, (\cos x)^2\, d x - S \sin x^3\, d x$$

$$2 \cos x^2 = \sin x^2$$

$$\sqrt{2} = \text{tangent of } x$$

$$x = 54^\circ\, 44'.$$

This is the case also of a windmill, if it is supposed to turn upon a vertical axis, under the impulse of a vertical current blowing upwards.* But in the case of the windmill the velocity of the wind is *constant*, in the sense, that is, of being independent of the inclination of the sails to the direction of the wind. But in the boomerang the *velocity* of the *body* determines the *velocity* of the *wind;* and the velocity of the body is proportional to the square root of the height from which it falls. Now the resistance is as the square of the velocity, and therefore is proportional to the height from which the body falls. But the height to which projectiles ascend, and from which they fall, is proportional to the cosine of the angle which the direction of the projection makes with the vertical, that is, to $\cos x$. Hence the backward pressure, which is proportional to $S (\sin x)^2 \cos x \times v^2$, becomes proportional to $S \sin x^2 \cos x^2$; in which (v) represents the velocity.

To find when this is a maximum, we make its differential equal to zero.

$$S\, 2 \sin x\, (\cos x^3)\, d x - S\, 2 \cos x \sin x^3\, d x = 0.$$

Hence,

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* It will be observed, that, after the windmill or the boomerang has begun to *move* under this pressure, the maximum angle alters. In the case of the windmill, when the velocity of the sails is double that of the wind, the maximum angle is 77°.7. The same will be true of the boomerang when the horizontal velocity is twice the descending velocity.

$$\cos x^2 = \sin x^2$$
$$\text{tangent } x = 1$$
$$x = 45^\circ.$$

So far, I have neglected the vertical component of the resistance. This component diminishes the velocity of the fall and increases the time of it, and its amount is proportional to the sine of the angle (x). Hence I infer that, with this component also taken into account, the maximum angle, instead of being exactly 45°, would be less than 45°.

The general expression of the backward resistance in this case becomes very complicated, being proportional to

$$S \sin x^2 \cos x \left[v - \frac{S \sigma v^3 \sin x^3}{2 g w}\right]^2.$$

σ being the specific gravity of the air, and (w) the weight of the boomerang. In this case it is evident that the value of (x) which would give a maximum would not be general, but would depend upon the particular values of σ, S, w, and v in the special case. Hence the solution would not be of much importance.

When I have found correctly the value of the angle (x) which produces the greatest backward pressure and consequently the greatest backward motion, it by no means follows that this is the angle of projection with which the boomerang would go to the greatest *distance behind the person who throws it.* For the vertex of the parabola is at a variable distance *in front* of the observer, depending on the product of the time of the ascent (which is as cos x) and the horizontal velocity (which is as sin x); the maximum distance *in vacuo* being when the angle of projection is 45°.* Hence in the very case in which the retrograde motion is the greatest, it begins at a point the farthest in advance. It is possible, therefore, that at some other angle than 45°, though the retrograde motion is less, still, as it begins from a less advanced vertex, it may carry the missile further behind the *point of original departure* than if the angle were exactly 45°. When the horizontal component of resistance is the greatest, it is safe to infer that, for the same body, the retrograde velocity is the greatest. But what that velocity is, and over what space it will carry the missile, and at what angle of projection this space, measured backward from the point of departure on its upward flight, will be a maximum; these questions cannot be answered without bringing into the account the weight, size, and shape of the individual instrument.

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* It is understood that the angle of projection which gives the greatest range is not the same in a resisting medium as *in vacuo*. Hutton observes: "By such a course of experiments it will be found that the greatest range, instead of being constantly that at an elevation of 45°, as in the parabolic theory, will be at all intermediate degrees between 45° and 30°; being more or less, both according to the velocity and the weight of the projectile; the smaller velocities and larger shells ranging farthest when projected almost at an elevation of 45°; while the greatest velocities, especially with the smaller shells, range farthest with an elevation of about 30°." — *Hutton's Mathematics*, II. pp. 140–141, and 475. The velocities Hutton had in view were velocities between 200 and 2,000 feet a second. The greatest velocity with which the boomerang can be thrown must be much less than the least of these; so

The semi-range of a projectile, or horizontal distance of its vertex, is equal (when V is the velocity of projection) to $\frac{V2}{g}$ sin x cos : and the problem is to ascertain at what angle the sum of all the retrograde motions, minus this semi-range, is a maximum. But it is obvious that there could be no general solution. Each instrument would furnish a special case.

Thus far I have supposed that when the boomerang began to fall it had no forward velocity left, so that the backward pressure began immediately to make it retrograde. Neglecting the slight resistance on the edge of the missile, which would diminish proportionally its *upward* and *forward* velocity, I would inquire how the horizontal velocity, in particular, is diminished, and perhaps destroyed, sometimes possibly even before the missile has reached the highest point of its flight. The backward pressure, of which I have treated, begins to take effect before the body begins to fall; even as soon as it begins to ascend. For, in consequence of gravity, the body, even when rising above the earth, is really *falling below the tangent to the first point of its orbit.* This fall of its path below the original tangent with which the principal section of the body corresponds, produces on that section a pressure from below upwards, the horizontal component of which retards the horizontal

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that its greatest ranges in a resisting medium will not differ much from 45°, on account of its small velocity and the slight front it presents in its advance, and in spite of its specific gravity being less than that of the cannon-ball. It must be considered that Hutton's angles are reckoned from the horizon, and not from the *vertical.*

velocity. It may have destroyed it at the moment the body reaches the vertex of its orbit, or not until it has passed beyond it, or possibly even before it has come to it. In these three cases its path may be illustrated by the diagrams on page 54.

It still remains to account for the alternating ascending and descending motions of the boomerang, on which so much stress is laid in some of the descriptions which I have quoted. Professor Lloyd is the only one, so far as I know, who has alluded to the cause of this, and ascribed it to a *nutation* of the axis of rotation of the boomerang. By recurring to the familiar experiments with the *gyroscope*, it will be remembered that, although the heavy rotating disc does not yield freely to gravity and drop, turning round a horizontal axis, nevertheless, the tendency to do so is combined, according to the *parallelogram* of *rotation*,* with the *inertia* of rotation, and causes the *precession*-like motion which the gyroscope exhibits around a vertical axis. So in the boomerang, the resistance of the air cannot act freely upon its flat face, and make it turn on a horizontal axis and present its edge in advance, as it would if there were no inertia of rotation. But the tendency to do so is combined with the inertia of rotation, and produces a *precession*-like motion in the axis of rotation around the vertical. In consequence of this motion around the resultant axis, even if the inclination of the principal section, or of the axis of rotation, of the boomerang to the horizon does not change, its azimuth will slowly. And this precession will, if we may argue from the analogy of the great astronomical case, be accompanied with a periodical, if not a permanent, change of inclination.

Thus far I have only considered those peculiar deviations of the boomerang which take place in a vertical plane. For I have considered the principal section of the boomerang to be inclined on a line at right angles to the vertical plane of projection; the *top*, that is, inclined *directly forward.* If this section be also inclined on a line situated in the vertical plane of projection, the *left edge*, for example, *downwards*, then new peculiarities of motion are to be noticed. The missile, after going some distance forward, wheels round to the left as it advances and retrogrades, sometimes describing 180° in azimuth. If the right edge were inclined downwards, the sweep taken by the boomerang would be to the right. If the forces resulting from this new inclination are analyzed, it will be seen that there is a new horizontal pressure, to the *left* or to the *right*, which, combined with the backward pressure, makes the body describe an orbit, more or less elliptical, according to the ratio of the two inclinations, and to the left or to the right, according to the direction of the inclination.

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* Élémens de Statique par Poinsot. 5me édit.

To the archæologist it may be interesting to know, that, in 1838, Mr. Samuel Ferguson communicated to the Royal Irish Academy * a paper on the antiquity of the kiliee or boomerang, in which his object is to prove that the peculiarities in its motions are shared by it with the *cateia* and *aclys* of the Roman Classics, and that the latter is the same as the *ancyle* of the Greeks. The proofs for the *cateia* consist, first in the epithet *panda*, applied to it by Silius Italicus, and, secondly, in a passage found in Isidorus (a writer at the close of the sixth century), in which it is said: "*Si ab artifice mittatur rursum redit ad eum qui misit.*" (Origin, l. xviii. c. 7.) The principal proof for the *aclys* consists, first in the identification of the *aclys* and *cateia;* secondly, in the conclusion drawn from a passage of Valerius Flaccus, in regard to its shape, namely, that it was that of a half-moon; and thirdly, in a passage of Sidonius Apollinaris (of the fifth century), who speaks of these weapons thus: "Quæ feriant *bis*, missa semel." Mr. Ferguson inferred the identity between the *aclys* and the *ancyle* from their etymological affinity and from the assertion of the scholiast on Euripides, *ἀγκύλαι τὰ ἀκόντια, ἀπὸ τοῦ ἐπηγκυλιάσθαι* [Euripides Orestes, v. 1479].

The assertion of Isidoras, that the *cateia* and the club of Hercules are the same, is confirmed by an investigation of the radical meaning of these words, each of which is properly descriptive of a *curved* instrument, and by an exhibition of drawings of curved *clavæ* taken from the antique. Instruments of this shape may exhibit the peculiar path which distinguishes the Australian weapon. The same is true of the club or hammer of Thor, the Hercules of the Scandinavian mythology; and weapons having the shape of a cross or hammer may resemble in motion the boomerang. Hence an illustration, Mr. Ferguson infers, of the crosses on the pagan British coins. From the remarkable fact, that the names of the straight spear in several of the European languages are either identical or radically connected with those ancient names which are characteristic of the *crooked* missile, it is argued that the boomerang was a more ancient weapon than the spear. In other words, we must conclude, that as the club appears to have been the parent of the boomerang, so does the boomerang appear to have been the parent of the spear.

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* Proc. Roy. Irish Acad., Vol. I. p. 130–133; Trans. Roy. Irish Acad., Vol. XIX. Part I. p. 22; Pogg. Ann. XLV. p. 478.

5. The motions of the boomerang may be illustrated by pieces of card of the proper shape, bent so as to be convex on the upper surface. These miniature instruments rest upon a block of wood, inclined to suit the experiment, and are struck at *one end* by a spring, which thus gives the two motions of *rotation* and *translation.*

IV. PHYSICS OF THE GLOBE.

1. On the Dynamics of Ocean Currents. By Lieut. E. B. Hunt, Corps of Engineers, U. S. A.

It can scarcely be denied, that the state of our knowledge of ocean currents is any thing but satisfactory. Not only are we to a very great extent ignorant of the precise state of the facts, but we are also deficient in the theoretical exposition of those already known. We can easily explain our lack of precise knowledge of facts by reference to the circumstances. The vast oceanic areas can only be observed by persons engaged in navigation, who are mostly unfurnished with proper means for correct determinations, and who lack that special training which is a prime essential for good observations. The facts to be observed are also of such a complex and elusive character, are so subject to fluctuation on a given locality, and are involved in movements of air and water of such vast compass, that we cannot hope for precision of knowledge by the use of means now in operation. The single fact, that most observations are made on the water surface while the ocean depths are of vital efficacy in shaping all marine phenomena, gives a signal character of incompleteness to those observations which have been mainly instrumental in fixing the received notions on the system of oceanic circulation. It is a fit subject of regret, that the discussion of ocean movements has been so rarely attempted by those whose previous training in mathematical or mechanical science would have been a sufficient guaranty and preventive against the wild and illogical rhapsodies of the too numerous theorists, who have run riot over the broad domain of the physics of the sea.

With a view to apprehending the mechanical elements of this problem of ocean currents, let us first suppose a terrestrial sphere, which has assumed the equilibrium condition, resulting from gravitation, diurnal rotation, a solid nucleus, and an homogeneous water envelope unbroken by land. This water stratum would shape itself so that its bounding surface would be a strictly mathematical level surface. A level surface of this nature may be defined as one which is at each point perpendicular to the resultant of all the forces acting on the individual molecules situated on that surface. In this case it would be a continuous oblate spheroid, to which the resultant of gravity and centrifugal force would be everywhere normal. If to this we add those diurnal disturbances of the normal level due to the irregularities of solar and lunar attraction during the earth's rotation, we obtain the tidal waves which appear as perturbations of the normal level. If the continental masses be supposed to be elevated, we have a slightly modified normal level surface for the ocean; but one which, once determined, becomes the proper standard of reference for all oceanic perturbations, to whatever cause due. This surface is everywhere the true bounding surface, and cuts the resultant of gravity and centrifugal action for the earth *as it is*, perpendicularly at each point of the surface, and is entirely continuous, though no more truly spheroidal. This is the normal ocean level, and it is a useful surface of reference for all vertical ocean movements or perturbations. If we now suppose the homogeneous earth without continents subjected to the heating action of the sun's rays, the result will be that the equator will become a line of maximum heating, from which to the poles there will be a progressive diminution of heat absorption. This would cause an expansion of the heated waters, which would thus rise above the normal level surface by an amount equal to the expansion at each point. Thus from the pole to the equator a spheroidal meniscus would be spread equal to the ocean expansion under the solar heat. It is a remarkable feature that the heating leaves each vertical ocean column of its original weight, and there is thus a perfect mechanical equilibrium between these columns considered as joined by their interior bases. Thus considering the grand ocean masses, there is no disturbance of static stability from the heating agency of the sun, hence there is no formation of massive currents due to this cause.

If now we regard the heated ocean in its hydrodynamic aspect, we find that the bounding surface having everywhere a slope toward the pole exceeding that of the normal level surface, there remains an unresisted surface tendency towards the pole which primarily tends to produce a superficial flow from the equator polewards. This gives manifestly but a slight disturbance of normal level, amounting in the meridian quadrant only to the vertical expansion at the equator, and being diffused over the entire quadrant. It is extremely doubtful if this would suffice to overcome the passive resistances and produce an actual surface overflow. If, however, a current were once established by any other agency, such as the wind, the equatorial heating would constantly operate to maintain this current. The heated waters would constantly be lifted as a floating mass on the colder waters which, pushing on the lighter equatorial mass at its base, would come in to replace any deficiency of mass due to the superficial outflow. Taking the facts as they are, we find in the trade-winds and the resistance of the continents, two causes fully adequate to break up the static equilibrium referred to, and obviously giving the precise direction to the outflow which it actually has. Thus while the type of action induced by the solar heating power considered along the meridian is surface outflow and deep inflow, the perennial trades determine this circulation along a different and constant route, fixed first by continental obstructions, and essentially modified in direction by the earth's rotation.

As the equatorial evaporation greatly exceeds the corresponding rain fall, this operates to counteract in part the regular outflow by diminishing the quantity of water to be discharged on account of expansion due to heating. This would also increase the saltness and specific gravity of the equatorial waters, and to that extent would bring their actual surface into close accordance with the normal level. It is clear that this saltness could not fully compensate for the expansion by heating, or we should have the surface reduced to the normal level when all would either be in stable equilibrium, or below it, when the currents would be reversed.

It thus appears that the expansion due to equatorial heats induces a superficial derangement tending to outflow towards the poles, which by the trade winds and continents is determined along a single line of *de-*

bouche. This gives a discharge with far less frictional resistance than a direct meridional outflow would encounter, as this would involve a polar set for the entire ocean surface.

Accepting the well-determined trade-winds and the equatorial current as certain facts, we shall find that the vast surface sheet of water which has a westerly set under the trades, having acquired a very considerable velocity, becomes the representative of a vast amount of living force. When by impact against western barriers this vast sheet of water undergoes inflection to the north or south, it still retains the greater portion of its living force, and will continue to do so until this is wholly expended in overcoming resistances. If now we bear in mind that the wide equatorial sheet is by this deflection consolidated into a compact current of deep section, and also that the resistance per mile is proportionate to the length of the line of frictional resistance in a cross section, we shall see that the currents turn towards the poles with their forward impulse almost unabated, and with the resistances greatly reduced. We ought, therefore, to expect that the inertia of this vast moving mass would suffice to carry it on with a mean velocity, slowly abating, to the polar regions. So soon as the progress of the current gives it an increasing latitude, the effect of the diminishing parallels in giving an eastward trend would show itself; and, combined with the forward projectile motion of the mass of waters, would determine the route of the current, governed, of course, by solid opposing masses of continents, islands, and shoals.

Reaching the arctic neighborhood, this current would fall in with the tendency to restore to the equatorial region the waters withdrawn by outflow, which thus leave a deficiency of static mass in that region. Its forward force not yet expended, would bring it into the equatorial flow only after a long arctic sweep. Then, bordered in by the eastern ocean coasts, it circles on to the equatorial belt, there to start the repetition of its course either directly, or by proxy, if, entering at great depths, it serve only to lift higher portions above the normal level. We have thus a continuous circuit in which the water whirls under the primary impulse derived in the equatorial regions, an outflow due to heating and the direct propulsion of the trade-winds. The primary order of circulation is in two currents, the upper running polewards, and the under from the poles to the equator. This order is entirely modified by the action of the trades, and becomes essentially a horizontal circulation, the propelling action of these perennial winds, conspiring with the outflowing declivity to determine an immense movement, of which the living force imparted in the equatorial region suffices to carry on the circuit in full and enduring activity.

This consideration of the effect of inertia in storing the living force of this immense equatorial current, and thus enabling it to sweep through the cycles of the seas, has not been duly considered. These currents, in such a place as Florida Straits, move in a closed channel, and are subject to the hydrodynamic rules for this case. The gradual changes of direction and velocity there imposed, produce less absolute resistance than is generally imagined, by reason of the great mass of waters relative to the area of frictional surfaces.

The problem of ocean currents is of very great complexity, not only on account of the difficult hydrodynamic questions involved, but because the effect of the winds on the ocean surface can scarcely be subjected to estimation. The permanent elevation of the equatorial waters above the normal level traced from the pole, might be approximately determined by knowing the mean equatorial ocean temperature at all depths, and the same from point to point towards the poles, accompanied with observations on the corresponding saltness. Were there a considerable deficiency of weight in the vertical equatorial column, relative to an arctic one, connected by their bases at the same deep level, this would at once generate a corresponding wave towards the equator. As we may be sure that the equatorial mean ocean temperature exceeds the arctic mean temperature, we must concede *some* elevation above the true normal level throughout all the warmer latitudes, but any attempt to definitely fix its amount, would be very rash in the present state of our knowledge.

There are numerous secondary points which might enter this discussion, but which need not now be considered. I will notice a slight oceanic oscillation which is practically unimportant; but which I believe has not before been noticed. The sun in its daily round must heat the waters of the sea, at a given locality, in such a manner that there shall be a daily maximum and minimum sea temperature due to absorption and radiation combined. This must give a maximum and minimum of expansion, or a species of tidal wave would follow the sun, which might well be called the heliothermal tide. It would clearly be too minute for separate observation, and, though curious, cannot be important.

Another circumstance is worth notice here. A forward current in the sea has a distinct bounding surface on which it encounters a frictional resistance. The mode in which this resistance is expended is by a constant dragging into the forward movement, parts of the layer of water making the boundary of the current. Thus if a current be moving through a sea otherwise tranquil, it will by this lateral dragging carry forward such a volume of water in addition to its own proper mass, that a counter current must set in to restore the level. This is, I suppose, the explanation of some of the countercurrents which exist along the great oceanic currents, as also of the eddy currents of rivers. These too imperfect generalizations may do something towards making the system of ocean currents more comprehensible. So great a subject needs treatment far different from what it has yet received, and first of all the essential facts should be more clearly established. Unfortunately this can only result from long, well-organized, and costly operations for this express purpose. We must be content to do our several small parts patiently, hoping for more light in the future.

2. ON SOME ANOMALIES IN THE FLORIDA GULF STREAM, AND ON THEIR FURTHER INVESTIGATION. By Lieut. E. B. HUNT, Corps of Engineers, U. S. A.

THERE is perhaps no portion of the ocean waters which has been so imperfectly studied, in proportion to its importance, as that from the west end of Cuba through the Gulf of Florida. The whole commerce of the Gulf of Mexico is directly concerned in whatever investigations shall more accurately define the currents or other physical peculiarities of that portion of the Gulf Stream area from the line joining Cape Antonio to Cape Catoche, and the latter with the Tortugas, thence to the eastward through that grand channel bounded on the north and west by the Florida Reef and mainland, and on the south and east by Cuba and the Bahama Banks. As being the natural outlet for the products of the entire gulf coast and of the valley of the Mississippi, the present commerce following this route, vast as it is, must ultimately be so far exceeded, that it will seem almost insignificant in comparison with that which another half century will direct through this channel. The character of this navigation, no less than its amount, is such as to demand the most careful study of the currents, by which it is so largely influenced. All are so familiar with the immense annual losses to commerce by wrecks and disasters on the Florida Reefs and Bahama Banks, that a simple reference to the fact will abundantly vindicate the importance of carefully gathering whatever knowledge can give greater safety to this navigation. The high extra premiums for marine insurance by this Florida channel route afford another striking testimony to the risks of this navigation; but I think it right to remark here, that, from the best information I could obtain at Key West, the Florida channel insurance rates are very much too high, and are annually giving exorbitant profits to the insurance companies. This makes it the interest of these companies to exaggerate the dangers of this passage; and it is believed that they are, on this account, less averse to wrecks, and less strict in distinguishing collusive or fraudulent wrecks than they should be. A reduction of rates must soon be made, if we may judge from the fact that owners are, to a considerable extent, becoming their own insurers, in preference to paying the established rates. The new light-houses and coast-survey beacons have added much to the security of this route, and the business of wrecking at Key West is, on the whole, diminishing, although commerce is of course increasing. Making all due abatement for the exaggerations of the terrors of the Florida Straits, and for the increased aids to navigation, there still remains a very serious annual marine loss, due almost entirely to the imperfect acquaintance of navigators with the peculiarities of the route, and especially with the currents. Misled by false or imperfect views about the Gulf Stream, and other currents prevailing here, sea captains are frequently so unfortunate as to run directly on the reefs, while they suppose they are well out in the channel way. I cannot but think that a system of reef pilotage, properly organized and well conducted, would lead to a great reduction in the number of casualties. By taking and leaving well-trained pilots at the entrance to and exit from the region of danger, the numerous casualties due to the imperfect knowledge of sea-captains would be in great part obviated. Skilful pilots, constantly engaged in taking vessels through the channel, would grow more and more certain of all the essentials for secure passages, until it would become a gross offence to lose a vessel, except from causes truly extraordinary. It is obvious that such pilots should be so situated that no possible advantage could accrue to them in case of wreck, while a premium should be awarded for each safe pilotage. Were such a system in full operation, it would be a proper rule, that a vessel failing to take a reef pilot should forfeit its insurance, except when no pilot could be procured. A great difficulty in accomplishing this plan, except by the combined action of the insurance companies, is found in the fact that three national jurisdictions enter the field of pilotage.

A first essential for giving greater security to the Florida Channel navigation is a more correct determination of the currents by which vessels are affected during the transit. Having spent the last winter at Key West, I was led to inquire about these currents with some particularity, and, as a result, was brought to the opinion that the prevalent views are very seriously at fault. There are many facts quite incompatible with the common notions of a vast current constantly sweeping around the Gulf of Mexico, and thence pouring in full volume through the Gulf of Florida. I will here cite some notes of testimony given me by various persons specially acquainted with the matters in question. They were questioned more particularly with reference to facts and opinions touching a south-westerly current prevailing more or less between the easterly Gulf Stream and the Florida reef. Such a counter or eddy current is definitely indicated on Jeffrey's map of 1794, by a dotted line, above which is written, "North of this line is a current setting south-westward, unless when the wind is at north or east, which winds admit of no south-west;" and, "South of this line the current of the Florida stream sets always northwardly."

Capt. Geiger, who for over thirty years has been observing the waters of this vicinity, most of that time having acted as a pilot off Key West harbor, and who is perhaps better acquainted than any other person with the currents there prevailing, gives the following statement of facts.

A strong north or north-east wind keeps the Gulf Stream back, and makes a westerly current near the shore. During June, July, and August, the westerly current prevails more than the easterly current from five to fifteen miles from the reef. The direction of the current depends mostly on the wind. The westerly current prevails for from one third to two fifths the entire time from year to year, for from two to fifteen miles outside the reef off Key West. He has known it twenty-five to thirty miles off Sand Key. When the Gulf Stream is strongest on the Cuba shore, the westerly current is strongest on the north side; and when it is weakest along the Cuban shore, the Gulf Stream sets close along the reef. He has found the westerly current as far up as Carysfort, but not frequently, and not broad or strong. This current broadens from Carysfort to the westward, and continues about constant along its course. The tides on the two sides of the reef are about six hours apart, on an average; but set, on the whole, as much one way as the other over the reef. Sometimes there is a narrow easterly current for a mile from the reef; then a westerly current, and then the Gulf Stream. Both the United States steamers Susquehanna and Wabash were set westwardly by the current about eight or ten miles during the past season. A considerable number of the Gulf traders know of the westerly current, and make more or less use of it in navigating westwardly. When running with the wind, the water is smooth, and rough when running against it. After northers, the westerly current is to be expected. Sometimes, in crossing to Havana, no Gulf Stream indications are found; and sometimes a westerly current is found along the north shore of Cuba. Notwithstanding Capt. Geiger's long observation of these currents, he says that he is quite unable to reduce them to rule, or in any way to know beforehand how the current will be found to set. He asserts that the Gulf Stream sets, from the vicinity of Cape St. Antonio, north-easterly through the Florida channel, and that the main stream does not make the circuit of the Gulf of Mexico as generally supposed.

Captain Richardson, pilot of the Coast Survey surveying steamer Corwin, says, in substance, as follows: The westerly current appears irregularly chiefly in the winter, but sometimes during the prevalence of the regular trades. It extends from ten to fifteen miles off from Sand Key, sometimes running as much as two miles an hour. It never prevails over the reef proper. It sets for two months or so some winters. It spreads further from the reef as it goes west. Has known it as far north as Carysfort, just outside the reef, and at Cape Florida even where the reef is narrow and deep, this current sometimes sets across it some two miles from shore, but is not very frequently found there. As it runs west it seems to increase in breadth. Off Indian Key he has known it extend seven miles from the edge of the reef; at Bahia Honda it is sometimes ten miles, and at Sand Key from ten to fifteen miles. In the winter of 1856–7 there was very little of this current. In crossing from Key West to Havana the Gulf Stream runs much stronger on the Cuban side. To some extent, navigators know this westerly current, and use it with great advantage when bound west. In one case in 1852 he knew of two vessels bound east past Tortugas, which separated about 100 miles in twenty-four hours, by one captain knowing this current and the channel, while the other kept in the westerly current. The tide between the Quicksands and Tortugas sets flood N. N. E. and ebb S. S. E., differing from the charts.

Captain Wilson, who has for several years been running on the vessel serving Fort Jefferson on Tortugas as a mail boat to and from Key West, says that for some three months prior to Feb. 11, 1857, there was a strong and decided westerly current on the north margin of the gulf, on the reef between Key West and Tortugas. It had then for some two months been constantly to the west. Running out from Tortugas on an E. S. E or S. E. course, and tacking to the north or east of north when the point was reached, which in an ordinary gulf stream would bring him out somewhere from Sand Key light to six or seven miles west of it, which tack if there were no currents would bring him out abreast the Marquesas, he has six times in the last three months come out abreast the Quicksands, thus falling short of where he would have been had the water been still, by some eighteen to twenty miles, or some thirty miles west of Sand Key, where he would have fetched during a full Gulf Stream current. It usually takes about twenty hours to run both branches of this tack. There is no appearance of any current on or within the reef either way except the set of the tides. When the westerly current is running, he finds it better to beat up within the reef than to attempt to cross over into the Gulf Stream. Mr. W. thinks the current sometimes extends half way across to Cuba. He says, this westerly current has prevailed, more or less, every winter for seven years that he has been running between Key West and the Tortugas; but never so strong as this winter (1857–8), or for so long a time, probably not over a month in any previous case. He thinks the westerly current mainly disappears during the prevalence of the regular summer trades.

I was informed by General Totten that Com. Bainbridge told him, that in a voyage made by him some fifty years since, when he supposed himself in the Gulf Stream west of Cape Florida, he found by known landmarks that he had drifted a considerable distance to the westward instead of to the eastward.

I am indebted to Mr. Charles Tift, of Key West, for the following notes: —

"In December, 1856 (I think), the barque Joseph Hale from Philadelphia for New Orleans, got ashore ten miles south-east from Cape Florida light-house. She had passed round the 'Isaacs,' and made the Orange Keys, steering for the Double-headed Shot Key light. While looking out for the light, the ship apparently going seven knots, she struck, and proved to be in the position above stated.

"The ship Rockland, from New Orleans to Boston, was off the Pan of Matanzas at four o'clock (say March 25th, 1858), wind E. S. E., ship going per compass E. N. E., intending to sight Double-headed Shot Key light. At twelve o'clock saw what was supposed to be the light on Double-headed Shot Key, and kept the ship off to pass it on the gulf, or western side. But the light proved to be the new one on Sombrero Shoal (just opposite), of which the captain had no notice, and she struck a shoal inside the main reef. A glance at the chart will show how far these captains were mistaken in their estimate of both the force and direction of the current.

"Some years since a fishing smack left Key West to go to Cape Florida. The wind was eastwardly, and after she had beaten to windward for some forty-eight hours, she stood in to make the land. She fetched twenty miles to the westward of the starting-point, showing, in this instance, a strong westerly current in the centre of the gulf."

Mr. Tift adds that he "knows that the 'gulf current' sometimes, though rarely, runs strong to the eastward a mile or more *inside* of the reef (at Key West), but that the general set is westward for a *short* distance from the main reef." The idea, however, of a "strong westerly current" on this "edge," must be taken with many grains of allowance. A shipmaster leaving the strong gulf current and approaching the margin, finds the set so reduced in its rapidity as to conclude that he has found the stream actually going westward. As stated above, this is only true to a very limited extent, or in other words the belt of westwardly current is very narrow.

I am indebted to Mr. Baldwin, collector at Key West, for a case in his own experience showing a westerly drift, and for some observations made specially valuable by his long and full acquaintance, not only with the matters in question, but with the navigators frequenting Key West.

In June, a few years since, Mr. B. made a passage in a fast-sailing brig from Mexico to Havana. After leaving Campeachy Bank, they made Tortugas Islands and took a departure about sunset, steering about south-east by east. About midnight it fell calm, and for five days they experienced only calms and occasional light airs from the south. On the sixth day there was a light wind from the east. The master, an experienced navigator and well acquainted with those waters, steered south, supposing he had drifted through the gulf. On the morning of the seventh day he made land, which he supposed was somewhere near Matanzas, but which turned out to be near the Colorados, a reef off the west end of Cuba.

Again, in a voyage from St. Marks to Key West, Mr. Baldwin says, that being set by strong south-west currents in the Bay of Mexico, they fell to leeward and made the Tortugas islands. Having an experienced pilot they ran through into the gulf between the Tortugas and the Quicksands. After beating to windward for three days they stood in, and found themselves six miles to leeward of where they entered the gulf. Satisfied that they had to contend with a strong westerly current, the master consented to beat up inside the reef, and they reached Key West in 36 hours.

Mr. Baldwin says he has conversed with many intelligent shipmasters, with the Key West pilots, and with the masters of fishing smacks who are constantly crossing and recrossing the gulf to and from Cuba, and says that they assure him that no dependence can be placed on the Gulf Stream; sometimes it runs very much stronger than at other times in a north-east direction; that it very frequently runs in a south-west direction; and that at other times there is no current at all. Very frequently they experience an easterly current on the Cuba coast, and the reverse on the Florida coast, and at other times a strong current in the centre. The current cannot be mistaken, as the change is perceptible to the eye.

Mr. Baldwin adds, "A great deal depends on the force of the wind. My own observation has satisfied me that the wind influences the set of the Gulf Stream; for instance, after a heavy north-east wind the stream sets to the north-east at a very rapid rate, and *vice versa*. Since my residence at Key West, I have known several vessels to be brought in from the north-west, having got into the Bay of Mexico, when supposing themselves east of Cape Florida."

He was assured by the master of a vessel from Honduras, and another from Central America, both stranded, that they had come round Cape Antonio, and after beating as they supposed in the gulf, aided by the Gulf Stream for a number of days, discovered land, and judging it to be the Bahamas, shaped their course through the gulf, and were stranded near the Cedar Keys.

These scraps of testimony might be much extended if necessary, but I suppose they fully suffice to show that we are still very far from possessing the knowledge the case demands. They clearly prove that there is enough westerly current in the Gulf of Florida to be of vast importance to navigation if its movements can be defined, and to constitute a great danger, if it is not known. Its variations are also well established, and should be known to navigators. I am also quite well persuaded, not only from actual testimonies, but from the fact that a coral bank extends above Cape Catochee, that at least a large part of the Gulf Stream turns to the north-east around the west end of Cuba, instead of making the circuit of the Gulf of Mexico. The effect of the earth's rotation, and of its own inertia, on the current coming north from the Caribbean Sea, would be to give it an eastward bend. It is also quite incompatible with the tendency of the westerly current to expand towards Tortugas, to suppose that the main Gulf Stream comes sweeping in from near the mouth of the Mississippi towards this point.

Before attempting to theorize on the cause of this westerly current, it is certainly very desirable that it should be more accurately defined. The effect of dragging by the Gulf Stream along its sides, may perhaps be to produce a deficiency of water behind, to be replaced by a return current of considerable force, but it would certainly not call for such a vast body of westerly current as is vouched for in some cases, nor would it explain its alleged fluctuations. Some of the wrecks which have lately occurred seem due to a strong current setting through the Santarem Channel, and we may see in this a suggestion of a cause for the westward currents when these exceed the magnitude of a proper eddy. A Santarem current projected across the gulf, may be thrown down the reef, though I should not much expect such a result.

It will be well here to call attention to the refutation of the theory that the Gulf Stream owes its progress to a declivity resulting from heaping up waters in the Gulf of Mexico, which this parallel counter current affords. There is no evidence of any such elevation of the Gulf of Mexico as this theory calls for. On the contrary there is no such south-east current across from the Bay of Mexico, Barnes's Sound, &c., — as such an elevation would inevitably create. The whole notion of a descending river in the sea, with its source in the Gulf of Mexico, seems to me quite untenable and conflicting with facts.

The natural conclusion from what has preceded is, that there is abundant need of further exploration into the movements of this whole system of currents. Their incalculable commercial importance makes such an inquiry any thing but speculative, and should stimulate active and well-conditioned observations. We well know how imperfect the observations by the drift of ships must be; they are rather indications than measurements.

In view of the present state of the case, I would ask attention to the promise of results offered by undertaking an extensive series of current bottle observations on the line from Cape St. Antonio to Cape Catoche. By systematic proceedings, several points might be well illustrated. Suppose a vessel to cross on this line, say twice monthly for a year, throwing over one or two hundred bottles each time, containing slips duly numbered so as to indicate each starting point accurately; these points being regularly distributed on the line run, and checked by the verification of the route sailed. As these bottles proceed on their course, they will become faithful witnesses of the currents, and by their spreading they will show conclusively what the real course of the Gulf Stream is, and whether it is broken, one branch sweeping around the gulf coast, and the other pushing on north-easterly. With a view to promote their being readily picked up at sea, I would propose that flasks of white glass, blown with broad bases, should be used. These could be seen at a distance, and in a region so crowded with sails as the Gulf of Florida, very many would be picked up while still afloat, thus giving a true measure of mean velocity. A small sailing vessel, such as one of the Key West pilot boats, or the revenue cutter at that station, might, by having a good observer put aboard, make such a course of observations with slight expense in proportion to the results. It is hardly needful that I should here further state the bearings of such a plan, but I think all will concede to it the promise of elucidating some important questions of the gulf currents. It would surely be much better, could deep sea observations be made also, and to some extent probably they might be connected with a current bottle campaign. The superficial study ought certainly not to be longer deferred; after this, we can take a next step more wisely.

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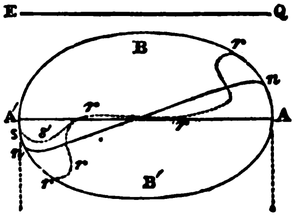
3. Note on the Currents of the Oceans. By Professor James D. Dana, of New Haven, Conn.

In the annexed figure, if $A\,A'$ represent a large elliptical basin of water warmed on the side towards $E\,Q$ and kept cold on the opposite, the isothermal diameter (or line of mean heat in the water's surface) will have the direction $A\,A'$. But if the water were put in equable circulation about the centre, in the direction of $A\,B\,A'\,B'$, the isothermal diameter, instead of having the direction $A\,A'$ would run obliquely across, like $n\,n'$, since the warm waters would be flowing in the direction $B\,A'$, and the cold in that of $B'\,A$. Or conversely, if in such a basin, warm on one side and cool on the other, as stated, an isothermal line had this oblique position, it would be evidence of an elliptical movement in the waters.



Again, if the waters moved most rapidly towards the margin of the basin, then the isothermal line might take the form $A\,r\,r\,r\,r\,A'$. This form would also result from the gradual shallowing of the basin towards its sides, since a slight motion about the deeper parts would be magnified in the shallower waters of the border (friction here being set aside, as it would have little effect if the basin were hundreds or thousands of miles in extent and of corresponding depth). Moreover, if either current, the warmer or colder, were an under current, the sloping sides of the basin would bear it up to the surface, and cause it there to affect the surface temperature, while elsewhere it might be unperceived. If in addition, the effect of the *colder* side were augmented, then a given isothermal line as $A\,r\,r\,r\,r\,A'$ might have a deeper bend on the side struck by the cold current than on the opposite, and so have the form $A\,r\,r\,r\,s'\,s\,A'$.

This figure and these conditions have a parallel in the oceans. The oceanic basins have their warm side against the equator, and their cold towards the polar regions. In the isothermal (or isocrymal) chart in my Expl. Exp. Report on Crustacea, and also in this Journal, vol. xvi. p. 153, I have given the direction of the isothermal lines of the surface water of the ocean in winter — or rather, in the coldest month of the year, each line corresponding to the mean temperature of the coldest month. In this chart, the isothermal lines closely correspond with the above diagram.

The mean position of the line of 68° is about half way between the equator and the parallel of 55°, within which the circuit mainly lies; and we may, therefore, take this line for our illustrations. We may look first to the South and North Atlantic. In the South Atlantic, this line reaches obliquely across, from latitude 31° south on the South American coast (the western side of the ocean, longitude 47° W.) to 7° S. on the African side (the eastern, in longitude 12° E.); and the line is almost straight, excepting the flexures near the coast, while the mean course makes an angle with the parallels or equator of about 22°. In the North Atlantic, the same line 68° F., is similarly oblique. It reaches from 56° N. on the American side, to 14° N. on the African, and the angle its mean direction makes with the equator is about 20°. Thus in each of these cases, the demonstration of the fact of an elliptical circuit movement in the ocean, is apparently complete. The modifications in the direction of the line are only those which result from the manner in which the South Atlantic and North Atlantic communicate with each other, it being such that the former contributes a large part of its warm waters to the latter, and so gives it a higher mean temperature.

The Pacific is a vastly wider ocean than the Atlantic, and the lines as they traverse the central waters, lose the most of their obliquity, since the movement is not much evident at the surface. But on the eastern border (the South American side) the cold waters are brought up to the surface by the continent, as was long since explained by

Humboldt. With this difference, the elliptical circuit is as apparent in the direction of the isothermal lines of the North and South Pacific as in the North and South Atlantic. In the South Pacific, the position of the isothermal of 68° F. off Australia is in latitude 30° S., and off South America in 2½° N.; and the mean obliquity through the ocean is ten or twelve degrees. In the North Pacific, the line of 68° F. off Japan is in latitude 35° N., and off the North American coast in 21° N.; and the mean obliquity is about 10°. The elliptical circuit in the Pacific, each side of the equator, is therefore demonstrated by the temperature. The Indian ocean exhibits the same facts.

This demonstration for each ocean is independent of the exceptional surface current movements, which give varying and often perplexing results.

It should be observed further, that the temperature of the ocean's surface in the equatorial regions during the colder season is little above 80 F., while that of the parallel of 50° is as low as 40° F., so that the former is 12° F. above 68° F., and the latter 28° F. below it. And hence unless the waters of the equatorial latitudes very much preponderated in quantity, the effect of the colder waters should be strikingly perceived along the east side of the ocean. In the North Pacific, as the ocean (as well as the globe) narrows north, the equatorial waters do thus strikingly preponderate. But little, if at all, is this the case with the South Pacific, since there is not the same convergence in the borders, and the ocean south of 30° is very free from islands, a fact indicating deeper waters. Besides, in the equatorial regions, the temperature diminishes rapidly in descending below the surface. There is abundant cause therefore for the excessive amount of influence of the cold waters on the western South American coast — an influence which is so great as to exclude a temperature of 68° from the Galapagos under the equator, as well as from the tropical American coast very near to the equator.

In my former explanations of this chart, I recognized these circuit movements in accounting for the currents, as they had been laid down by Mr. Redfield. But in accounting for the remarkably low mean temperature of the South Atlantic, I manifestly attributed too much influence to polar waters flowing in from the far south. Now the waters which move eastward in all the oceans — those of the higher temperate latitudes — in this elliptical circuit, move faster than the earth by an amount equal to their apparent rate of motion; and they manifestly could not have derived this velocity from any source but their previous position in more equatorial latitudes, where the earth moves faster. No other argument is needed to prove that the eastern movement is a counterpart of the western. In addition to this, since the waters flowing from the polar regions have less motion than the earth near the equator, and therefore are carried against the western side of the oceans (eastern side of the continents), as is well understood (and was fully illustrated, upon the isothermal chart referred to, by the cold current down the Asiatic coast, as well as that better known along North America), it is not possible that the South Atlantic current which flows towards the equator along western Africa can have accessions from the far south, as such waters would press against South America, the western side of the ocean; and there, in fact, as the chart shows, they do make themselves apparent. Its main supply is in the elliptical circuit itself as above explained.

My object here is not to discuss or dispute actual observations on currents, but to bring out a little more distinctly than has hitherto been done, the argument from the temperature of the ocean itself with regard to the grander points in the system of oceanic movements. The fact that the chart is a winter one for each ocean, does not affect the argument.

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4. On the Tidal Currents of New York Bay, near Sandy Hook. By A. D. Bache, Superintendent U. S. Coast Survey.

[Communicated by authority of the Treasury Department.]

In a former notice of the observations of tidal currents near Sandy Hook, I stated the general character of the results obtained, and their specific bearing upon the explanation of the growth of Sandy Hook, and of the changes above and below water in its vicinity. I propose now to describe more in detail the observations themselves, to discuss numerically and by the aid of diagrams the results obtained, and to show from them how the Hook is built up, the False Hook Channel

deepened, and Sandy Hook Bay modified in its shore line, and especially in the configuration of the bottom.

Sketch No. 1 shows the current stations occupied on the outside of the Hook, and in False Hook Channel and the approaches, in the different parts of the Main Ship Channel and its approaches, and in the eastern, middle, and western portions of Sandy Hook Bay. It will be convenient to discuss the subject under the three divisions already referred to of (1) the normal currents in the Main Ship Channel and its approaches, of (2) the currents of False Hook Channel and the approaches, and (3) the currents of Sandy Hook Bay.

1. *Normal Currents at the Entrance to New York Bay.*

Diagram 1 (sketch 1) shows the comparison of the average tidal currents at three groups of stations; the most eastern, comprehending the positions R_2, R, Q, and H, show the currents at and within the bar in the vicinity of Gedney's Channel; the middle, embracing the stations P, O, I, A_2, A, lying for the most part between the eastern portions of Flynn's Knoll and the point of Sandy Hook, shows the currents of this part of the Main Ship Channel; while the western group, B and B_2 gives the currents for the western part of the Main Ship Channel in the vicinity of the South-west Spit. Diagram 1 (sketch 1) also exhibits the type form of the tide wave as observed at Sandy Hook. The currents are plotted for flood and ebb, above and below a common axis, which corresponds to the mean level of the sea. These curves are plotted in their true relative positions to the tide wave as regards their epochs.

Table I. contains the *dates, names of stations, localities, luni-current intervals, durations, intervals after the tide wave, velocities*, and *directions* of the currents. In this table are given the numerical results from all the stations to which we have referred, reduced to their mean values. In Table II., the averages for the groups are given.

From the first line of Table II., we find that about 56^m after the time of high-water at Sandy Hook, the ebb current begins to make in the locality covered by the eastern group of stations, running east by south, reaching its greatest rate of about 1.71 miles per hour (in about three hours), and then decreasing (in about three hours) to zero, and turning to flood at about one hour thirty-three minutes after the time of low-water, the ebb thus being estimated at six hours nineteen minutes. The flood reaches its maximum in about three hours, when its rate is 1.11 miles per hour, running generally north-west for about five hours forty-three minutes, and reaching slack water about fifty-six minutes after the time of high-water at Sandy Hook. The times of running ebb and flood are nearly equal. The greatest rates are as seventeen to eleven, and the average as ten to six.

Line two of Table II. gives the results from the middle group of stations, from which it appears, that in that portion of the Main Ship Channel lying north of Sandy Hook, the ebb currents run generally east by south, the flood west by north. The ebb has a less velocity, and the flood a greater, than for the eastern group. The maximum velocity of ebb is 1.47 miles per hour, and of flood 1.68 miles; the mean velocities of ebb are 1.09, and of flood, 1.04 miles. The reason of this change of proportion between the maximum and mean velocities of the ebb and flood currents, is obvious from an inspection of the type curve. It will be seen that the ebb is characterized by a uniformity of rate during the second, third, and fourth hours of its existence, while the curve for the flood resembles the curve of sines.

The ebb current turns earlier, lagging but twenty-two minutes after low-water, thus turning more than thirty minutes before the corresponding currents to the eastward.

The third line of Table II. shows the results for the western group, in the vicinity of South-west Spit. Here the currents have a course round the compass, are much less rapid than the others, and are earlier.

It would be curious to know where the earliest turning of the current is to be found, and indeed these results open up many questions in regard to the whole bar and the channels through it. For our present purpose we have limited ourselves to obtaining the means of comparison of the normal currents sweeping by the point of Sandy Hook with those secondary ones which we shall next proceed to trace.

2. *False Hook Channel and the Approaches.*

The current stations *S, T, C, D, G, F,* and *V* (see sketch 1) show the movements of the water in this channel and its vicinity, while stations at *E* and "off Long Branch" lie quite to the southward. *S* shows the action at the northern entrance, and *V* at the southern; *C* in mid-channel. *T* at the entrance is influenced by the shoals of the False Hook. *G*, *F*, and *V*, between the ocean and False Hook Channel are unsatisfactory. *D* shows the shore currents of Sandy Hook.

The results from these stations are given in Table III., which is constructed in a manner similar to Table I. already described.

Treating *S*, *T*, *C*, *F* and *G* together as a group, we have for the luni current interval eight hours twenty minutes, or fifty-eight minutes after the time of high-water at Sandy Hook, for the commencement of ebb, the current of ebb beginning earliest at *S*, *G*, *T*, the flood first at *F*, *S*, *T*. The interval of ebb is about the same as that of the vicinity of Gedney's Channel, the flood three quarters of an hour earlier. This gives above a half hour for the difference of duration between the main ebb and flood, and the corresponding ones of this part of the approach to New York Bay.

The mean epochs (Table IV.) are eight hours fourteen minutes, and fourteen hours two minutes, being seventeen minutes and forty-six minutes less than those found for the vicinity of Gedney's Channel. The flood exceeds the ebb about twenty-four minutes; whereas for the stations near Gedney's Channel the ebb exceeds the flood by about thirty-four minutes, and in the South Channel the ebb has greatly the advantage both in duration and velocity. This shows the influence of the secondary or draught currents through the False Hook Channel.

The average maximum velocity at these stations of both flood and ebb is 1.41 miles, which last in the vicinity of Gedney's Channel was 1.71 miles. The mean maximum velocity of the ebb is but 0.91 miles, which was 1.03 miles for the stations near Gedney's Channel, and about the same at station *S*. The ebb makes out but slowly through the False Hook Channel; the maximum velocity of the flood at *C* is 1.62 miles, and of the ebb but 0.78, and the means 1.02 and 0.50 miles per hour. The time during which the ebb runs is five hours forty-one minutes, and the flood seven hours eight minutes, a difference of one hour twenty-seven minutes, and this in the axis of the stream. As the scouring action of these currents may be regarded as varying with the product of the square of the mean velocity and the duration, we have for station *C* the ratio of about one to five for the relative work of ebb and flood. At the edge of the False Hook Shoal the duration of the ebb appears to be even less, there being but five hours twenty minutes of ebb. The currents at C are plotted with the tide wave on diagram 3 (sketch 2). It appears that the curve representing the flood drift approaches the type form, while the ebb is anomalous — it is weak and inconstant.

We are now prepared to find the shore currents such as the observations prove them to be. The results at station D off the wreck of the brig Commerce, are shown in diagram 4 (sketch 2), where they are compared with the tide wave of July 18. The flood runs for seven hours fifteen minutes out of twelve hours forty-two minutes. The greatest velocity of the flood is 1.23 miles against 0.62 of the ebb. The directions are nearly opposite. The currents "off Long Branch," off Ocean House, at station D, and at station I near point of Sandy Hook, are shown on diagrams 4 and 5 (sketch 2). The ebb currents are quite feeble in both. It is the shore current, just discussed, which transports materials to the end of Sandy Hook; it is the grand current, in fact, which makes the False Hook Channel; and the False Hook occupies the debatable ground between the outside and False Hook Channel currents at certain times of tide, and is built up by the slackening ebb. In this point of view the shoal tends to restore equilibrium. The difference of action of flood and ebb is defined by this shoal. At station V on the southern extremity of the Outer Middle Ground, and in close proximity to the lower entrance of False Hook Channel, the currents are feeble, and an equilibrium of ebb and flood drifts seems to be established. It would appear that the scouring action of the tidal currents is insufficient to keep the bar of this channel open.

It is the material carried along the outer shore of Sandy Hook, and deposited at the point, which chiefly causes the growth of the Hook. Whenever it shall be considered desirable to limit this increase it will be easy to do so, by a series of jetties so placed as to arrest the movement of the sand to the northward, successive structures stopping the material which may pass round the point of the more southwardly jetties.

3. *Currents of Sandy Hook Bay.*

To obtain a clear idea of the tidal currents of Sandy Hook Bay, let us suppose it divided into three sections, eastern, middle, and western; and ascertain the relations of direction at successive periods. To this

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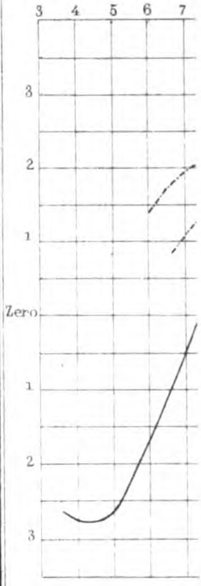
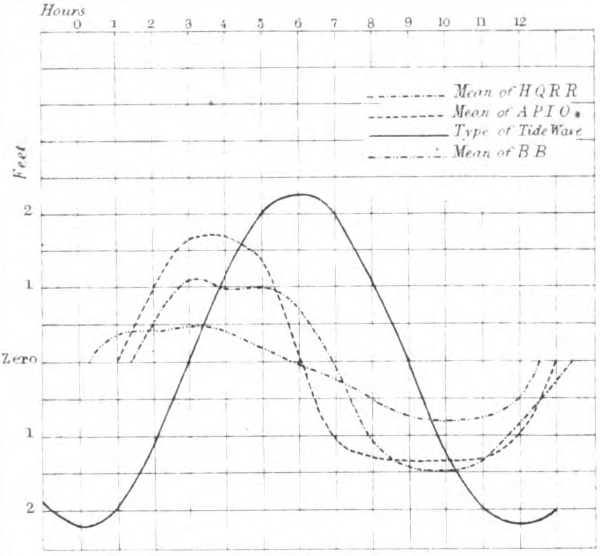
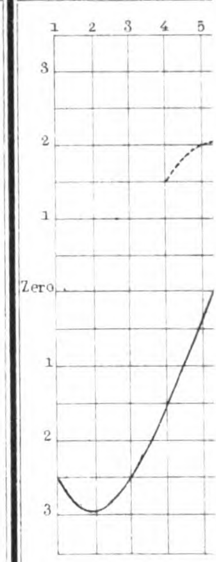
DIAGRAMS Nos 1 to 5.
Showing the velocity of currents
in and near
NEW YORK HARBOR ENTRANCE.

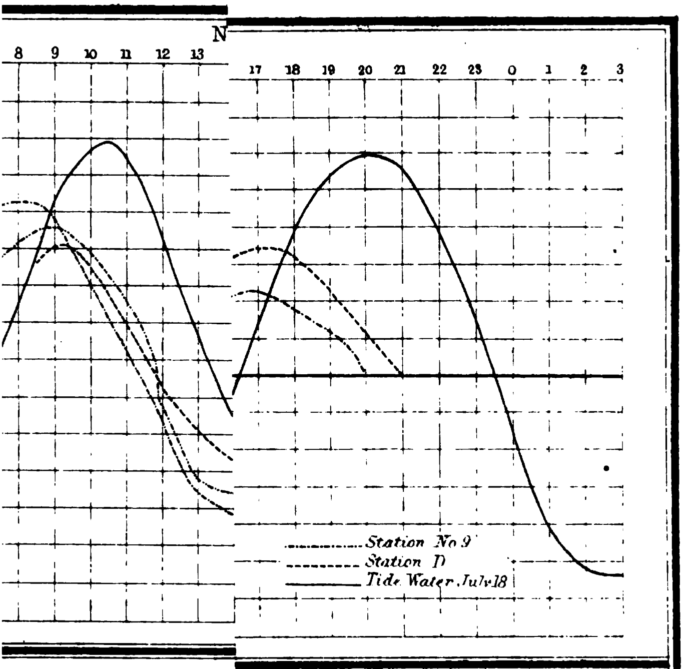


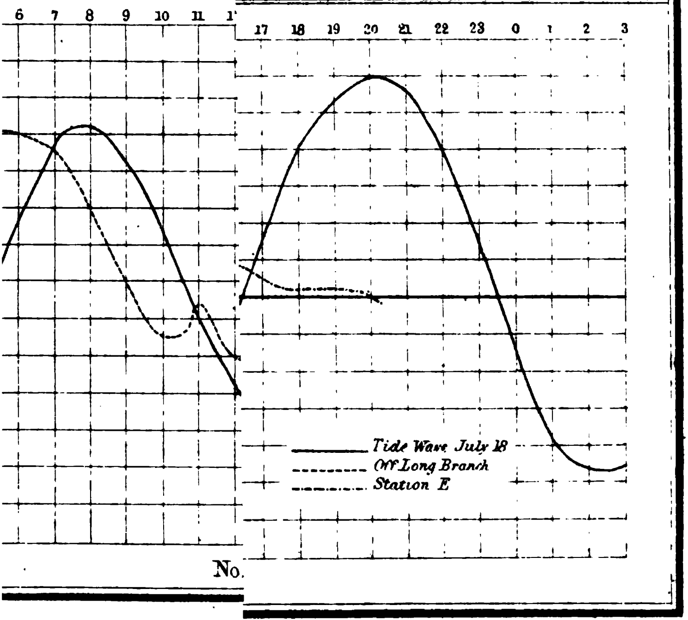


No 3.



Drawn by W.T. Bright.





Lith J Bien 60 Fulton St N Y

l we arrange the results according to the twelve tidal hours, calling epoch of low-water 0^h, the epoch of high-water 6^h, etc.

he flood current commences in the Main Ship Channel and in dy Hook Bay at nearly the same period, namely, about one hour four minutes after the time of low-water. The water of the a Ship Channel now flows to the westward; that of the eastern on of the bay to the southward; that of the middle to the south- and that of the western section to the west. This general condi- continues for about two and a half hours, and is exhibited upon ram 1 (sketch 3). It will be noticed upon this diagram that a ed region about the point of Sandy Hook is subject to a counter or whirl. This whirl is increasing in size, and its axis is moving hward, its existence is due to the lateral draught produced by the ng and steady stream of the ship channel, to which we shall refer after. At three hours the effect of this lateral draught on the ern section is already seen in the slacking up of the currents, and ur hours it has completely reversed the direction of the drifts. At period (four hours) the conditions are exhibited by diagram 2 etch 3). The currents are northward (with velocities of 1.4 and 0.3 es per hour at stations near to and more remote from the Main p Channel) in the eastern; southwestwardly (with velocity 0.3) in middle; and westwardly (with velocity 0.4) in the western section he bay, while the current of the Main Ship Channel, now at its ximum, holds a course due west with a velocity of 1.9 miles per r. Later still, we find this whirl, heretofore elliptical, assuming nore circular form. At five hours it has extended to the middle tion of the bay, reversing the currents of that region. (See dia- am 3, sketch 3.) Its axis still moves to the southward, and at six urs there occurs a space of slack-water, near the point of Sandy ook, where we at first noticed the existence of the whirl that we ve followed. The currents are now diminishing rapidly in the Main hip Channel, as well as in the western section of the bay, and at ven hours we find the ebb current prevailing. At first the course of e ebb currents of Sandy Hook Bay may be represented by straight nes converging to a point, in the Ship Channel north of Sandy ook, but these lines of direction gradually curve to the southward, ntil, as in diagram 4 (sketch 3), (which represents the conditions at the ninth hour), the waters of Raritan Bay press into Sandy Hook Bay; then, escaping along the shore of Sandy Hook, rush into the ship channel, causing a rip and counter drift at the point of the hook. At this time (nine hours) the current of the Main Ship Channel runs east (velocity 1.3 miles per hour); that of the eastern section of the bay, north (velocity 0.5); that of the middle, east by south (velocity 0.5); and that of the western, east by south (velocity 0.2).

The point of greatest curvature, in the sweep of the ebb drift through Sandy Hook Bay, moves continually to the westward, as will be seen by comparison of the conditions at nine hours and eleven hours (see diagrams 4 and 5, sketch 3). This grand movement ultimately creates a counter drift near the point of the hook, which assumes the form of a whirl as the current of the ship channel slackens. The ebb (northwardly) is the dominant current in the eastern section of Sandy Hook Bay; it commences to prevail three hours before the flood of the ship channel slackens. It is evident that this earlier drift is the lateral draught created by the stream of the ship channel, since, as we have previously shown, its domain increases with the velocity of the main stream, and it prevails generally in the eastern section of the bay when the main stream has reached its maximum velocity. Moreover, our several stations show that the duration and velocity of this northwardly drift are alike dependent upon the distance from the ship channel.

Lines one and four of Table VI. give the averages of the maximum velocities at several stations, as one mile of ebb against 0.4 of flood; and the mean velocities of 0.6 of ebb against 0.2 of flood. The mean durations are more than ten hours of ebb against less than two hours of flood.

In the middle of Sandy Hook Bay the ebb current begins nearly two hours before slack-water in the Main Ship Channel; its general course is south by east, that of the flood being east by north. The average maximum velocities, given by the fifth line of Table VI., are 0.6 of ebb against 0.4 of flood; the mean rates are above 0.4 of ebb against 0.2 of flood. The durations are about eight and a quarter hours for ebb, and three and a half for flood. In the western section of the bay, the ebb commences at nearly the same period as in the

U. S. COAST SURVEY

A. D. BACHE, Superintendent

Sketches Nos. 1 to 5.

Showing the set of currents in

SANDY HOOK BAY.

channel. The average velocities are nearly equal, and
ns are more nearly so than for the other portions of the

aring done by these currents will vary with the product of
n and the square of the mean velocity, if the material to be
l is the same. In the eastern section, therefore, the work
e ebb is to that done by the flood as forty-five to one. In the
tion the work done by the ebb and flood will be as nine to
e in the western section the feeble labors of ebb and flood are
al, and their direction so nearly opposed that little ultimate
s effected. The form of the bottom is in accordance with
lts.

he tide wave is propagated most rapidly in deep water, it fol-
the fall of the tide takes place earlier in the channel than
shore; hence the water tends to flow laterally *from* the shore
he channel. In this way a convergence of the ebb streams
xpected, especially in shallow bays. With the flood streams
rse must be true, and the tide wave, rising earlier in the chan-
ow of water takes place toward the shore. In consequence of
tinctive characteristics, the ebb and flood assume an unequal
the moulding of sandy coasts. The ebb current with its con-
on of forces is a far more powerful agent than the flood; its
g capacity along its normal course must be more considerable,
creates more extensive draught currents. The secondary or
t currents within Sandy Hook, called into activity by the un-
ly constrained flood of the ship channel, cover a district not
one and a half miles in extent, while the draught of the ebb
e outer shore is sensibly felt twelve miles down the New Jersey
- it is felt at Long Branch, where the primary ebb and flood are
extinct.

in, the ebb stream holds its course across the middle of shallow
in a narrow stream, while the flood, dispersive in its character,
s the broadest possible path, and presses along the shores. We
epared, therefore, even in the absence of draught currents, to find
od predominating over the ebb in certain localities near the

s not probable that the axis of the grand ebb current has moved
ward to avoid Sandy Hook, but rather that it naturally converges

at the mouth of New York Harbor, and that the weaker flood has not been able to destroy the work which the ebb has *directly* or *indirectly* accomplished. It is true that the flood, at the point of the hook, is sweeping the sand inward,—that, in fine, it is giving the spit of land the *hook form;* but the ebb is the primary working agent, and the characteristic features of all channels and basins on alluvial tidal coasts, must, as a rule, reflect the efforts of the ebb current.

That part of the growth of the hook which takes place from the inside, and which is not so considerable as that from the outside, is caused by the draught current along the shore, which we have been engaged in studying. The action of the waves undoubtedly contributes, by the stranding of the material, to this growth. The material on the inside is less favorable to the action of transportation, and the currents are lost at a moderate distance from the extremity of the hook.

TABLE I.

Date. 1856.	Station.	Locality.	Luni Current Interval.		Duration of Current.			Interval after tide of Sandy Hook.		Velocity of currents in nautical miles per hour.				Direction of Current.			
										Max.		Mean.		At Maximum.		General.	
			Ebb.	Flood.	Total.	Ebb.	Flood.	Ebb after H. W.	Flood after L. W.	Ebb.	Flood.	Ebb.	Flood.	Ebb.	Flood.	Ebb.	Flood.
			h. m.	h. m.	h. m.	h. m.	h. m.	h. m.	h. m.	m.dec.	m.dec.	m.dec.	m.dec.				
Aug. 2	R	W. end of Gedney's Channel.	8 32	14 53		6 21		1 05	2 10	2 14	1 15	1 22	0 47	E. by S.	N. W.	E. by S.	N. W.
"	Q	Between R. and P.	8 42	14 47		6 05		1 15	2 04	2 17	1 15	1 11	0 80	E.	W. N. W.	E.	N. W. by W.
July 13	R_2	East end of Gedney's Channel.	8 47	14 57	12 30	6 10	6 20	0 55	0 55	1 15	0 74	0 70	0 57				
" 23	H	Junction of Swash and Gedney's Ch.	8 02	14 36	12 31	6 34	5 57	0 30	1 04	1 37	1 42	1 10	0 72	E. by S.	N. W.	S E.	N. W.
Aug. 1	P	South-east of Flynn's Knoll.	7 48	14 48		7 00		0 20	1 19	1 90	1 42	1 15		E. N. E.	W. N. W.	E. by N.	N. W. by W.
"	O	Main Ship Channel near Flynn's Kn'll	8 18	14 23		6 05		0 50	1 27	1 02	1 53	0 80	0 86	E.	N. W.	E.	N. W. by W.
July 15	A_1	Main Ship Channel N. of E. Beacon.	8 30	15 25	12 00	6 55	5 05	0 16	1 09	1 32	1 90	1 02	1 15	E. by S.	W.	E. S. E.	W.
" 23	I	Near O.	8 12	14 35	12 23	6 23	6 00	0 15	0 37	1 63	1 50	1 39	1 10	E. by S.	W. by N.	E. by S.	W. by N.
" 15	A_2	Middle of Ship Channel.	7 43	14 31		6 48		0 11	0 36		2 07				W. by N.		W. by N.
" 15	B_2	S. of S. W. Spit.	7 46	14 16	12 08	7 08	5 00	— 10	1 00	1 12	0 43	0 47	0 26	E.	N. W. by W.	Round the compass	
" 16	B	Near B_2	8 12	15 58													

TABLE II.

Stations.	Luni Ebb.	Luni Flood.	Total.	Ebb.	Flood.	Ebb after H. W.	Flood after L. W.	Max. Ebb.	Max. Flood.	Mean Ebb.	Mean Flood.	At Max. Ebb.	At Max. Flood.	General Ebb.	General Flood.
R Q R_2 H	8 31	14 48		6 17		0 56	1 33	1 71	1 11	1 03	0 64			E. by S.	N. W.
P O A_1 I A_2	8 06	14 44		6 38		0 22	1 04	1 47	1 68	1 09	1 04			E. by S.	W. by N.
B_2 B	7 59	15 07		7 08		—10	1 00	1 12	0 43	0 47	0 26	Round the compass			

Note. — The minus sign is used to indicate that the slack current precedes the stand of the tide wave.

TABLE III.

Date. 1856.	Station.	Locality.	Luni Cur't Interval.		Duration of Current.			Interval after tide of Sandy Hook.		Velocity of Current.				Direction of Current.				
										Maximum.		Mean.		At Maximum.		Mean.		
			Ebb.	Flood.	Total.	Ebb.	Flood.	Ebb after H. W.	Flood after L. W.	Ebb.	Flood.	Ebb.	Flood.	Ebb.	Flood.	Ebb.	Flood.	
			h. m.	h. m.	h. m.	h. m.	h. m.	h. m.	h. m.	m.dec.	m.dec.	m.dec.	m.dec.					
July 15	U	S. Channel E. of Main Light.	8 34	15 41	13 17	7 07	6 10	0 20	1 25	1 80	1 00	0 71	0 52					
" 18	C	Middle of False Hook Channel.	8 28	14 09	12 49	5 41	7 08	0 50	0 25	0 78	1 62	0 50	1 02	S. E.	N. by W.			
" 21	F	West side Oil Spot.	8 28	13 48		5 20		1 08	0 28	0 62	0 95	0 35	0 60	S. S. E.	N. W.	S. E.	N. W.	
" 21	G	East side Oil Spot.	8 11	14 09		5 58		1 10	0 55	1 04	1 14	0 73	0 62	S. E. by S.	M. by W.	S. E.	N. by W.	
" 24	9	East of Stake No. 19.	7 07	10 36		3 29		—0 25	—3 05	0 35	1 01	0 20	0 40	S. E.	N. W. by W.	S. E.	N. W.	
" 18	D	E. of Main Lt. 500 feet from shore.	8 02	13 29	12 42	5 27	7 15	0 30	—0 20	0 62	1 23	0 15	0 72	S.	N.	S.	N. by W.	Shore currents from N. end of Sandy Hook to Long Branch, N. J., in 12 to 18 feet of water.
" 18	E	Off Ocean Ho. 500 feet from shore.	7 37	11 46	12 09	4 09	8 00	1 15	—2 30	0 17	0 15	0 12	0 07	S.	N.	S.	N.	
" 21		Long Branch 500 feet off shore.	10 42	11 49	11 07	1 07	10 00	3 28	—1 25	0 10	0 15	0 01	0 03	S. S. W.	N. by E.	S.	N.	
Aug. 4	S	Outside False Hook N. end.	8 09	13 55		5 46		0 40	0 50	1 77	1 64	1 06	1 10	S. E.	N. W. by N.	S. E.	N. W. by N.	
" 4	T	South-east of Station S.	8 22	14 03		5 41		1 00	0 55	1 44	1 44	0 95	0 86	S. E.	N. W.	S. E.	N. W. by N.	
July 19	V		8 09	14 31	12 22	6 22	6 00	—0 20	1 00	0 20	0 20	0 10	0 12					

TABLE IV.

S T C F G	South Channel, E. of Main Light.	8 20	14 01		5 41		0 58	0 48	1 13	1 36	0 72	0 84			S. E.	N. N. W.	
U		8 34	15 41	13 17	7 07	6 10	20	1 25	1 80	1 00	0 71	0 52					
G S T		8 14	14 02		5 48		0 57	0 58	1 41	1 41	0 91	0 86			S. E.	N. W. by N.	

NOTE. — The minus sign is used to indicate that the slack current precedes the stand of the tide wave.

TABLE V.

VELOCITIES CORRECTED FOR DIURNAL AND HALF MONTHLY INEQUALITIES.

Station.	Locality.	Luni Cur't Interval.		Duration of Current.			Interval after tide of Sandy Hook.		Velocity of currents in nautical miles per hour.				Direction of Currents.				Depth in feet.	
									Maximum.		Mean.		At Maximum.		General.			
		Ebb.	Flood.	Total.	Ebb.	Flood.	Ebb after H. W.	Flood after L. W.	Ebb.	Flood.	Ebb.	Flood.	Ebb.	Flood.	Ebb.	Flood.		
		h. m.	h. m.	h. m.	h. m.	h. m.	h. m.	h. m.	m.dec.	m.dec.	m.dec.	m.dec.						
No. 7	Near end of Hook, 200 feet off shore.	3 30	14 51	12 21	11 21	1 00	—4 15	1 15	1 40	0 23	0 58	0 12	N. N. E.	N. by W.	N. by E.	N. W.	28	Shore currents.
“ 6	500 feet S. of Station 7, 200 feet off shore.	3 18	14 37	12 24	11 19	1 05	—4 20	0 35	1 57	1 02	1 09	0 57	N. E.	S. W.	N. E.	S. W.		
“ 5	Off W. Beac'n, 600 feet and 400 feet from shore.	5 07	14 42	13 45	9 35	4 10	— 40	—0 30	0 78	0 46	0 46	0 21	N. N. W.	S.	N. N. W.	S.	6	Inside Sandy Hook, N. J.
		4 20	14 59	13 14	10 39	2 35	15	—2 50	0 78	0 15	0 43	0 11	N. by E.	S. E. by S.	N.	S. E.	7	
“ 4	Off tide gauge W. of Lt. Ho.	5 23	14 18	12 25	8 55	3 30	— 30	0 20	0 43	0 54	0 30	0 35	N. by E.	S.	N. by E.	S.	12	
K	2,200 feet off shore S. W. of W. Beacon.	4 32	14 19	12 12	9 47	2 25	—3 08	0 40	0 76	0 21	0 44	0 05	N. N. E.	S. by W.	N. N. E.	S.	24	Eastern portion of Sandy Hook Bay.
L_1	3,600 feet off shore, W. of Light-house.	5 09	14 34	11 36	9 26	2 10	—3 25	0 40	0 61	0 35	0 39	0 16	N. by E.	S. by E.	N.	S. by E.	24	
L_2	1,200 ft. S. W. of L_1.	5 28	14 27	11 39	8 59	2 40	—2 40	0 40	0 50	0 28	0 32	0 07	N.	S.	N.	S.	25	
M_1	W. of Lt. Ho. Middle of S. Hook Bay.	6 38	14 29	12 02	7 51	4 11	—1 50	1 45	0 82	0 69	0 43	0 26	N. E.	E. S. E.	N. E.	E. S. E.	25	Middle of Sandy Hook Bay.
M_2	2,000 ft. N. E. of M_1.	5 58	14 42	11 24	8 44	2 40	—1 50	1 40	0 80	0 47	0 45	0 15	E. by N.	S. E.	E. by N.	S. E.	27	
M_3	2,000 feet N. of M_1.	6 10	14 29	11 49	8 19	3 30	—1 45	00	0 78	0 16	0 45	0 03	E. S. E.	S. W.	E. S. E.	S. W.	30	
N_1	West of Light Ho., N. of port Monmouth.	8 23	14 43		6 20		—0 35		0 30	0 52			S. E.	W.	E.	W.	18	West'n portion of Sandy Hook Bay.
N_3	Between N_1 and N_2.	7 45	14 29	12 54	6 44	6 10	0 35	—0 20	0 36	0 42	0 17	0 21	E. by N.	W. by N.	E.	W.	30	
N_2	North end of Port Monmouth Dock.	7 58	13 52	13 24	5 54	7 30	0 10	—1 35	0 26	0 46	0 17	0 23	N. E.	W. N. W.	N. E.	W.	8	

NOTE. — The minus sign is used to indicate that the slack current precedes the stand of the tide wave.

TABLE VI.

y.	Luni Cur't Interval.		Duration of Current.			Interval after tide of Sandy Hook.		Velocity of currents in nautical miles per hour.				Direction of Currents.				Depth in feet.	
								Maximum.		Mean.		At Maximum.		General.			
	Ebb.	Flood.	Total.	Ebb.	Flood.	Ebb after H. W.	Flood after L. W.	Ebb.	Flood.	Ebb.	Flood.	Ebb.	Flood.	Ebb.	Flood.		
	h. m.	h. m.	h. m.	h. m.	h. m.	h. m.	h. m.	m.dec.	m.dec.	m.dec.	m.dec.						
	3 24	14 44	12 22	11 20	1 02	—4 18	0 55	1 49	0 63	0 83	0 34			N. by E.	W.	28	N. E. portion of Sandy Hook Bay.
	4 43	14 50	12 29	10 07	3 22	—0 12	1 40	0 78	0 30	0 44	0 16			N. by W.	S. by E.	6	Shore currents.
	5 23	14 18	12 25	8 55	3 30	—0 30	0 20	0 43	0 54	0 30	0 35			N. by E.	S.	12	Sandy Hook
	5 05	14 27	11 47	9 22	2 25	—3 04	0 40	0 62	0 27	0 38	0 09			N.	S.	24	Eastern portion of Sandy Hook Bay.
	6 16	14 33	11 47	8 17	3 30	—1 47	1 08	0 63	0 44	0 44	0 15			E. by N.	S. by E.	27	Middle portion of Sandy Hook Bay.
	8 02	14 22	13 10	6 20	6 50	0 03	0 58	0 30	0 47	0 17	0 22			E. by N.	W.	18	West'n portion of Sandy Hook Bay.

Note. — The minus sign is used to indicate that the slack current precedes the stand of the tide wave.

5. DEEP-SEA SOUNDINGS. By W. P. TROWBRIDGE, Assistant U. S. Coast Survey.

COAST SURVEY OFFICE,
Washington, D. C., May 31, 1858.

DEAR SIR, — On receiving directions from you to examine the deep-sea soundings of Lieut. Com. Joseph Dayman, R. N., along the line previously explored by Lieut. Com. Berryman, I took advantage of your suggestions and made an investigation of the laws of motion of the sounding weight and line used in deep-sea soundings. This seemed to offer the best mode of arriving at correct conclusions with regard to the probable errors of the results generally obtained by the methods employed, by which means only the values of the results can be determined, and comparisons of different results made.

Having had opportunities of consulting with you from time to time, I have been enabled to follow out your suggestions more satisfactorily than I could otherwise have done.

The substance of the report, which I have the honor to submit herewith, was given by your kind permission at the late meeting of the American Association, and I have prepared it in a form to be printed with the proceedings of the Association, if you should think it advisable.

The conclusions to which the investigations lead seem to be, briefly, as follows: —

1st. The sounding weight reaches its maximum velocity within the first three seconds of time after the weight has reached the surface; this maximum varies with the form and specific gravity of the weight, being sixteen feet per second for a thirty-two pound shot, and twenty-seven feet for a ninety-six pound iron weight, such as was used by Lieut. Com. Dayman.

2d. When the line begins to offer resistance to the motion, the velocity is rapidly diminished, becoming at the depth of two thousand fathoms, only two or three feet per second, and at three thousand fathoms, still less.

3d. This diminution of velocity is due entirely to the resistance offered by the line, the resistance offered by the sinker being at the depth of two thousand fathoms only a fraction of a pound, while that of the line amounts to nearly the whole weight of the sinker, the excess of the weight of the sinker being sufficient to impart a very small velocity to the line.

4th. The resistance upon the line varies, 1st, as the square of the velocity; 2d, as the diameter of the line; 3d, as the length of line immersed.

5th. The variations of gravity do not sensibly affect the rate of sinking.

6th. The variations of density do not increase the resistance at great depths, but it appears that bodies which are compressible will fall more rapidly at great depths than near the surface.

7th. The smallest line that can be used with safety will give rise to sufficient resistance to support nearly the whole weight of the heaviest sinker at considerable depths.

8th. The probable errors of results when the length of line paid out is taken as the depth, are so great that this method is the most unreliable for determining the depth.

9th. The method of ascertaining the depth by Massey's Indicator is the best that has been devised, provided the indicator be always used with the same line and weight. This method, however, has also been very unsatisfactory, giving large errors on account of the faulty construction of the machines used.

10th. The results of Lieut. Com. Dayman were obtained from the former method; those of Lieut. Com. Berryman by the indicator; and therefore Berryman's results are undoubtedly nearer the truth than Dayman's; but from neither can the existence of a "plateau" between Newfoundland and Ireland be made out.

There is yet, it seems to me, nothing to show that the bottom of the sea here presents features any different from the face of the country across any portion of our territory, except perhaps the highest mountain ranges. The Highlands of the Hudson, for instance, would be unnoticed in this "plateau."

11th. To obtain correct results, some improved method of registering the depth seems to be necessary, and I would respectfully suggest a modification of Mr. Saxton's Current Indicator to suit the circumstances of the case.

I would also beg leave to suggest the coiling of the line within a cylinder, which is to be attached to the weight, so that the line will un-coil from the moving body. By this means the resistance due to the motion of the line in the water will be avoided. If this be practicable, much greater depths may be reached, and the weight will descend through all depths with a rapid and uniform velocity.

The mode of registering the depth may be the same as heretofore used, with less liability to error, on account of the short time required for the apparatus to sink to the bottom.

Where the time required for the weight and line to sink to the depth of three thousand fathoms is now one hour and a half; by the method proposed, the apparatus might be made to sink to the same depth in fifteen or twenty minutes, and to smaller depths in proportionally less time.

Yours very respectfully,
W. P. TROWBRIDGE,
Assist. U. S. Coast Survey.

To Prof. A. D. BACHE, *Sup. U. S. Coast Survey.*

DEEP-SEA SOUNDINGS.

The explorations of the Gulf Stream by officers of our navy, in connection with the survey of the coast, first gave rise to systematic efforts to determine sections of the bottom of the sea, along continuous lines of great depth; afterwards the extensive system of deep-sea soundings executed under the direction of the Navy Department, through the exertions of Lieut. Maury, furnished most useful experience and results, and more recently the soundings along the line of the proposed Atlantic telegraph, first by Lieut. Com. Berryman, U. S. Navy, and again by Lieut. Com. Joseph Dayman, R. Navy, have shown that to reach the bottom of the sea is not beyond the power of human effort.

Notwithstanding so much has been done to overcome the practical difficulties which formerly embarrassed and discouraged any attempts to sound at great depths, it is undeniable that much uncertainty exists in the determinations that have been made, even where the depth is not greater than two thousand fathoms; while below two thousand or two thousand five hundred fathoms, the errors and uncertainties amount to failures; and a limit has been reached beyond which, it seems difficult to pass.

When the subject is viewed as a mechanical problem, it seems hardly possible, however, that insurmountable obstacles can exist to its practical solution. The facility with which an observer can place himself over any point, the distance of which below the plane on which he rests, it is his object to determine, and from which he is separated by a medium freely and almost equally penetrable at all depths, the laws of which are well known, would seem to offer greater hopes of our being able, ultimately, to determine the depressions of the bottom of the ocean by a greater number of observations, and with greater accuracy, than the elevations of continents.

The solution of the question requires that the difficulties which have been found to exist, in the present methods of sounding, should be clearly determined and overcome; and when these methods fail, that they should be improved if possible, or new ones substituted.

It will be seen, on the least reflection, that the difficulties are of a mechanical nature; not simply practical, for all that skill and experience can accomplish has been effected in overcoming the practical difficulties which have been met in applying the ordinary methods; but the methods which are most naturally adopted for small depths fail when the depth is very great, on account of their inapplicability in the latter case.

It is important to separate those difficulties which may be overcome by skill and experience, from those, which, under the most favorable circumstances and conditions, are still as great as ever, and depend only upon the mechanical forces developed; and of course some proof will be required to show that the obstacles presented by the latter causes are, in the present mode of sounding, almost, if not quite insurmountable.

If we suppose the water of the ocean to be in a state of rest, a sounding weight and line will descend according to the laws which govern the falling of heavy bodies in a resisting medium; the combined weight of the sinker and line, in the water, being the accelerating force, and the resistance offered by the water to the motion of the weight and line, the retarding force; and, upon the relative magnitudes of these forces, the circumstances of descent will depend.

The weight of the sinker and line in the water may be easily determined from their specific gravities; and for the purpose of determining the nature of the resistance upon these bodies, moving in the water, we have numerous experiments.

The experiments of Dubuat, in the "Principes d'Hydraulique," furnish data for determining the resistances to the motion of bodies whose forms and dimensions correspond to the sinkers generally used in sounding; and the numerous experiments in deep-sea sounding, to which reference has been made, especially those of Lieutenants Lee and Berryman, U. S. Navy, afford the means of determining the resistance of the water upon the line.

In these latter experiments, the rate of descent was accurately observed; and the results of numerous trials, with different weights attached to the same line, and with different lines, furnish the means of ascertaining the velocity of descent in each case, at every point of the path described by the falling body, with sufficient accuracy to determine all the circumstances of motion.

The problem then becomes one of the most simple in mechanics, namely, having given the space, time, and velocity corresponding to the fall of a given weight in water, to find the law of resistance to the motion.

In some respects the problem is a new one. Although the resistances to the motion of any body, in water, under ordinary circumstances, are known to be proportional to the squares of the velocities, yet the experiments from which this rule was deduced, were made upon bodies of certain regular forms, the length of which, compared with the diameter of their cross-section, was not very great; and it will be at least interesting to know whether this law is true with reference to a line of great length and small diameter; and especially when this line is subjected to the great pressure which is exerted upon it in the depths of the ocean. The resistance in this case is entirely due to the velocity which is imparted to a certain quantity of water along the line, by its motion, and not to friction in the ordinary sense of the term. If the resistance from friction be at all considered, it must increase greatly with the depth, since the pressure is increased by fifteen pounds on every square inch for every thirty-four feet in depth. It will be seen by the subsequent discussion, that the pressure has no influence in increasing the resistance.

Again, the density of the water at great depths, as affected by the compressibility of that fluid, must be considered, and also the variations of the force of gravity; which will of course depend upon the depth. These circumstances make the solution of the problem of interest, when considered in relation to the experiments which have been made upon the motion of bodies in water, under ordinary circumstances, at the surface of the earth; and the discussion of the subject should enable us to determine the best means of overcoming the difficulties which now exist in sounding to great depths.

The experiments of Lieutenants Lee and Berryman were made with one and two thirty-two pound shot (iron), the line used being about seven hundredths of an inch in diameter, and capable of bearing a strain of sixty pounds; the weight of one hundred fathoms in air being about one pound.

The mode of observing was as follows: several thousand fathoms of the line were coiled upon a reel of light construction, marks being first placed along the line at intervals of one hundred fathoms; the end of the line was then attached to the weight, one or two thirty-two pound shot, as the case might be, and the shot dropped into the sea. The rate of descent was observed by noting the time of running out of each one hundred fathoms, by a chronometer. The experiments were made from a boat, which was kept accurately over the descending line by the oars. In this manner the effects of the drifting of the boat in giving apparently a more rapid descent, and a greater depth, were avoided; and although it is probable that in many instances the drift of the boat was considerable, yet the mean results of the experiments of Lieutenants Lee and Berryman, from observations extending through several years, differ only by a small constant quantity, which is inappreciable in the determinations of velocities to the depth of one thousand fathoms, and below this the difference is still slight. I have therefore made use of the experiments of Lieutenant Lee, which in some respects are preferable (and which are sufficiently numerous to give good results), rather than a mean of the results of Lee and Berryman.

The intervals of descent for one and two thirty-two pound shot are given in the table (I.); the first column shows the depth in fathoms; the second, the intervals of descent, in time, for each successive one hundred fathoms.

The velocities are determined from these intervals, by supposing the motion uniform during the running out of each one hundred fathoms, and dividing the space in feet, by the time in seconds, which will give,

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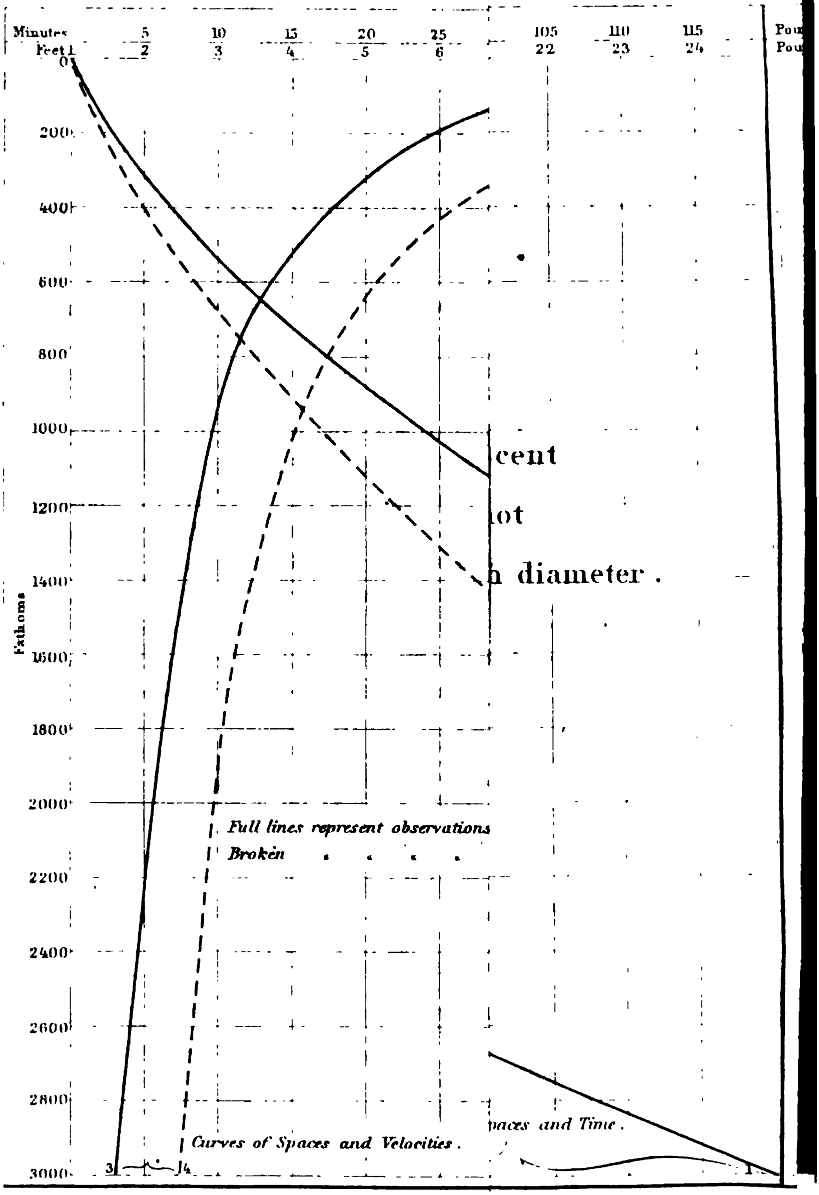

ry nearly, the velocity in feet at fifty, one hundred and fifty, two ndred and fifty, &c., fathoms. The velocities at these points are ren in the third columns under their respective heads.

It will be observed that the velocities of descent diminish, in the se of one thirty-two pound shot, from 8.83 feet per second at fifty homs, to 2.84 feet at one thousand, and 2.09 feet at two thousand homs, and so on. And in the case of two thirty-two pound shot, m 12.50 feet per second at fifty fathoms, to 3.48 feet at one thou-nd, and 2.99 feet at two thousand.

The curves representing these circumstances of motion are given in agrams marked (A); the vertical distances representing the spaces fathoms, and the horizontal distances for curves (1) and (2) repre-nting intervals of time, and for curves (3) and (4) velocities in feet. urves (1) and (2), therefore, show the relation of the spaces and nes, and curves (3) and (4) the relation of spaces and velocities. he curves in black are for one thirty-two pound shot, and those in d for two thirty-two pound shot; the sounding line being the same both cases. As these bodies have no initial velocity, but start from st at the surface, it is evident that the curves of velocity must pass rough the origin (0), and as their velocities are already diminishing pidly at fifty fathoms, it is interesting to determine the point of maxi-um velocity. For this purpose we must refer to the mathematical rmulæ for the motion, which are well known. In case of a heavy ody falling in water, the velocity expressed in terms of time, weight, ensity of medium, and form of body, is given by the following expres-ion,

$$v=\sqrt{\frac{2\,g\,W}{S.D(m+q)}}\left(\frac{N-1}{N+1}\right),$$

in which (g) represents the force of gravity, (W) the weight of the body in water, (S) the area of the greatest cross-section of the body, (D) the density of water, and ($m+q$) a constant determined by experiment; and the Hyperbolic Log. of $N=\frac{t}{P}\sqrt{2\,g\,W.\,S.\,D.\,(m+q)}$.

In this latter expression (P) represents the weight of the body in air, including also the weight of the fluid drawn along by the body; and (t) the time in seconds.

The velocity will increase as (t) increases and will become constant when the factor $\frac{N-1}{N+1}$ becomes unity, and it is evident that when (t) becomes so great that N may be taken for $N-1$ and $N+1$, without sensible error, the velocity will have become nearly constant, and will be represented by

$$v=\sqrt{\frac{2gW}{SD(m+q)}}.$$

In the case under consideration, $g=32.189$ (feet), $W=27.38$ (pounds), the weight of a thirty-two pound iron shot in sea water, $s=0.21$ (square feet), D represents the weight of one cubic foot of sea water $=64.177$ (pounds), and $(m+q)$ as determined by Dubuat in the experiments given in the "Principes d'Hydraulique" for a sphere moving in water $=0.5$. These constants substituted in the formula give for one thirty-two pound shot, falling freely in sea water,

$V=16.21$ (feet per second) for the maximum velocity.

To find the space passed over, before this velocity is reached, we have this expression,

$$Z=\frac{2P}{S.D(m+q)}.\log\frac{N+1}{2\sqrt{N}};$$

or when $N+1$ may be taken for N, this expression may be placed under the form

$$Z=\frac{2P}{S.D(m+q)}\left(\tfrac{1}{2}\frac{t}{P}\sqrt{2gW.S.D(m+q)}-L.2\right).$$

N becomes so great that $N+1$ may be taken for N, may be determined from the expression,

$$L.N=\frac{t}{P}\sqrt{2gW.S.D(m+q)}.$$

Substituting in this expression, the values of the known quantities for a thirty-two pound shot we have,

$$L.N=t.(3.13).$$

For one second $L.N=3.13$ and $N>10$
For two seconds $L.N=6.26$ and $N.=1000$ nearly
For three seconds $L.N=9.39$ and $N=10000$ nearly.

Thus at the end of the third second N becomes so great that $N+1$ may be taken for N, without sensible error, in determining the velocity and space; and the formula for the space gives:

For the first and second seconds,
1st second $Z = 9.11$ feet.
2d second $Z = 25.$ feet nearly, and 16.21 feet for each second afterwards.

The velocity of the shot reaches its maximum, therefore, before the resistance of the line has any great effect in retarding its motion; that is, during the first two or three seconds. Two thirty-two pound shot placed side by side will of course fall in the same time, and will attain the same maximum velocity, and the value 16.21 (feet) will be also the maximum for two thirty-two pound shot; when the resistance of the water upon the line is developed, however, the greater weight will have the greater power of overcoming this resistance, and its velocity will be retarded less than in the case of one thirty-two pound shot, and the curves representing the velocities will commence to diverge at the maximum point.

It will be seen from the curves, that the velocities diminish very rapidly for the first three or four hundred fathoms, then more slowly, and in case the specific gravity of the line is nearly the same as that of water, the vertical $\overline{O.C}$ will become tangent to the curve of velocity at an infinite distance; and will become sensibly tangent at the depth of a few thousand fathoms, or, in other words, the velocity of the sounding weight will be almost entirely destroyed. If the line is much heavier than water, and incompressible, as in the case of a wire, the velocity will reach a minimum at a certain depth, generally less than two thousand fathoms, and will afterwards increase. Both of these cases have important bearings, as will be seen hereafter.

The velocities as determined from experiment, show but little diminution below 2,000 fathoms; but the number of cases below that depth was much less than at smaller depths, and moreover the difficulty of determining the intervals correctly, below this depth, is very great; the action of currents upon the line, the drift of the boat, &c., when three miles of line are out, would affect the determinations greatly, and it seems to be preferable to determine these lower velocities by induction; instead of finding a velocity of two feet at three thousand fathoms, the actual velocity as determined by the curve, will not be greater than one foot and a half.

The curves of velocities seem to approach nearer to the hyperbola than any other form.

The resistances which cause this diminution of velocity in the descent are due entirely to the constant increase in the length of line, since, as we have seen, the resistance which would be due to the shot falling alone corresponds to a constant velocity of 16.41 feet; and as the velocity is diminished, the resistance to the shot is diminished. To separate the resistance upon the line from that due to the shot, and to find the amount of these forces in pounds, we have for the resistance upon the shot at any point

$$R = S.D\,(m + q)\,h.$$

(h) being the height due to the velocity; and if at any point of the path described the motion be supposed uniform, for one second, we shall have, calling (W) the weight of the shot and line in water, (R') the resistance upon the line, and (R) the resistance upon the shot or sinker,

$$W - (R + R') = 0\,;$$

that is, the resistance is equal to the weight.

This is the simplest mode of considering the subject. If the inertia of the masses be taken into account, we must employ the expression,

$$\int_b^a W\,dz - \int_b^a R\,dz - \int_b^a R'\,dz = \tfrac{1}{2} M\,(V^2 - V'^{[2]})\,.$$

V and V' being the velocities at any two depths, (a) and (b) and the first member representing the quantity of action of the accelerating and retarding forces.

The term $\frac{1}{2} M\,(V^2 - V'^{[2]})$ involves the resistance opposed by the inertia of the shot and line, while the velocity is diminished from V to V', and the resistance opposed by the inertia of a quantity of line equal to the distance ($b - a$) as it is drawn from the reel at rest, and a velocity V' imparted to it. If the distance ($b - a$) equal unity, or one foot, V and V' may be considered equal, and the latter expression reduces to the first equation; and by finding the relation between the quantities in the first equation, at different points of the depth, the values of the forces at these points will become known, and their variation with the depth will be exhibited.

The expression $W-(R+R')=0$, or $W-(S.D\,(m+q)\,h-R')=0$, involves two variable quantities, (g) the force of gravity and (D) the density of the water.

The variations of gravity within the external surface of the earth, supposing the density of water to be the mean density of the earth, are proportional to the distance from the centre of the earth, and if (g) represent the force of gravity at the surface, (g') the same force at the depth z, and (r) the radius of the earth, we shall have

$$g'=g\left(\frac{r-z}{r}\right)=g-g\frac{z}{r}.$$

The equatorial radius in fathoms is equal to 3.487,266, and at the equator for a depth of three thousand fathoms we shall have

$$g'=g-g\frac{3000}{3.487{,}266},$$

the second term being only .0004 of the force of gravity; but the density of the water is less than the mean density of the earth, and the force of gravity will therefore not diminish with the depth, but in a less ratio; so that the amount of diminution at three thousand fathoms will be less than .0004 of the force; we may therefore consider the force of gravity constant; and, in reality, its diminution due to depth will be less than its variation with the latitude on the surface.

The density of the water increases with the depth, according to the law of compressibility of water; and to find the variations of density with depth, we must refer to the law of compressibility.

From a report of Mr. Schott, it appears that the shortening of a column of water from the pressure due to six hundred fathoms is two fathoms; or the cubical diminution is about .0001 of volume for one atmosphere.

The density being inversely as the volumes, for a given weight, we shall have, supposing (D) to be the density at the surface, and (D') the density at the depth (z),

$$D'=D.\left(\frac{1}{1-.0001\,.\,\frac{z}{8390}}\right).$$

From this expression, we find the density at

Surface	=1.029
One thousand fathoms	=1.048
Two thousand fathoms	=1.066
Three thousand fathoms	=1.084

This increase of density is too small to affect sensibly the buoyancy of the fluid, or the resistance to motion, when considered with reference to the quantities which we wish to determine.

The weight of a thirty-two pound shot, for instance, in water, at the surface, will be

$$W = 32 - 32 \, . \frac{1.029}{7.2 \ldots},$$

the numerator and denominator of the last term being the specific gravities, respectively, of water and cast-iron. At three thousand fathoms the weight of the shot would be

$$W = 32 - \frac{1.084}{7.2 \ldots},$$

and we see that the variations fall within the range of the specific gravities of the different varieties of cast-iron.

The variation as affecting the resistance to motion will be shown in the expression,

$$R = S \, . \, D \, (m + q) \, h \, .,$$

where it is impossible to determine $(m + q)$ or $h\,.$ within the range of variations of D. We may therefore regard all the quantities as constant, and known, in the expression

$$W - (S \, . \, D \, (m + q) \, . \, h) - R' = 0,$$

except R', which represents the resistance upon the line, and may be determined from the equation.

If the specific gravity of the line be very different from that of water we shall have

$$W = W \pm W'z,$$

in which $(W'z)$ is the weight of that part of the line immersed; the plus sign being used when the line is heavier than water, and the minus sign when it is lighter; and (W') being the weight in water of one foot of line.

In the experiments with one and two thirty-two pound shot, the specific gravity of the line is supposed to be the same as that of water, and it is only in case of a wire being used, or a hemp line of large diameter, that it is necessary to take the specific gravity into account. As we wish to determine the *laws* of the resistances and not their absolute quantities in pounds, except in general terms; these very small quantities may with propriety be neglected.

We may now find the values of R' at different depths in the cases under consideration, namely, the fall of one and two thirty-two pound shot attached to a small line.

For one thirty-two pound shot $W = 27.36$ pounds, and $S.D(m + q)h$. for the different velocities in the third column of the table, are given in the fourth column of the same table. At the maximum velocity there is practically no line attached, as the action of a few feet may be neglected; and the whole resistance will be equal to 27.36 pounds, or equal to the weight of the shot in water; which is determined also from the expression,

$$V = \sqrt{\frac{2gW}{S.D(m+q)}},$$

from which

$$R = S.D(m+q)\frac{v^2}{2g} = W = 27.36 \text{ pounds.}$$

These resistances to the shot are given in the fourth column of the table, and the curve representing these resistances, in pounds, is given in diagrams marked (B), curve No. 1.

The values of $R' = W - S.D(m+q)h$. are given in the fifth column of Table I.

It will be seen that the resistance to the shot diminished very rapidly at first, and at the depth of two thousand fathoms it becomes only 0.5 of a pound, while the resistance upon the line increases with the depth, becoming at two thousand fathoms $27.36 - 0.5 = 26.8$ pounds.

For two thirty-two pound shot with the same line the resistances at the maximum velocity will be 54.72 pounds; and at two thousand fathoms, 1.97 pounds; the tension of the line in this case is, at two thousand fathoms, nearly equal to its bearing weight; which was only sixty pounds. How much further would it have gone without breaking, and thus *indicating bottom?*

The diagrams marked (*B*) represent the laws of the resistances to the sinker, or shot, and to the line, respectively. Those in black represent the resistance to the shot, in pounds, at depths fifty, one hundred and fifty, two hundred and fifty, &c. fathoms; the abscissar representing pounds. Curves (3) and (4) represent the resistances to the line at corresponding depths.

Diagrams, in every respect similar, have been made for the descent of sinkers of 126 and ninety-six pounds, attached to the ordinary deep-sea line, which is about four tenths of an inch in diameter; and also for weights of thirty-five and ninety-six pounds attached to a silk line one tenth of an inch in diameter. These latter results were obtained from the observations of Lieutenant-Commanding Joseph Dayman, who sounded across the Atlantic along the line proposed for the ocean telegraph, in the summer of 1857. The deductions from Lieutenant Dayman's experiments confirm those obtained from the observations of Lieutenants Lee and Berryman.

A glance at the tables or curves will show: 1st, that the velocity of a descending weight and line, even with the smallest line, diminishes very rapidly after it has attained its maximum, which always occurs very near the surface; 2d, that at the depth of about two thousand fathoms the velocity is hardly two feet per second; 3d, that at this point nearly the whole resistance is due to the line, the resistance to the sinker being only a fraction of a pound; 4th, the resistance upon the *line* increases with the depth, becoming nearly equal to the weight, at two thousand fathoms; 5th, comparing the resistances upon the line when attached to one thirty-two pound shot, and two thirty-two pound shot, respectively, these resistances for the same depth, or length of line, seem to be proportional to the squares of the velocities. To determine whether this law be true, Table IV. has been constructed, in which the ratios of the resistances in pounds at the same depth in the two cases are compared with the squares of the velocities. The ratios agree very nearly, the means differing by only a small fraction.

In Table V. the same result is obtained, with a larger line, the deep-sea line; the moving weights being 126 and ninety-six pounds.

We may therefore conclude that for the same length of line moving with different velocities, the resistances are proportional to the squares of the velocities.

To ascertain the law of variation with the length, I have taken those points, in the two cases, at which the velocities are the same, corresponding of course to different depths, or lengths. The results are given in Table III., where the ratios of the lengths are compared with the ratios of the resistances. From this table it appears that the resistances are directly proportional to the lengths. That is, if R represent the resistance upon a given length, moving with a velocity V, the resistance upon double the length moving with the same velocity will be $2\,R$.

This does not appear to hold true for all depths, for if we take the differences between the lengths corresponding to the same velocity, for different velocities, or what is the same thing, different depths, and compare these differences of length with the differences of resistances, the resistances appear to diminish with the depth. This is particularly the case with the deep-sea line, where a given pressure of the water will have a greater effect in diminishing the diameter. It would seem to be a result which ought to have been expected, namely, that in case of compressible lines, the pressure of the water, instead of increasing the resistance to motion, diminishes it, since the combined tension and pressure tend to diminish the diameter of the line; while, as we have seen, the change of density is so small that no sensible increase of resistance to motion among the particles is produced.

This fact I do not consider conclusively shown by the investigation, for although in the last two columns of Table VI. and VIII. the numbers expressing the ratios of resistances when compared with the ratios of the lengths, appear to diminish with the depth, yet, from a want of knowledge of the diameter and specific gravity of the lines used, it is impossible to detect very slight variations in the resistances. The conclusion is a natural one, however, that the diameter of the line being diminished by the combined pressure and tension, the resistance will diminish with the depth.

The observations which I have discussed are not sufficiently numerous to determine with certainty the influence of increasing or diminishing the diameter of the line. Taking the deep-sea line and the small line, seven hundredths of an inch in diameter, the resistances to the same length moving with the same velocity, appear to be proportional to the diameter. Table VII. The diameters were probably as one to three or four, but the diameter of the deep-sea line was not exactly given.

The resistance of the water upon a sounding line, may therefore be represented by the following expression,

$$R = (\text{constant}) \times \sqrt{S.l}.v^2,$$

in which

s represents the cross-section of the line.
l. " " length,
v. " " velocity.

The constant must be determined by experiment.

From these results we are justified in drawing the following conclusions: —

1st. The resistance to the sinker forming such a small part of the whole resistance at great depths, its form is not a matter of importance. 2d. With regard to its weight; the effect of a heavier sinker is to increase the velocity during the first part of the motion, which develops a greater amount of resistance upon the line, requiring of course a stronger line: with the line used in the experiments with two thirty-two pound shot, the resistance became so great at the depth of two thousand fathoms, that a slight addition to the velocity by means of a greater weight, would have broken the line; and this result generally attended all attempts to make very deep sea soundings with the line attached to three or four thirty-two pound shot. Thus increasing the weight requires a larger line and causes an increase of the resistance in the double ratio of the diameter of the line and the square of the increased velocity, the resulting velocity at great depths will therefore remain nearly the same; a result which is found in practice.

The experiments with one thirty-two pound shot attached to a small line, given above, probably involve the most favorable conditions, as far as regards the weight, form of weight, and diameter of the line.

3d. The uncertainty of knowing the exact instant of the weight striking the bottom, the effect of drift or currents, and the great loss of velocity, in the descent, requiring so much time for a single cast, are therefore difficulties which cannot be overcome by this mode of sounding, and there is a limit at two or three thousand fathoms depth, beyond which the method practically fails; and even at less depths, large errors must enter into the results.

The effect of increasing the specific gravity of the line, as in making use of wire, is unfavorable, since at a certain depth the weight of the wire alone must overcome the resistance offered by the water, and it would run out of its own weight. The depth at which this would occur for a wire seven hundreths of an inch in diameter is about 1,700 fathoms; and for a wire three tenths of an inch in diameter, 400 fathoms.

The same difficulty occurs when a large hempen line is used; a line two inches in diameter of this material would run out of its own weight, with a velocity of 4 feet per second, at the depth of 1,900 fathoms. An instance of this kind is given in the experiments of Lieutenant Joseph Dayman, R. N., in sounding across the Atlantic in 1857.

Table No. IX gives the results of the experiment referred to; when the line was hauled in, two hundred fathoms of it were coiled upon the sinker, the latter having struck bottom at 2,200 fathoms.

The next consideration is the mode of noting or measuring the depth within the limits which are accessible. For this purpose, several plans have been followed, none of which have been so successful that the degree of accuracy attained, or the probable error of the determination, might be estimated.

The most natural method is to measure the length of the line paid out; the errors arising from the drifting of the vessel and currents acting upon a long length of line in the water, may in this case amount to as much as the depth, and the method has accordingly been rejected entirely, where there is any current or drift. Computing the depth from the time and rate of descent is another mode, which, in the manner in which it has been applied, is unreliable; especially on account of the difficulty of noting the exact instant when the sinker strikes the bottom.

The most successful plan thus far is to attach a self-registering indicator to the weight, the indicator being brought back to the surface and the weight detached at the bottom.

The indicator heretofore used is known as "Massey's Indicator," and although theoretically well adapted to the purpose to which it is applied, yet on account of faults in its construction, the results obtained by its use, at great depths, have not been satisfactory; and in fact are of little more value than the determinations by the length of line paid out. In the soundings across the Atlantic, in 1857, Lieutenant Commanding Dayman rejects the determinations by the indicator, after numerous experiments to ascertain what he called its "index errors." The difficulty seems to be in the small surface given to the helix, the great size of the axis upon which the helices are placed, the friction of the wheel work, and, generally, the clumsy construction of the machine, which prevents the helix from revolving with equal facility at all velocities. The comparisons made by Lieutenant Dayman, show that different machines attached to the same sounding weight will give very different indications; and the same machine will give different indications when attached to different weights and different lines.

The "Current Indicator," invented by Mr. Joseph Saxton, of the Office of Weights and Measures, might, by very slight modifications, be applied to the purpose of sounding, and would be, in every respect, superior to Massey's.

With all of its imperfections, however, Massey's Indicator furnishes better results, when carefully used, than the method of measuring the depth by the line out; and for this reason I think the soundings of Lieutenant Berryman, where he used the indicator, on the line of the Atlantic telegraph, are much nearer the truth than those of Lieutenant Dayman.

In both cases the probable errors are very large, amounting in many instances, I think, to several hundred fathoms.

The idea of a "plateau" existing between Newfoundland and Ireland is not warranted, it seems to me, by any observations that have yet been made. The range of errors may be as great as five hundred fathoms, which would permit of the existence of a submarine mountain on this line, half as high as Mount Washington. A true section of the Atlantic can only be determined by improved modes of sounding, since from the preceding discussion, we may be warranted in the conclusion that it is practically impossible to determine the greatest depths of the ocean by the methods now in use.

TABLE I. — SHOWING THE RATES OF DESCENT AND RESISTANCES, IN POUNDS, UPON THE SINKER AND LINE IN SOUNDING WITH ONE AND TWO THIRTY-TWO POUND SHOT, ATTACHED TO A LINE .07 OF AN INCH IN DIAMETER.

Depth, Faths.	One Thirty-two Pound Shot.						Two Thirty-two Pound Shot.					
	No. of Casts.	Intervals of Times of Descent.		Velocity.	Resistance to Sinker.	Resistance to Line.	No. of Casts.	Intervals of Times of Descent.		Velocity.	Resistance to Sinker.	Resistance to Line.
		Min.	Sec.	Feet.	Pounds.	Pounds.		Min.	Sec.	Feet.	Pounds.	Pounds.
0				16.41	27.36						54.72	
50				8.83	8.15	19.15				12.50	32.70	22.02
100		1	08					0	48			
150				6.66	4.64	22.72				9.40	18.49	36.23
200		1	30					1	04			
250				5.41	3.06	24.30				7.32	11.21	43.51
300	29	1	51				27	1	22			
350				4.65	2.26	25.10				6.45	8.70	46.02
400	29	2	09				28	1	33			
450				4.14	1.81	25.55				5.61	6.58	48.14
500	29	2	25				28	1	47			
550				3.77	1.50	25.86				5.36	6.01	48.71
600	29	2	39				28	1	52			
650				3.55	1.33	26.08				4.81	4.98	49.74
700	27	2	49				22	2	03			
750				3.37	1.20	26.16				4.76	4.85	49.87
800	26	2	58				18	2	06			
850				3.11	1.02	26.34				4.48	4.30	50.42
900	27	3	13				16	2	14			
950				2.94	.91	26.45				4.23	3.83	50.89
1000	26	3	24				16	2	22			
1050				2.84	.85	26.51				4.03	3.48	51.24
1100	22	3	31				16	2	29			
1150				2.66	.75	26.65				3.85	3.17	51.55
1200	21	3	45				14	2	36			
1250				2.66	.75	26.65				3.73	2.98	51.74
1300	19	3	45				11	2	41			
1350				2.53	.68	26.68				3.45	2.55	52.17
1400	18	3	57				11	2	54			
1450				2.43	.62	26.74				3.44	2.53	52.19

TABLE I.—*continued.* SHOWING THE RATES OF DESCENT AND RESISTANCES, IN POUNDS, UPON THE SINKER AND LINE IN SOUNDING WITH ONE AND TWO THIRTY-TWO POUND SHOT, ATTACHED TO A LINE .07 OF AN INCH IN DIAMETER.

Depth, Faths.	One Thirty-two Pound Shot.						Two Thirty-two Pound Shot.					
	No. of Casts.	Intervals of Times of Descent. Min.	Sec.	Velocity. Feet.	Resistance to Sinker. Pounds.	Resistance to Line. Pounds.	No. of Casts.	Intervals of Times of Descent. Min.	Sec.	Velocity. Feet.	Resistance to Sinker. Pounds.	Resistance to Line. Pounds.
1500	19	4	07				11	2	56			
1550				2.37	.59	26.77				3.26	2.28	52.44
1600	17	4	13				10	3	04			
1650				2.31	.56	26.80				3.23	2.23	52.49
1700	17	4	20				8	3	06			
1750				2.25	.53	26.83				3.19	2.18	52.54
1800	17	4	26				7	3	08			
1850				2.21	.51	26.85				3.08	2.03	52.69
1900	15	4	31				7	3	15			
1950				2.15	.48	26.88				3.06	2.00	52.72
2000	12	4	39				5	3	16			
2050				2.09	.44	26.92				2.99	1.91	52.81
2100	8	4	47				5	3	21			
2150				2.08	.43	26.93				2.93	1.84	52.88
2200	6	4	48				3	3	25			
2250				2.04	.41	26.95				2.84	1.73	52.99
2300	5	4	54				2	3	31			
2350				1.91						2.83	1.71	53.01
2400	4	5	13				2	3	32			
2450				1.99						2.67	1.53	53.19
2500	4	5	01				2	3	45			
2550				2.03						2.69	1.55	53.17
2600	3	4	56				1	3	43			
2650				1.83								
2700	3	5	27									
2750				2.22								
2800	1	4	30									
2850				2.11								
2900	1	4	44									
2950				2.14								
3000	1	4	40									

TABLE II. — SHOWING THE RATES OF DESCENT AND RESISTANCES, IN POUNDS, UPON THE SINKER AND LINE, IN SOUNDING WITH 96 AND 126 POUND WEIGHTS, ATTACHED TO A DEEP-SEA LINE $\frac{4}{10}$ OF AN INCH IN DIAMETER.

Depth, Faths.	96 Pound Weight. Deep-sea Line.					126 Pound Weight. Deep-sea Line.				
	Intervals of Times of Descent.		Velocity.	Resistance to Sinker.	Resistance to Line.	Intervals of Times of Descent.		Velocity.	Resistance to Sinker.	Resistance to Line.
	Min.	Sec.	Feet.	Pounds.	Pounds.	Min.	Sec.	Feet.	Pounds.	Pounds.
0			27.26	82.03				26.99	106.26	
50			11.54	14.39	67.64			13.93	29.24	80.02
100	0	52				0	43			
150			7.41	6.08	75.95			8.44	10.68	98.58
200	1	21				1	11			
250			5.94	3.91	78.12			6.82	6.97	102.29
300	1	41				1	28			
350			5.17	2.96	79.07			5.60	4.70	104.56
400	1	56				1	47			
450			4.69	2.44	79.59			5.31	4.29	104.97
500	2	08				1	53			
550			4.35	2.10	79.93			5.04	3.81	105.45
600	2	18				1	59			
650			4.11	1.87	80.16			4.89	3.59	105.67
700	2	26				2	05			
750			3.89	1.64	80.39			4.48	3.10	106.16
800	2	34				2	14			
850			3.73	1.54	80.49			3.98	2.38	106.88
900	2	41				2	31			
950			3.61	1.44	80.59			4.19	2.65	106.61
1000	2	46				2	23			
1050			3.47	1.33	80.70			3.98	2.38	106.88
1100	2	53				2	30			

TABLE II.—*continued.* SHOWING THE RATES OF DESCENT AND RESISTANCES, IN POUNDS, UPON THE SINKER AND LINE, IN SOUNDING WITH 96 AND 126 POUND WEIGHTS, ATTACHED TO A DEEP-SEA LINE $\frac{4}{10}$ OF AN INCH IN DIAMETER.

Depth, Faths.	96 Pound Weight. Deep-sea Line.				126 Pound Weight. Deep-sea Line.			
	Intervals of Times of Descent. Min. Sec.	Velocity. Feet.	Resistance to Sinker. Pounds.	Resistance to Line. Pounds.	Intervals of Times of Descent. Min. Sec.	Velocity. Feet.	Resistance to Sinker. Pounds.	Resistance to Line. Pounds.
1150		3.37	1.26	80.77		4.05	2.44	106.82
1200	2 58				2 28			
1250		3.33	1.23	80.80		3.92	2.29	106.97
1300	3 00				2 33			
1350		3.37	1.27	80.76		3.87	2.23	107.03
1400	2 58				2 35			
1450		3.24	1.17	80.86		3.61	1.94	107.32
1500	3 05				2 46			
1550		3.11	1.08	80.95		3.53	1.85	107.41
1600	3 13				2 50			
1650		3.00	1.00	81.03		3.41	1.72	107.54
1700	3 16				2 56			
1750		2.90	.93	81.10				
1800	3 16							
1850		2.80	.87	81.16				
1900	3 27							
1950		2.70	.81	81.22				
2000	3 43							
2050		2.70	.81	81.22				
2100	3 43							
2150		2.47	.68	81.35				
2200	4 03							

TABLE III. — SHOWING INFLUENCE OF DIFFERENT LENGTHS OF LINE MOVING WITH THE SAME VELOCITY. RATIOS OF LENGTHS TO RATIO OF RESISTANCES.

Velocity.	Depth.		R	R'	Ratio of Lengths.	$\frac{R'}{R}$	Depth.		R	R'	Ratio of Lengths.	$\frac{R'}{R}$
	1 32 lb. Shot.	2 32 lb. Shot.	1 32 lb. Shot.	2 32 lb. Shot.			96 lb. Weight.	126 lb. Weight.	96 lb. Weight.	126 lbs. Weight.		
Feet.	Fath.	Fath.	Pounds.	Pounds.			Fath.	Fath.	Pounds.	Pounds.		
8.	70	200			2.857		120	175			1.458	
7.5	100	220			2.200		140	200	71.6	96.8	1.428	1.352
7.	120	280			2.325		170	240	73.8	100.0	1.411	1.355
6.5	150	340	21.7	47.6	2.266	2.193	200	280	75.2	101.5	1.400	1.350
6.	190	420	23.2	48.5	2.211	2.090	240	330	76.6	102.7	1.375	1.341
5.5	230	510	24.5	49.3	2.217	2.012	300	400	77.8	103.9	1.333	1.336
5.	290	640	25.7	50.0	2.206	1.945	380	500	78.8	104.9	1.315	1.331
4.5	350	820	26.7	50.8	2.342	1.902	520	660	79.6	105.7	1.269	1.328
4.	480	1080	28.2	51.8	2.250	1.837	710	880	80.2	106.5	1.239	1.328
3.5	650	1400	29.7	52.4	2.153	1.764	1080	1340	80.7	107.2	1.240	1.328
3.	880	1900	31.3		2.159		1650		81.2			

TABLE VI. — SHOWING INFLUENCE OF LENGTHS AT DIFFERENT DEPTHS.

Velocity.	Depth.		Difference.	$R' - R$	R	Ratio of Lengths.	Ratio of Resistances.	Depth.		Difference.	$R' - R$	R	Ratio of Lengths.	Ratio of Resistances.
	1 32 lb. Shot.	2 32 lb. Shot.						96 lb. Weight.	126 lb. Weight.					
Feet.	Faths.	Faths.	Faths.	Pounds.	Pounds.			Faths.	Faths.	Faths.	Pounds.	Pounds.		
8.	70	200						120	175					
7.5	100	220						140	200		25.2	71.6		2.841
7.	120	280						170	240		26.2	73.8		2.817
6.5	150	340	190	25.9	21.7			200	280	80	26.3	75.2	2.50	2.859
6.	190	420	230	25.3	23.2	1.211	1.090	240	330	90	26.1	76.6	2.66	2.938
5.5	230	510	280	24.8	24.5	1.217	1.012	300	400	100	26.1	77.8	3.00	2.981
5.	290	640	350	25.3	25.7	1.207	0.984	380	500	120	26.1	78.8	3.17	3.019
4.5	350	820	470	24.1	26.7	1.343	0.903	520	660	140	26.1	79.6	3.71	3.049
4.	480	1080	600	23.6	28.2	1.250	0.837	710	880	170	26.3	80.2	4.18	3.072
3.5	650	1400	750	22.7	29.7		0.764	1080	1340	260	26.5	80.7	4.16	3.045
3.	880	1900	1020		31.3			1650				81.2		

Table VIII.—Showing Influence of Lengths at different Depths.

Velocity.	Depth.		Difference.	$R'-R$	R'	Ratio of Lengths.	Ratio of Resistances.	Depth.		Difference.	$R'-R$	R'	Ratio of Lengths.	Ratio of Resistances.
	1 32 lb. Shot.	2 32 lb. Shot.						96 lb. Weight.	126 lb. Weight.					
Feet.	Faths.	Faths.	Faths.	Pounds.	Pounds.			Faths.	Faths.	Faths.	Pounds.	Pounds.		
8.	70	200						120	175					
7.5	100	220						140	200			96.8		
7.	120	280						170	240			100.0		
6.5	150	340	190	25.9	47.6	1.789	1.838	200	280	80	26.2	101.5	3.500	3.874
6.	190	420	230	25.3	48.5	1.826	1.917	240	330	90	26.1	102.7	3.666	3.935
5.5	230	510	280	24.8	49.3	1.821	1.988	300	400	100	26.1	103.9	4.000	3.981
5.	290	640	350	25.3	50.0	1.829	1.976	380	500	120	26.3	104.9	4.166	3.988
4.5	350	820	470	24.1	50.8	1.745	2.108	520	660	140	26.1	105.7	4.714	4.049
4.	480	1080	600	23.6	51.8	1.800	2.195	710	880	170	26.3	106.5	5.176	4.049
3.5	650	1400	750	22.7	52.4	1.866	2.308	1080	1340	260	26.3	107.2	5.154	4.076
3.	880	1900	1020					1650						

Table VII.—Comparison of Resistances upon the same Lengths of Lines of different Diameters, moving with the same Velocity.

Velocity.	Deep Sea Line.			Small Line.			Remarks
	Depth.	R	Resistance upon one fathom.	Depth.	R	Resistance upon one fathom.	
	96 lb. Weight.			1 32 lb. Shot.			
Feet.	Fathoms.	Pounds.	Pounds.	Fathoms.	Pounds.	Pounds.	
8.	120			70			
7.5	140	71.6		100			
7.	170	73.8		120			
6.5	200	75.2	0.3765	150	21.7	0.144	
6.	240	76.6	0.3191	190	23.2	0.122	
5.5	300	77.8	0.2593	230	24.5	0.106	
5.	380	78.8	0.2073	290	25.7	0.088	
4.5	520	79.6	0.1530	350	26.7	0.076	
4.	710	80.2	0.1129	480	28.2	0.058	
3.5	1080	80.7	0.0747	650	29.7	0.045	
3.	1650	81.2	0.0492	880	31.3	0.035	

Table IV. — Showing Velocities and Resistances at different Depths and Comparisons of Squares of Velocities to Resistances. Observations made with one and two 32 Pound Shot. Small Line.

Depth, Faths.	One Thirty-two Pound Shot.		Two Thirty-two Pound Shot.		Ratio of Squares of Velocities.	Ratio of Resistances to Line.
	Velocity.	Resistance to Line.	Velocity.	Resistance to Line.		
	Feet.	Pounds.	Feet.	Pounds.		
0	16.41		16.41		1.000	
50	8.83	19.15	12.50	22.02	2.004	1.150
150	6.66	22.72	9.40	36.23	1.992	1.595
250	5.41	24.30	7.32	43.51	1.831	1.790
350	4.65	25.10	6.45	46.02	1.924	1.833
450	4.14	25.55	5.61	48.14	1.843	1.884
550	3.77	25.81	5.36	48.71	2.021	1.887
650	3.55	26.03	4.88	49.74	1.890	1.911
750	3.37	26.16	4.76	49.87	1.995	1.906
850	3.11	26.34	4.48	50.42	2.084	1.914
950	2.94	26.45	4.23	50.89	2.070	1.924
1050	2.84	26.51	4.03	51.24	2.014	1.933
1150	2.66	26.65	3.85	51.55	2.095	1.934
1250	2.66	26.65	3.73	51.74	1.966	1.941
1350	2.53	26.68	3.45	52.17	1.859	1.911
1450	2.43	26.74	3.44	52.19	2.004	1.907
1550	2.37	26.77	3.26	52.44	1.892	1.914

TABLE V.—SHOWING VELOCITIES AND RESISTANCES AT DIFFERENT DEPTHS, AND COMPARISON OF SQUARES OF VELOCITIES TO RESISTANCES. OBSERVATIONS MADE WITH 96 AND 126 POUND WEIGHTS. DEEP-SEA LINE.

Depth, Faths.	96 Pound Weight.		126 Pound Weight.		Ratios of Squares of Velocities.	Ratios of Resistance to Line.
	Velocity.	Resistance to Line.	Velocity.	Resistance to Line.		
	Feet.	Pounds.	Feet.	Pounds.		
0	27.26		26.99			
50	11.54	67.64	13.98	80.02	1.457	1.183
150	7.41	75.95	8.44	98.58	1.297	1.298
250	5.94	78.12	6.82	102.29	1.318	1.310
350	5.17	79.07	5.60	104.56	1.173	1.322
450	4.69	79.59	5.31	104.97	1.282	1.319
550	4.35	79.93	5.04	105.45	1.342	1.319
650	4.11	80.16	4.89	105.67	1.414	1.318
750	3.89	80.39	4.48	106.16	1.326	1.321
850	3.73	80.49	3.98	106.88	1.138	1.328
950	3.61	80.59	4.19	106.61	1.347	1.323
1050	3.47	80.70	3.98	106.88	1.316	1.325
1150	3.37	80.77	4.05	106.82	1.444	1.322

TABLE IX. — SHOWING THE RATES OF DESCENT, VELOCITY, RESISTANCE TO SINKER AND LINE, AND WEIGHT OF LINE IN WATER; FROM OBSERVATIONS MADE BY LIEUT.-COM. JOSEPH DAYMAN, R. N.

Diameter of Line 2 *inches*; *specific gravity* 1.3; *weight* 96 *pounds.*

Depth.	Intervals of Times of Descent.	Velocity.	Resistance to Sinker.	Resistance to Line.	Weight of Line in Water.	Remarks.
Faths.	Min. Sec.	Feet.	Pounds.	Pounds.	Pounds.	
0		27.26	82.0			On bringing the apparatus back to the surface after 2,400 fathoms had run out, 200 fathoms of line was found coiled upon the sinker. The depth was probably not greater than 2,000 fathoms.
100	1 24	7.14	5.62	100.6	23.	
200	1 17	4.35	4.33	119.4	46.	
300	2 01	"	4.33	138.1	69.	
400	2 07	"	"	156.8	92.	
500	2 20	"	"	175.5	115.	
600	2 18	"	"	194.2	138.	
700	"	"	"	212.9	161.	
800	"	"	"	231.6	184.	
900	"	"	"	250.3	207.	
1000	"	"	"	269.0	230.	
1100	"	"	"	287.7	253.	
1200	"	"	"	296.4	276.	
1300	"	"	"	325.1	299.	
1400	"	"	"	343.8	322.	
1500	"	"	"	362.5	345.	
1600	"	"	"	381.2	368.	
1700	"	"	"	399.9	391.	
1800	"	"	"	418.6	414.	
1900	"	"	"	437.3	437.	

6. ABSTRACT OF THE PRINCIPAL RESULTS OF THE MAGNETIC OBSERVATIONS OF THE SECOND GRINNELL EXPEDITION, IN 1853–5, AT VAN RENSSELAER HARBOR AND OTHER POINTS ON THE WEST COAST OF GREENLAND, BY ELISHA K. KANE, U. S. N. Presented by Prof. A. D. BACHE (from a reduction and discussion by C. A. SCHOTT), of the U. S. Coast Survey.

PROF. BACHE introduced this paper by a few remarks. The scientific reader of the narrative of the second Arctic Expedition, the one which Dr. Kane commanded, would be struck by the fact that while in the preface Dr. Kane disclaimed all pretensions of a scientific character for his work, it contained, nevertheless, some of the most important contributions to our knowledge of the natural history and natural phenomena of the interesting regions visited by the hardy explorers. Dr. Kane appreciated highly all the relations direct and indirect which science has to an exploring expedition. He was always careful to surround himself by those who could in their special departments make valuable observations, while his own rare administrative capacity always gave them opportunity for the exercise of their abilities. Himself an admirable observer, and well trained in many subjects, he was always on hand to direct or to assist as the occasion might require. The labors, in physical observation, of Dr. Kane and his associates, had in Prof. Bache's opinion, few parallels, when the difficulties to be surmounted and the results produced are considered. These results the early death of Dr. Kane had prevented him from recording and presenting to the scientific public in a complete form.

It was to be feared that some of them could not now be recovered, so scattered were the papers in the journeyings of the explorer, which terminated in his loss to the country and to science. The results which he (Professor Bache) had been able to examine, only made him more desirous to procure the others. The magnetic observations which form the subject of the present communication have proved of great interest, and he hoped that the tidal observations, as well at the winter-quarter station as those made during the voyage, might yet be recovered. Prof. Bache had placed these observations in the hands of Mr. Schott for discussion, as he had been selected for similar service by Dr.

Kane himself, and felt a strong interest in his work. Prof. Bache intended thus to preserve as far as possible the monuments of the labors of one whom he admired as an explorer, to whom he was warmly attached as a friend, and to whose family he was bound by the strongest ties of affection.

It is proposed to give an outline of the principal magnetical results obtained by Dr. Kane on the coast of Greenland during the years 1853–5, an account of which is supposed to be of interest to the Association. In regard to the high magnetic and geographical latitudes in which the observations have been made, as well as the completeness of the observations for diurnal changes of the horizontal needle at a station before unknown to geography, they deserve the attention of those engaged in the study of the law of the changes of the magnetic elements in the arctic regions. At the late meeting of the British Association at Dublin, a resolution was passed that application be made to government to send a vessel to the vicinity of Mackenzie River to make a series of magnetic observations with special reference to the determination of the laws now known to rule the magnetic storms. Further, Captain Younghusband, R. N., remarks in his discussion of the observations of Sir John Richardson, that so few observations of the diurnal variation of the declination in high latitudes are, up to this time at command, that not even an approach can be made towards indicating a general law of the phenomena in such localities.

In reference to the circumstances under which the present observations were obtained, I need but refer to the narrative of the expedition.

The instruments, a Jones's unifilar magnetometer, and a Barrow's dip cirle, were loaned to the expedition by the United States Coast Survey and the Smithsonian Institution.

The latitude of the winter quarter where most of the observations were made, is in 78° 37′ to the northward and eastward of Cape Alexander (beyond Smith's Sound).

Diurnal Range of the Declination.— It has been deduced from observations in seventeen days, the instrument being read every five minutes during each twenty-four hours, during the months of January, February, and March, 1854. The following table contains the total range observed, expressed in scale divisions, the value of which is 0′.8.

DAILY RANGE OF THE DECLINATION.

Date. 1854.	Range.
	d.
January 10–11	73.0
" 13–14	166.0
" 18–19	284.0
" 24–25	91.0
" 27–28	87.5
" 31–32	162.0
February 3–4	199.0
" 7–8	119.5
" 10–11	309.0
" 14–15	365.0
" 17–18	172.0
" 21–22	101.0
" 24–25	290.5
March 0–1	305:0
" 3–4	235.0
" 7–8	122.0
" 22–23	76.0

The mean diurnal total range observed during the above period, becomes 2° 28′.6, and the greatest diurnal range observed took place on the 14th and 15th of February, and amounted to 4°.52′.0. For comparison with similar quantities at other high latitude stations, we may take Lake Athabasca, where the greatest range in any one day between Oct. 1843 and Feb. 1844, was 2° 35′; it happened Oct. 16, 1843; at Fort Simpson, the maximum range was 7° 27′, observed on the 16th of April, 1854, in a series of observations extending over April and May, 1844. The *mean* diurnal range at Athabasca was was 31′.4, and at Fort Simpson 1° 12′. At Port Bowen, in 1825, the diurnal range appears to have been seldom lower than one degree, and sometimes to have amounted to five, six, and even seven degrees. This place is in latitude 73° 14′, and only differing 16° in magnetic declination from that at Van Rensselaer Harbor.

If we classify the ranges according to their magnitude, we find but once a range less than one degree; six between one and two degrees, and ten between two and five degrees, none beyond five degrees. The diurnal range in the winter months is probably below the mean value of the year.

Diurnal Inequality of the Declination was obtained from hourly reading during seventeen days in, January, February, and March; the monthly results are presented in a diagram. The following table contains the diurnal inequality at Van Rensselaer Harbor and at Greenwich during the same days.

Local Mean Time.	Van Rensselaer.	Greenwich.	Local Mean Time.	Van Rensselaer.	Greenwich.	Local Mean Time.	Van Rensselaer.	Greenwich.	Local Mean Time.	Van Rensselaer.	Greenwich.
h.	′	′	h.	′	′	h.	′	′	h.	′	′
5	+24.3	—0.5	11	—21.8	—4.5	17	—23.1	—0.3	23	+29.6	+3.5
6	+11.8	—2.5	Midn.	—28.2	—4.1	18	—10.5	+0.6	Noon.	+37.8	+5.8
7	— 3.2	—1.6	13	—28.3	—3.1	19	+ 0.8	—0.4	1	+35.0	+5.8
8	—13.5	—3.9	14	—29.0	—0.8	20	+12.2	+0.5	2	+26.3	+5.0
9	—20.8	—4.5	15	—27.8	—0.3	21	+17.4	+1.0	3	+21.2	+3.9
10	—21.0	—5.1	16	—27.8	+0.5	22	+30.6	+2.3	4	+ 7.0	+2.6

A negative sign indicates a deflection to the east, a positive one a deflection to the west of the mean position. The diurnal inequality at the two stations presents in general the same characteristic features, namely, the principal deflection to the west shortly after noon, and the opposite eastern position about midnight; in regard to this inequality, therefore, the motion of the magnet at Van Rensselaer Harbor follows the same law (in general) as recognized in lower latitudes. The extreme westerly position is attained at noon, after this hour the westerly declination diminishes gradually, with an exception of a period of opposite motion of very limited range between the hours of four and five. The easterly extreme is reached two hours after midnight. Whether the small irregularity, just noticed, producing apparently a secondary minimum and maximum is real, or only caused by accidental deviations of the (few) observations under discussion, cannot be decided with certainty. The motion from fourteen hours to twenty-four hours is performed with great uniformity. Thus while the diurnal motion agrees with that observed at Lake Athabasca, Fort Simpson, Sitka, Toronto, &c., it shows no trace of that marked deviation observed at Reikiavik in Iceland, or at Fort Confidence. In 1824, at the Whalefish Islands the maximum westerly deviation happened at about a quarter past one o'clock, P. M.; the time of the maximum easterly deflection has not been determined. At Port Bowen the maximum westerly variation appears to have occurred between the hours of 10 A. M. and 1 P. M.; the mean result being 11 hours and 49 minutes; the greatest deflection of the north end of the needle to the eastward, took place between 8 P. M. and 2 A. M., the mean hour being 10 P. M. These observations were made during January, February, March, and April, 1825.

The range of the *mean* diurnal inequality at Van Rensselaer was 1° 07′, and during the same time at Greenwich, 10′.9.

Analysis of the Disturbances of the Declination. — The mean disturbance for each of the twenty-four hours has been obtained by comparing the monthly mean with each hourly reading, let $\varDelta$ equal this difference, n the number of hourly readings, and m the mean disturbance, then $m = \pm \sqrt{\frac{\Sigma \varDelta^2}{n-1}}$. This quantity is analogous to the mean error of an observation. At Van Rensselaer Harbor the *mean* disturbance force is greater than at Lake Athabasca or Fort Simpson, and pretty regular during two well-marked periods, as shown by the following table:—

LOCAL MEAN TIME.

h. 5	h. 6	h. 7	h. 8	h. 9	h. 10	h. 11	Midnight.	h. 13	h. 14	h. 15	h. 16
±31′	41	37	47	49	50	46	52	51	47	50	±53′

h. 17	h. 18	h. 19	h. 20	h. 21	h. 22	h. 23	Noon.	h. 1	h. 2	h. 3	h. 4
±49	42	54	48	46	31	46	60	46	39	42	±41′

The disturbing force is least during the day (if such an expression is admissible in this case) from 10 A. M. to 7 P. M., and greater but equally regular during the hours of the night (?) from 8 P. M. to 8 or 9 A. M. At Lake Athabasca, the hours of least disturbance are between 9 A. M. and 7 P. M., and at Fort Simpson from 10 A. M. to 7 P. M. Captain Lefroy in his discussion of the disturbances of the declination, remarks: "There are indications in each of the three curves (for Lake Athabasca, Toronto, and Sitka) of a small increase in the mean disturbance about noon." At Van Rensselaer we find the maximum disturbances at this very hour preceded and followed by quite small values. Further coincidences of the disturbing force can be noticed at 5 P. M., at which hour at Van Rensselaer, Lake Athabasca, and Sitka, the minimum disturbance has been observed.

By adding the squares of the differences for each hour of the day and month we find the mean monthly disturbance by the formula $\sqrt{\frac{[\Sigma \Delta^2]}{N - 24}}$, viz., for Jan. ± 30′, Feb. ± 65′, and March ± 40′.

At Lake Athabasca, the greatest mean monthly disturbance occurred in January; but at Toronto, on the contrary, the months of January and June are those of the minimum value.

Hitherto the recognition and separation of the disturbed observations have been effected by an arbitrary process of fixing upon a certain deviation from the mean as the greatest allowable departure, and regarding all observations beyond this limit as disturbances. In the present case a more definite idea has been introduced by the application of Professor Peirce's criterion for the rejection of doubtful observations, or what is here equivalent for the separation of those following a different law, namely, the disturbances. The criterion shows that deviations over 1° 38′ must be recognized as disturbances; twenty-three of such were found averaging one in every eighteen values; at Toronto this ratio was 1 in 17.

Excluding the disturbed values, the mean diurnal inequality thereby underwent no material change. Eleven deflections were towards the east, and twelve towards the west.

Aurora Borealis.—In conformity with the supposed periodicity of this phenomenon as recognized by Professor Olmsted, no brilliant and complete auroras have been seen; with an exception of a few, they may all be placed in his fourth class, to which the most simple forms of appearances have been referred. Nineteen auroral displays have been noticed in all, and in no case did the needle indicate any disturbance.

Term-day Observations.—A term in each month from January to July, 1854, has been observed, and the results were compared with corresponding readings at Greenwich and Washington; no remarkable disturbance common to these places, was observed. A remarkable absence of disturbances of any magnitude, and a small diurnal range of motion at the time of the vernal equinox, are exhibited by the March curve.

The *absolute declination* was determined June 9, 14, and 26, 1854, and was found 108° 12′ west.

The *magnetic inclination* was determined at ten stations; at each the polarity of the needle was reversed; the results have been put together in the following table:—

TABLE OF MAGNETIC INCLINATION.

			° ′
I.	New York,	May, 1853,	72 55.6
II.	Fiskernaes,	"	80 41.3
III.	Fiskernaes harbor,	June,	80 53.0
IV.	Saikatle,	July,	80 56 (approx.)
V.	Sukkertoppen,	"	80 49.7
VI.	Proven,	"	82 57.0
VII.	Upernavik,	"	83 41.5
VIII.	Bedevilled Reach,	August,	85 08.0
IX.	Marshall Bay,	Sept.	84 49
X.	Van Rensselaer harbor,	Jan. 1854 to May, 1855,	84 45.8

The mean value at the last station referring to June, 1854.

Magnetic intensity.—For the determination of the magnetic intensit at the winter quarters a number of observations of deflections an vibrations were observed. The observations extend over the time fro January, 1854, to May, 1855. The moment of inertia of the magnet was carefully determined at Washington after the close of the expedition; twenty-five separate values were deduced for the magnetic moment and horizontal intensity. The mean horizontal force, in absolute measure, corresponding in time to June, 1854, was found = 1.139, and when multiplied by the secant of the mean dip, the total force was found to equal 12.48.

At Hakluyt Island, during the escape of the party, the horizonta intensity was found = 1.344, and near Cape York = 1.573, in the summer of 1855.

A detailed account of the observations and results is intended to be given elsewhere.*

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* See volume of the Smithsonian Contributions to Knowledge, for 1858.

7. ON AN EARTHQUAKE IN WESTERN NEW YORK. By Prof. Charles E. WEST, of Buffalo, New York.

AN earthquake was felt in Western New York, on the 23d of October last. The following pages contain such statistics respecting it as have come to my knowledge.

It occurred in Buffalo, at a quarter past three o'clock, P. M., and was violent, compared with other earthquakes in the Northern States. I was seated in a chair with my head leaning against the mantel of the fireplace, when the shock occurred, and so great was its violence, as to throw me forward to my feet. My first impression was that the chimney had fallen. I hastened into the street to see if that was the case, and found that others had left their houses for a similar reason. A young lady belonging to my family was standing by the wall of the rotunda — which is built of stone, and is some sixty feet high, — and so strong was her impression that the wall was falling, she rushed out of the place in great fear.

The principal of one of our public schools informs me, that he was in the third story of the school-house at the time; that the walls vibrated for a minute, and that the vibration increased and diminished, then increased and diminished. The supreme court of the city was in session, and so great was the alarm produced by the shock that most of the persons in attendance left in great haste. A mechanic who was present said that the walls swayed four inches from a vertical line. A person walking in the street remarked that the noise produced by the shock seemed to him like the dumping of coal upon the pavement. Another resident of the city said the roar seemed as though it were produced by a multitude of carriages rapidly driven under ground. It appeared to him to come from the north-east and pass to the south-west.

Many examples of a similar character might be given to show the nature of this earthquake, as experienced by the inhabitants of Buffalo, but sufficient have been cited for our purpose. Houses were shaken, door-bells were rung, crockery rattled upon the shelves, the people were alarmed.

I have received communications from persons living in neighboring towns, which will aid us in tracing the character and extent of the earthquake wave. The shock was generally severe throughout this county. Farmers at work in the fields were much alarmed. An interesting fact is related of a farmer living in Aurora, a town sixteen miles south-east of Buffalo. He was digging potatoes in his field, at the time of the earthquake, and so powerful was the shock, that he instinctively leaned upon his hoe-handle, and while in this posture, he observed the dirt shake back and forth over the top of the hoe, which was partially buried in the soil.

J. Henning, Esq., editor of one of the Toronto, Canada West, papers, writes: "In reply to your note of yesterday, inquiring respecting the supposed shock of an earthquake in Toronto on the 23d October last, I may say, that I have been unable to find any one who felt such a shock. Indeed, I do not think that it came in the direction of this city; but extended in a line through Buffalo and the central portions of the State of New York. It is said that it was experienced sensibly in Hamilton, Canada West, at three o'clock, P. M.; that there were two distinct shocks at intervals of ten seconds, accompanied with a rumbling noise. This is possible; as it was also felt in another portion of the same ridge of hills at Queenstown. In Port Hope and Peterboro' it was felt, or I am inclined to think, fancied to be felt, by some parties. There was no mention of it in those places until after the account of its visit to Buffalo was published. After its tricks in the latter city, several other localities claimed the honor of a visit. I think on the whole, the Hamilton hills bounded its progress in our direction, still I may be in error."

Rev. Mr. Cook of Lewiston writes: "It occurred here at the same time as in Buffalo. The noise was very distinct, as of a train of cars; and the motion was such as to cause a perceptible undulation in our dwellings, and to make dishes and stoves rattle audibly. The same phenomena attended its appearance at about the same time at St. Catharines, twelve miles west of this."

Judge Dayton of Lockport writes: "The earthquake on the 23d of October was felt here very sensibly, though probably less so than at Buffalo. I was myself in the cars between Lockport and Niagara Falls at the time, and of course did not notice it. At the county clerk's office in this village, it is said to have been preceded by a low rumbling noise not unlike the heavy rumbling of a train of cars approaching from a distance. The sound came from the west, and, as it approached, increased, though still dull and heavy, and was accompanied with a perceptible motion of the walls of the building, a shaking of the windows, and a distinct vibratory motion was given to the chandeliers suspended in the office. The general impression seemed to be that the shock lasted about a minute, though there is nothing reliable in that particular. Probably the time was less than that, as there seems to have been no repetition of the shock. At my house, which is a mile and a half distant, the observations made by my family were very similar to those made at the clerk's office. There was a distinct rattling of the windows.

Some in the village described the noise as like that made by the echo following the discharge of a cannon. It is generally understood that it occurred a little after three o'clock in the afternoon. I can find no one, however, who was particular to note the time."

"At Pendleton, about five miles south-west of this, the observations made were the same as at this place, in regard to the shock and its accompaniments. My informant says it occurred there precisely at three o'clock."

"It may not be amiss to state that we had another shock here in the early part of January, 1858, about ten or eleven, P. M. It was slighter than that in October, and was accompanied by the same dull, heavy, rumbling sound, and a perceptible shaking of windows, doors, &c. I was partly asleep at the time, but thoroughly aroused by the same."

Professor Dewey of Rochester University, writes: "I have no information to give. The earthquake was distinct at Lockport about half past three o'clock, P. M. It was noticed at Medina I heard, and at Batavia, as well as at Brockport. It was said to have been noticed here, but I could not find any clear evidence of the fact."

The principal of the Brockport Collegiate Institute, writes: "The shock of an earthquake was very sensibly felt here on the 23d of October last by many persons."

Rev. Dr. Samuel H. Cox, Chancellor of Ingham University, at Leroy, N. Y., writes: "In relation to your inquiry touching the earthquake, said to have occurred in all this region, on Friday, October 23, 1857, I can only say, not from my own consciousness, or memory, but from the concurrent testimony of many others, that a sensible terra motus was realized here and in many other places, on that afternoon.

"Several young ladies of the University, some then at houses in the vicinity, declare their sensations at the time, all taking notice of it. One of them says, 'I was at the piano playing alone. The shock made the windows rattle, and was repeated after a few moments.'

"A very intelligent lady in my immediate neighborhood told me distinctly as follows: 'I well recollect it, I was sewing quietly in my chamber, when the shaking of the house alarmed me; the windows rattled; the reality of it was certain. I descended to the kitchen and asked my cook if she knew what was the matter; she replied, Yes, ma'am, it thundered.'

"An educated and excellent friend of mine at Avon, near us, about fourteen or fifteen miles east by south of my residence, says: 'Yes, I felt it—I was reclining on my bed at the time. The whole house shook so alarmingly that I arose, went down and compared notes with others. We all felt it and recognized it as a veritable earthquake.'"

Rev. E. S. Wright of Fredonia, Chautauque county, New York, forty miles south-west of Buffalo, writes: "The earthquake to which you allude, I should think, was felt in this village, about half past three o'clock on the 23d of October last; at least somewhere between fifteen and thirty minutes past three, P. M. At the time I was conversing with a deacon of my church, a short distance from the village, who was husking corn in his barn. The sound was a rumbling noise, approaching slowly and loudly like the heavy wheels of a loaded wagon, in the road. The noise continued about the length of time in which a wagon would be passing the barn pretty rapidly with a load. Mrs. Wright says she was aroused by a sudden noise as if my book-case had been struck by a person falling against it in a chair. Soon the house seemed to rise and sink a little, and all was over. The earthquake was felt similarly throughout the place, I believe."

Rev. Charles F. Mussey of Westfield, Chautauque county, New York, fifty-seven miles W. S. W. of Buffalo, informs me that the shock was felt in that village.

C. D. Sacket, Esq., editor of the Jamestown Journal, Jamestown, Chautauque county, New York, south-west of Westfield, writes: "The shock of an earthquake was distinctly felt at this place and at Warren, Pa. (twenty miles south), at three to four o'clock, P. M. The character of the phenomenon was first a shock and then a vibration of the ground. Stoves and windows shook. My wife, on a visit to Westfield, New York, heard a sound, as if the cars were approaching the house, though the road was half a mile off. It was noticed by our citizens generally. Dr. Parker, of the water-cure here, was reclining on a sofa, reading, when he felt a jar and heard a sound, as if some one had jumped from quite a height to the floor in the hall. The effect did not seem to continue more than a moment or two."

Rev. C. L. Hequembourg of Warren, Pa., a town situated on the Alleghany river, about 120 miles N. N. E. of Pittsburg, writes: "Several persons felt the shock here, and it was also felt at Sugar Grove (fifteen miles north-west of Warren). It was felt so slightly here that very few facts could be ascertained in regard to its direction, &c. My impression is that the movement was from south-east to north-west, perhaps the reverse. The time, if I recollect right, was a little earlier in the afternoon than you mention. The shock appears to be of a local character, confined to this region only. It is worthy of remark that the shock was preceded by a series of rains, such as had not been known before in this section of country for many years. I should think the movement was horizontal only, but as to whether it radiated, and where the centre was, I can give you no information. Perhaps it was altogether linear. . . . . . When I felt the shock to which you refer, I was sitting in my study writing; I thought some one was moving violently upon the lower floor; but listening, found it was not the case. In a few seconds I felt another shock, and if I remember right, there may have been a third. I felt satisfied on the second or third movement that it was an earthquake. In connection with one of the shocks I heard a noise as well as felt a movement or shaking of the house."

At Hinsdale, Cattaraugus county, New York, which is not many miles east of Jamestown, there was no earthquake felt on the 23d of October, as I learn from a correspondent — an important fact to be borne in mind by the reader.

Rev. William S. Plumer, D.D., of Alleghany, Pa., opposite Pittsburg, writes: "I have no recollection of feeling any earthquake here during the month of October last; nor do I remember to have heard any one speak of it."

Prof. Smith of Jefferson College, Canonsburg, Pa., twenty miles from Pittsburg, writes: "I can only say that no shock of an earthquake was felt in this region at the time alluded to in your letter. Nor was there any thing indicating that there was such a thing in this region. I am in the habit of recording all the atmospheric and other phenomena that fall under my observation, and I would not have failed to notice and record the time and circumstances of an earthquake, if it had been perceptible here. I have inquired of several persons, both in the town and country, but was unable to procure any information on this subject."

A correspondent from Erie, Pa., ninety miles south-west of Buffalo, writes: "It was felt in various parts of the city, both north and south of us — houses were shaken, and the sound was like a heavy wagon passing over frozen ground."

Wm. L. Perkins, Esq., of Painesville, Ohio, on the railroad from Erie to Cleveland, writes: "I have made diligent inquiry, and referred also to the files of the two village newspapers here, but have no recollection, and find no mention of an earthquake here, on the 23d of October last, or about that time . . . . . We have, within about a year past, experienced two, and it seems to me, three earthquake shocks here. The first, and by far the most energetic, was on the last day of February, 1857, I think in the evening. The last was on the 16th of April, 1858, about 6 o'clock, A. M."

F. J. Prentiss, Esq., of Cleveland, Ohio, writes: "I have made repeated and thorough inquiries with reference to the earthquake of which you speak, and am satisfied that it was not known or felt in this part of the State. Within a few weeks past, two shocks were felt in this city, at Painesville and Ashtabula, but I can find no one who knows any thing about an earthquake in October last."

Hon. David Campbell of Sandusky, Ohio, writes: "I have made inquiries in relation to the earthquake, but have not heard that it was observed anywhere west of Westfield, New York."

From the facts submitted the following inferences may be drawn: —

1. That the maximum intensity of this earthquake was felt in Buffalo and its vicinity.

2. That its undulations were propagated in a linear direction, passing through Port Hope, Lockport, Buffalo, Jamestown, and Warren, which lie on an arc of a great circle, varying but little from a north and south line.

3. That its intensity diminished on either side of this line, nearly subsiding at Hamilton on the west, and entirely so at Rochester and Hinsdale on the east.

4. That the limits of this line were Port Hope, C. W., which is situated on Lake Ontario, and some point south of Warren, Pa.

IV.—CHEMISTRY.

1. On the Causes of the variable Illuminating Power of Coal-Gas. By Professor William E. A. Aikin, of Baltimore, Md.

In common with a large number of our citizens, my attention was directed some short time since, to a somewhat sudden, inexplicable, and enormous increase in the amount of our quarterly bills for gas consumed; an increase equal at times to an advance of a hundred per cent. over the corresponding quarter of the preceding year. As it would have been absurd to suppose a simultaneous derangement of all the meters over an extensive district, it was obvious that the difficulty could not lie in any error in the registry of the gas, but in its illuminating power, necessarily requiring the consumption of a greater bulk of gas to produce a given quantity of light.

It has long been known that the quality of the gas produced from the fat coals is very materially influenced by the circumstances of the decomposition. In the elaborate experiments made some years ago on a most extended scale by Hedley, the British engineer, as detailed in his report to a committee of the House of Commons, we find this subject most satisfactorily discussed. Below a cherry-red heat the products obtained by heating coal in close vessels contains hardly any illuminating material. At that temperature it is furnished most freely, but after having been formed is liable to decomposition, involving a loss of carbon, by contact with any highly heated surface in passing through the apparatus. Such decarbonization increasing with the degree of heat, with the extension of the red-hot surface, and with the time of contact. Again the duration of the heat is most important, the best gas coming over during the first hour, the quality rapidly deteriorating, until at the expiration of four hours the product is worth very little to the consumer, and after five hours may be considered as worthless. But the bulk of such worthless gas that can still be obtained by pushing the process to completion is very considerable, equal sometimes to 2-5 of all that passes over.

How far any neglect in the observance of the precautions required to produce a proper illuminating gas may explain the result, the public have no means of knowing. All that we know is, that the manufacturers furnish an article which they say is the right article and prepared in the right way, and possessing an illuminating power varying from fourteen to seventeen candles. That is, their engineer reports, that on trial with a photometer, at stated times, the gas burning from a jet, consuming five cubic feet per hour, gives an amount of light equal in the average to that of fifteen patent candles six to the pound; the patent candle being ostensibly a mixture of spermaceti and wax. Assuming as true all that is claimed by the manufacturers, it can still be shown that the gas, even if properly made and correctly tested, may be, and is furnished to the consumer in a condition of greatly diminished illuminating power, compelling the consumption of a greater bulk to obtain the required light, and consequently swelling the record of the meter and the sum total of the quarterly bills. In my trials to determine the specific gravity of our gas by weighing a globe previously exhausted and then filled with it, I obtained a result ranging from .570, to .580, somewhat below that given as characterizing good gas. But in reality I attach very little importance to this result, since the mere specific gravity of such a complex mixture as coal-gas, can hardly be relied upon to determine its commercial value.

Although good gas certainly has a higher specific gravity than poor, yet the difference could not be taken to represent the true difference in value, since the principal components of the mixture, hydrogen, light carburetted hydrogen, carbonic oxide, olefiant gas, and other still heavier hydrocarbons, having specific gravities widely different, might vary somewhat in their relative proportions sufficient to affect the illuminating power, without at the same time and to the same extent affecting the specific gravity. The action of chlorine in removing the olefiant gas and other more dense hydrocarbons, the principal light-giving materials of the coal-gas, showed a percentage of these substances never exceeding ten per cent. But not having time at the moment to guard against all sources of error in the process, I laid it aside. My attention was principally directed to the simple inquiry, to what extent will the illuminating power of the gas be impaired by keeping it in contact with water for noted periods. That it does deteriorate when thus kept, or when kept in contact with oil or even in close vessels, has been long known.

Dr. Ure tells us that gas from oil when first made and with a specific gravity of 1.054, will give the light of one candle when burned from jets consuming two hundred cubic inches per hour. But keep the gas three weeks, and then to get the same light from the same burner you must supply six hundred cubic inches per hour. He adds that with coal-gas the deterioration appears to be more rapid. For if such gas when first made will give the light of one candle by the consumption of four hundred cubic inches per hour, when kept four days will require the consumption of four hundred and sixty cubic inches per hour to give thé same light. My first attempt to obtain some definite results began on the evening of the 8th ultimo, when I filled a large receiver from the street main and placed it on the shelf of the pneumatic trough; the next evening I filled a second one and put it alongside of the first; the following evening I filled a third receiver; and still the following evening, the 11th inst., I filled a fourth receiver. On the evening of the 12th I was thus provided with four jars of gas, one of which had been standing twenty-four hours, or one day, over the pneumatic trough, — this I will call No. 1; another, No. 2, had been standing two days, No. 3 had been standing three days, and No. 4 had been four days in contact with the water. The diminution in volume by such exposure was indicated by a receiver graduated to cubic inches, into which I introduced one hundred and thirty cubic inches of gas on the evening of the 8th; on the evening of the 12th this had lost ten and a half cubic inches, indicating a loss of about eight per cent. of the original bulk.

The effect produced on the illuminating power of the gas by the loss of volume became at once apparent as I proceeded to contrast the value of the flames furnished by the contents of the several receivers, 1, 2, 3, and 4. I used for this purpose the ordinary photometer arrangement, taking the relative intensity of the shadows produced, as a measure of the relative intensity of the light. The candle employed for the comparison was the patent candle already referred to, and the burner was the kind known as fish-tail burner, which had been previously gauged, and known to consume a trifle more than five cubic feet per hour with the average maximum pressure of the gas works. I need hardly add that the burner was the same in all the trials, and occupied exactly the same position. The burner and the screen on which the shadows fell were not moved at all during the experiments. The only adjustment wanted was to bring the candle nearer to or further from the screen, and by beginning with the most luminous gas the adjustment became simply a gradual withdrawal of the candle.

The capped receiver from which the gas was passed, floated freely in a large glass jar, supported in an erect position by the perpendicular sides of the jar, its own weight, with all attachments, making a difference of level between the water around it and that within equal to three and a half inches, a little exceeding the ordinary evening pressure in the gas pipes. This difference of level, and consequently the pressure on the escaping gas, was kept uniform by the spontaneous sinking of the receiver as the gas was consumed, a flexible tube communicating between the stop of the receiver and the gas burner. This arrangement gave me a steady, equable flame, which continued perfectly uniform long enough to enable me, after a few trials, to note, very exactly, its true value. The results as first obtained were too startling to be at once believed, but subsequent repeated trials satisfied me that they were very close approximations to the truth. The first trial was with the gas from the street main, which I found equal to 10.71 candles. The same gas, transferred from the pipe to the capped receiver, and burned immediately, gave exactly the same power, 10.71 candles. Gas No. 1 was next used, and found equal to only 3.50 candles; gas No. 2, after standing two days, gave the light of 3.20 candles; gas No. 3, three days old, was equal to 1.90 candles; and gas No. 4, four days old, gave the light of 1.75 candles, — these quantities representing the average of repeated trials.

It thus appears that the illuminating material of our coal-gas is so rapidly abstracted by suffering it to remain in contact with water, that the same volume of gas which to-day will give me the light of nearly eleven candles, by standing until to-morrow, will give the light of only three and a half candles, and if left standing four days will give the light of only one and three fourth candles, while the only means left to the consumer to get the light he requires from this deteriorated gas is to burn more of it, as we have all been doing through the past winter. If we now take into account the well-known fact that gas of less illuminating power has less density, and that gas of less density passes more rapidly through a given aperture than gas of greater density, we have another cause operating to increase the consumption. In Hedley's experiments the Argand burner which gave the light of twenty-five candles when supplied with three cubic feet per hour of gas from Welsh canal-coal, with a specific gravity of .737, required no less than seven and a half cubic feet per hour to give the same light from the same burner, when the gas was made from the Newcastle coal and had a specific gravity of only .475.

Again, as we diminish the illuminating power of the gas we increase its heating power, and this necessarily brings with it a higher temperature given to the burners, a higher temperature given to the gas passing through them, and again an increased rapidity in the flow. It is thus manifest that the public are placed in a peculiarly unfortunate position, since all the mistakes that are likely to occur in the process of manufacture, are mistakes that must inevitably increase the bills of the consumer and the profits of the manufacturer. If the workman fails to raise the heat with proper rapidity, if he overlooks a retort and allows the heat to continue a little too long, if towards the close he allows the heat to rise a little too high, the result is inevitable, the product is deficient in illuminating power. Or if on any one day a little more gas is produced than is legitimately required, the surplus remains in the gasometer to vitiate the supply of to-morrow. To what extent this vitiating action operates may be inferred from the fact that I have never been able to obtain from the gas of our pipes an illuminating power equal to the minimum of that reported by the engineer of the gas company. In my trials the power has varied from that of thirteen candles down as low as that of nine candles, instead of ranging from fourteen to seventeen candles.

This difference is perfectly intelligible if we assume the last quantities to represent the value of the gas when first made, and my results to represent its value as delivered to the consumer. In conclusion I would merely add that the difficulty suggests its own remedy. And that would be to have a standard of quality established by the proper authorities, taking the illuminating power as the basis of the calculation, and then to have the requirements of such standard insured by a nightly examination, if necessary on the part of some one entirely disconnected with the manufacture. In other words, the photometer can be made as available and as valuable to the consumer of gas as the hydrometer is to the spirit merchant. As he distinguishes with his instrument in any mixture, between the spirit he wishes to buy and the water he is unwilling to pay for; so the consumer of gas can distinguish with the photometer between the true illuminating material and the worthless, heat-producing gases, hydrogen and light carburetted hydrogen, that make up the bulk of the ordinary coal-gas.

2. INVESTIGATION OF THE ACTION OF NITRIC ACID UPON THE METALLIC CHLORIDS. By Prof. HENRY WURTZ, of Washington, D. C.

DURING the spring of 1850, while attempting to devise a method for the easy separation of magnesia from the alkalies, in the laboratory of Dr. Wolcott Gibbs, in New York City, I found that the chlorids of magnesium and the alkali-metals were readily and perfectly decomposed by mixture with an excess of nitric acid and subsequent evaporation. Intending to investigate the subject thoroughly, and to make it the basis of a new method of separation, I did not publish the observation at the time, and it is but recently that I have had an opportunity to pursue the path of investigation thus indicated.

Subsequently, in the American Journal of Science, for January, 1853, Professor J. Lawrence Smith, of Louisville, announced his discovery of the complete decomposition of chlorid of ammonium by evaporation of its solution with excess of nitric acid, and followed shortly with another paper,* in which he showed that the chlorids of potassium and sodium are also decomposed by the same treatment and converted into nitrates. My friend Dr. Gibbs, remembering, although not perfectly it seems, my own previous experiments in his laboratory, was kind enough to append, without my knowledge, a note to Dr. Smith's paper, which reads as follows.

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* Am. Jour. Science for Nov. 1853, Vol. XVI. p. 373.

"*Note to Dr. Smith's paper on the decomposition of the chlorids by nitric acid.* — Dr. Smith's observation that the alkaline chlorids are decomposed by heating and evaporating with nitric acid is not new, although I am not aware that the fact has ever been published. Mr. H. Wurtz made experiments on the subject in my laboratory *two years since,* and obtained the same results as Dr. Smith. The complete decomposition of chlorid of magnesium by evaporation with nitric acid, was also observed in the laboratory of the late Prof. Norton, *a year or two, I believe, before* Mr. Wurtz studied the subject. The alkaline nitrates are easily converted into chlorids, by boiling them with an excess of chlorhydric acid, in presence of any metallic oxyd having a strong affinity for oxygen. I employ the protochlorid of tin for this purpose, and when the reaction is over, and gas (nitrous oxyd) is no longer evolved, a current of sulphydric acid gas removes the tin, and the filtrate contains only the alkaline chlorid and free chlorhydric acid. — W. G." *

I have introduced the italics for the purpose of directing attention to what is apparently a misremembrance of date on the part of Dr. Gibbs; my experiments in his laboratory having been made, as above stated, about *three years and a half* previously, and so far as I can ascertain, no experiments having been made in Professor Norton's laboratory. Although the matter is of no great importance, it still seems right that the discovery of the complete decomposition of chlorid of magnesium by nitric acid, which was the starting-point of the following investigation, and which appears to have been first made by me, should not go without a claimant.

With this brief historical introduction, I will proceed to present the results of my experiments, the object of which has been to ascertain the character of the action of nitric acid upon *all* the metallic chlorids, in order not only to complete our knowledge of this class of reactions, and enable us to generalize them, but also to furnish facts which may reasonably be hoped to have application in analysis; of which the separation of magnesia from the alkalies already promises to be one instance.*

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* Am. Jour. Science [2], XVI. 416.

My experiments were made generally upon each chlorid both in the crystallized or dry state and in the state of solution, a mode of proceeding equivalent in many cases only to the determination of the action of the nitric acid in the concentrated and dilute conditions. The acid chiefly operated with was chemically pure and of specific gravity = 1.29, but in some cases a fuming acid which had been rectified and of spec. grav. = 1.43 was made use of.

1. *The Alkali-metals, Potassium, Sodium, and Lithium.* — Dr. J. Lawrence Smith, in the memoir of November, 1853, before alluded to, mentions his having obtained the complete decomposition of the chlorids of sodium and potassium by nitric acid. It is by no means necessary, however, as stated there by Dr. Smith, that the nitric acid should be "boiled gently" with the chlorids; for a mere evaporation to dryness without ebullition effects a complete reaction, and if the nitric acid be in sufficient excess, and the chlorids are in solution, or *become dissolved* during the process, one evaporation is enough. These facts are of evident importance in any proposed analytical applications.

Pure chlorid of lithium† behaves with nitric acid precisely like the chlorids of potassium and sodium.

Pure crystals of chlorid of sodium being heated gradually with an excess of nitric acid of spec. grav. = 1.29 (by immersing the beaker in water and applying heat to the latter), the following appearances were observed.

At about 75° C., small bubbles of gas were evolved, the evolution becoming very copious at 100° C., and the salt rapidly dissolving to a clear yellow liquid. On cooling, the evolution ceased again at 75° C., and on further cooling small granular crystals separated plentifully, which were washed with strong alcohol, and found to contain but a trace of chlorine, and to deflagrate with charcoal, being in fact *nitrate of soda.* A further quantity of nitrate of soda was obtained by evaporating and adding alcohol to the solution. I may remark here that Gmelin states with regard to *chlorid of potassium,*[*] that "aqueous (wasserhaltige) nitric acid forms with it a potash salt with separation of chlorohydric acid." Neither the authority for the statement, nor the *temperature* at which the result was obtained, are given.

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* It would be wrong for me not to acknowledge here my indebtedness to some of my friends, especially to Messrs. T. Sterry Hunt, George J. Brush, and James R. Brant, for the very generous manner in which they have supplied me with preparations of some of the *rare metals*, needed to render my investigation complete. — H. W.

† Two preparations were operated upon, made from specimens of the carbonate presented to me by Messrs. Hunt and Brush, one of which was found to be pure, and the other contained only a little carbonate of ammonia.

The gas evolved during the reaction is *brown-yellow,* having none of the reddish tinge of nitrous acid, nor perceptibly of the feeble greenish tinge of chlorine. Attempts made to isolate it by collection over hot water failed, owing to its very rapid and almost total absorption by the latter. No reasonable doubt can exist, however, that this brown-yellow gas is the same as that evolved by heat from *aqua regia,* in which Gay-Lussac found, together with chlorine, his chloronitrous and hypochloronitric acids, $NO^2 Cl$ and $NO^2 Cl^2$, especially when we take into consideration that, according to Gay-Lussac, the products in the earlier stages of the reaction are chiefly chlorine and $NO^2 Cl^2$, the latter consisting of equal volumes of nitric oxyd and chlorine, while E. Davy,[†] in an earlier examination of this very case, namely the action of nitric acid upon chlorid of sodium, actually found a gaseous product composed of nitric oxyd and chlorine in equal volumes. The *color* of the evolved gas moreover corresponds to that attributed by Gay-Lussac to the gas evolved by *aqua regia,* namely brownish-yellow, and I find, upon comparison of the two gaseous mixtures, that the only difference distinguishable by the eye is a greater depth of color on the part of that evolved by the chlorid of sodium. In order to ascertain whether the gas contained either of the lower oxyds of nitrogen, a large quantity of it was passed into a receiver over hot water, and by this means was finally obtained a small portion which was not absorbed by the water nor by potash. Transferred to a tube this was found to support combustion, and even to produce an increased glow upon a glowing splinter of charcoal introduced into it, whilst no red color was produced by admixture with air; from which is of course to be inferred the presence of nitrous and the absence of nitric oxyd.

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[*] Handbuch der Chemie, II. 56.

[†] Liebig und Kopp's Jahresb. für 1847–48, p. 387.

2. *Ammonium.*—In Dr. Smith's investigation of the action of nitric acid upon sal-ammoniac, he has shown that the chief product of this reaction is nitrous oxyd.* On his results he has founded his highly important method of removing sal-ammoniac in the process of analysis, a method which I have frequently used and consider to be of high value. In fact if pure sal-ammoniac and pure nitric acid be mixed together and evaporated on the sand-bath (much below ebullition), in most cases no residue whatever is left. Cases however have occurred to me in which a quantity of *nitrate of ammonia* was formed, that proved more troublesome to get rid of than the original sal-ammoniac. This may have depended on the relative proportions of acid and chlorid in the mixture, but of this point I have made no examination.†

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* Am. Jour. Science [2], XV. 240, note.

† With regard to the character of the reaction in this decomposition, Dr. Smith, after recounting the results of some partial experiments to determine the nature of the products, remarks as follows: "The character of the decomposition which takes place, is somewhat curious and unexpected. At first I supposed that the decomposition resulted in the formation of equal volumes of NO, Cl, and N, but it appears that such is not the case, and that all but a very small portion of the ammonia with its equivalent of nitric acid is converted into NO; the liberated hydrochloric acid mixing with the excess of nitric acid; a little of the sal-ammoniac and nitric acid does undergo the decomposition first supposed, and in this way only can the small amounts of chlorine and nitrogen be accounted for."

Upon considering this subject from every point of view at present available, I am led to ask the question whether *all* the reactions of this class, in which nitric acid comes into contact with another substance containing hydrogen or an electro-positive metal under the influence of heat, are not occasioned simply and solely by the combination of such hydrogen or metal with the oxygen of the nitric acid to form water or metallic oxyd. Thus in the formation of NO^2Cl^2 from *aqua regia*, according to Gay-Lussac the reaction is

$$3\,HCl + NO^5 = 3\,HO + Cl + NO^2Cl^2;$$

and in the formation of NO^2Cl,

$$3\,HCl + NO^5 = 3\,HO + Cl^2 + NO^2Cl.$$

Look also at Mauméné's reaction between sal-ammoniac and nitrate of ammonia (Liebig and Kopp's Jahresbericht for 1851, p. 321; Am. Jour. Science [2], XIII. 412), in which are obtained five volumes of nitrogen and one of chlorine,

$$NH^4Cl + 2\,(NH^4O, NO^5) = 12\,HO + Cl + 5\,N;$$

3. *Hydrogen.* — Reasoning from the analogy of hydrogen to the alkali-metals, chlorohydric acid ought to comport itself with nitric acid like chlorid of sodium. In fact the method so long in use of freeing nitric acid from chlorohydric, by distillation and rejection of the first portion, which contains all the chlorine, plainly depends upon this relation; although, of course, the true theory of the method could not be understood until Gay-Lussac had published his elegant discoveries regarding *aqua regia.*

Some *aqua regia,* containing excess of nitric acid, was evaporated on the sand-bath until the evolution of chlorine and Gay-Lussac's acids had ceased, and the liquid, at first highly colored, had become colorless. No trace of chlorine could any longer be found in it.

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and at the familiar equation representing the formation of laughing gas from nitrate of ammonia,

$$3\,(NH^4\,O, NO^5) = 12\,HO + 6\,NO.$$

I might probably multiply examples, but these will serve to illustrate my meaning. It is obvious at a glance that in all the above cases all the hydrogen present becomes water at the expense of the oxygen of the nitric acid, and the other elements arrange themselves according to their relations at the temperature. Now on this hypothesis the action of nitric acid upon the chlorids of the alkali-metals, for instance that upon chlorid of sodium, the gaseous products of which, according to E. Davy are chlorine and $NO^2\,Cl^2$ (or something having the same equivalent constitution), would be

$$3\,Na\,Cl + 4\,NO^5 = 3\,(Na\,O, NO^5) + Cl + NO^2\,Cl^2;$$

and that of nitric acid upon sal-ammoniac would be

$$3\,NH^4\,Cl + 4\,NO^5 = 12\,HO + Cl + NO^2\,Cl^2 + 6\,NO;$$

two equations which present striking analogies. Nor is it difficult to reconcile the results of Dr. Smith with this last equation. The gaseous mixture which he examined was collected over hot water, which would absorb the $NO^2\,Cl^2$, together with a portion of the chlorine, and the remaining gas would evidently be nearly all NO.

The verification of this hypothesis seems to me a subject worthy of attention, and will be kept in view throughout the present investigation, as this may develop facts throwing light upon these reactions.

I may add here that on careful comparison of the color of the gas evolved by nitric acid in its action upon sal-ammoniac with that of the gas evolved by *aqua regia,* the two appeared, not only to my own eyes but to those of other persons, to be the same, namely a mixture of yellow and brown, differing merely in *shade,* the tint in the case of sal-ammoniac being much lighter.

4. *Magnesium.* — A strong solution of pure chlorid of magnesium mixed with pure nitric acid of sp. gr. 1.29 and evaporated upon the sand-bath, left a residue in which chlorine was still easily detected, but on moistening the residue with the same nitric acid and reëvaporating, no trace of chlorine was found in the second residue. With crystallized chlorid of magnesium the same results were obtained. Two evaporations with excess of acid seem to be sufficient under ordinary circumstances to complete the decomposition, and in favorable conditions it may be effected by one. Thus pure crystals of the chlorid $MgCl + 6HO$, dried by pressure between folds of paper, were heated with nitric acid of sp. gr. 1.43. They quickly dissolved to a clear solution, which when evaporated to dryness, left a residue containing no trace of chlorine.

Pure crystals of chlorid of magnesium, heated with excess of nitric acid of sp. gr. 1.29, began to give off gas in minute bubbles at 60° C., quickly dissolving to a clear yellow solution. The evolution of gas however was not copious even at 100° C.; but on heating a little above this temperature, it became rapid and violent, the color of the gas given off being a brownish-yellow. On gradual cooling, the gas ceased to be formed at about 75° C. As in the case of chlorid of sodium, an attempt was made to collect the gas over hot water, which failed for the same reason as in that case. On cooling to the temperature of the air, no change took place in the liquid, and none on the addition of alcohol in large quantity.

5. *Calcium.* — Experiments were made with a concentrated solution of the pure chlorid, with the crystals, and with the anhydrous salt. In each case, one evaporation expelled every trace of chlorine.

6. *Strontium.* — Parallel experiments, as in the case of calcium, were made with this chlorid in solution, crystallized and dry, and with similar results.

7. *Barium.* — With the chlorid in solution, crystallized, and dry, results similar to those under (5) and (6) were obtained. Chlorid of barium in the *solid* form appeared however to be much more difficult of decomposition than the other salts of this group, requiring generally one or more repetitions of the evaporation with nitric acid to convert it entirely into nitrate, although a dilute solution yielded with the greatest readiness. This fact was at first explained upon the belief commonly entertained that chlorid of barium is very difficult of solution in nitric

acid, a belief founded upon the fact that when a strong solution of this chlorid is added to nitric acid, unless the latter be very dilute, a precipitate appears, which has been supposed to consist of chlorid of barium. Thus H. Rose says:* "When to a solution of a salt which is to be examined (for sulphuric acid) chlorohydric or nitric acid has been added, it must still be remarked that upon then adding a concentrated solution of chlorid of barium or nitrate of baryta, a white precipitate of chlorid of barium or nitrate of baryta may appear, because these salts are far less soluble in free acids than in water."

As it seemed to me possible, however, that the precipitate produced by chlorid of barium solution in nitric acid might contain some *nitrate*, I determined to examine it. A quantity of this coarse crystalline precipitate was therefore drained, dried by pressure between folds of paper, and finally heated on the sand-bath until it no longer reddened litmus paper. It then presented the appearance of a fine white powder, which when heated in a glass tube, fused easily to a clear colorless liquid without giving off water. At a higher heat the liquid gave off bubbles of red gas. Its solution in water gave a very strong reaction for nitric acid, and one much less marked for chlorine. 1.0375 gram gave 0.9233 of BaO, SO^3, which is equivalent to 1.0347 of pure BaO, NO^5. In an attempt at a determination of the chlorine, 0.297 gram of the substance gave one milligram and a half of $Ag\,Cl$. We thus come to the rather unexpected result that this precipitate, instead of being chlorid of barium as heretofore supposed, is pure *nitrate of baryta*. In a subsequent experiment, the precipitate formed by an excess of nitric acid in a strong solution of chlorid of barium was washed twice by decantation with pure nitric acid, then dried, first upon a slab of porous earthenware biscuit, and finally upon the sand-bath. It was then chemically pure nitrate of baryta and contained no trace of chlorine. The first liquid poured off from it gave no reaction with dilute sulphuric acid in the cold, but a cloudiness appeared on warming it. It gave, however, with nitrate of silver a very strong curdy precipitate. It is therefore evident that when strong nitric acid is added to a solution of chlorid of barium in excess in the cold, very nearly the whole of the barium is immediately converted into insoluble nitrate, and chlorohydric acid is set free in the liquid. It is plain, also, that the reason why solid chlorid of barium resists decomposition by nitric acid is, that it becomes immediately covered with a coating of nitrate of baryta, soluble with difficulty in the acid. In order to ascertain whether the presence of an excess of chlorid of barium modifies the result, a strong solution of the chlorid was added in large excess to nitric acid. The precipitate, collected on a filter, pressed strongly between folds of paper and dried on the sand-bath, contained but a trace of chlorine. The filtrate contained but little nitric acid.

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* Ausführliches Handbuch der analytischen Chemie, i. 485.

On experimenting with chlorid of strontium, similar results were obtained. A stronger solution, however, was required, and the precipitate did not appear so soon and was more coarsely crystalline. It contained, after being rinsed with nitric acid, dried on porous earthenware and then on the sand-bath, but a faint trace of chlorine. The liquid poured off contained but little strontia, giving a precipitate with sulphuric acid only after standing for some time.*

8. *Aluminum.* — Pure chlorid of aluminum left, after one evaporation with acid of 1.29 sp. gr., a nitrate beautifully crystallized in thin plates, which contained no chlorine and deliquesced only in very damp air. These crystals were not further examined.

9. *Glucinum.* — The pure chlorid † was entirely converted into nitrate by one evaporation with acid of 1.29 sp. gr.

10. *Iron.* — Experiments were made upon the green crystals of the anhydrous sublimed sesquichlorid, and upon a solution of the same. Any experiment with the protochlorid is obviously useless. In all cases it was found almost, if not quite, impossible to expel all the chlorine. After frequently repeated evaporations with acid of 1.29 sp. gr., traces of chlorine remained. When the evaporation was conducted on the water-bath a large quantity of a reddish-brown substance, insoluble in water was formed, and water poured on the residue was exceedingly difficult to filter clear. After filtration it had a brownish-red

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* The action of nitric acid *in the cold* upon the chlorids will be examined, and the results presented in a subsequent part of this memoir. It opens a new field, probably more or less fertile in *practical* applications, which was entirely unforeseen at the commencement of this investigation.

† Made from pure carbonate obtained from Mr. James R. Brant of New York city.

or exactly port-wine color, and gave marked indications of the presence of chlorid, but contained also a large proportion of nitric acid in combination, as was proved by the *indigo test*, after precipitating the oxyd of iron with ammonia. The indigo test cannot be applied in iron solutions without this previous precipitation, both on account of their color, and because of the unexpected discovery, which I shall more fully set forth in another paper, that solutions of the sesqui-salts of iron themselves bleach solution of indigo powerfully when no nitric acid is present.

The port-wine-colored liquid, I may remark, contained undoubtedly one of Ordway's polybasic sesquinitrates of iron.* On adding to it a little chlorohydric acid *a precipitate was at first formed*, but on standing for a few minutes the liquid cleared up again, at the same time changing its color to the clear pure brown of sesquichlorid of iron. The portion of the residue insoluble in water having been washed (in which operation it nearly all ran through the filter), until the washings no longer gave a chlorine reaction, was examined and found to contain both chlorine and nitric acid.

Another experiment was made with the fuming acid of spec. grav. 1.43, upon the sublimed sesquichlorid. It quickly dissolved in the acid to a brown-yellow solution, which was evaporated, at first rapidly, but afterwards very carefully, at a heat much below 100° C., in order to avoid decomposition of the nitrate formed. The brown gummy translucent residue, having been exposed for some hours to a very gentle heat to drive off all traces of free acid, was deliquescent in moist air, and gave with water a clear dark brown solution, leaving little or no insoluble residue. It still contained a trace of chlorine.

The influence of the coöperation with the nitric acid of some other oxydizing agents was tried, but so far without success. Those experimented with were the deutoxyds of lead and manganese and arsenic acid. Chlorine was still present after four evaporations with the acid in conjunction with the deutoxyds.

11. *Manganese.* — Both the dry chlorid of manganese and its solution lost all chlorine by one evaporation with excess of nitric acid.

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* Am. Jour. Science [2], ix, 32; Liebig and Kopp's Jahresbericht for 1850, p. 328.

12. *Cobalt.*—Chlorid of cobalt was also converted entirely into nitrate by one evaporation. In applying the indigo test for nitric acid in cobalt solutions, the colors of which interfere with it, I resorted to the ingenious expedient for the application of Crum's manganese test to these solutions given by Dr. Gibbs, in his "Contributions to Analytical Chemistry,"[*] which consists in previously neutralizing the color of the cobalt solution by adding a solution of *chlorid of nickel.*

13. *Nickel.*—Chlorid of nickel behaved with nitric acid precisely like chlorid of cobalt. The green color of nickel solutions interferes still more with the indigo test than the cobalt colors, but the difficulty is readily and effectually overcome by Dr. Gibbs's method, that is, by previously destroying the green color by addition of chlorid of cobalt.

14. *Zinc.*—After *five* repetitions of the treatment with nitric acid of sp. gr. 1.29, the chlorid still retained chlorine. Pure fused chlorid of zinc was then boiled with acid of sp. gr. 1.43. It dissolved gradually, with evolution of brown gas, and gave a colorless solution, which when evaporated at a gentle heat, left a crystalline translucent residue. Every trace of free acid having been expelled, the residue was boiled with water. A white substance remained undissolved in considerable quantity. The solution contained both chlorine and nitric acid in large quantity, and the white substance having been well washed, contained but little chlorine, and was most probably one of the basic nitrates of Grouvelle and Schindler.[†]

15. *Cadmium.*—The chlorid, like that of zinc, resisted five repetitions of the action of nitric acid of sp. gr. 1.29. Pure crystallized chlorid of cadmium, dried at a gentle heat to expel its crystal-water, was boiled with acid of 1.43 sp. gr., and dissolved with difficulty, giving off brown gas. The clear, colorless solution was evaporated slowly, raising the heat towards the last until all free acid was expelled. The crystalline residue dissolved completely in water, and the solution contained both chlorine and nitric acid.[‡]

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[*] Am. Jour. Science [2], XIV. 204.

[†] Gm. Handbuch, III. 32.

[‡] The paper which covered the dish during the evaporation, although the heat had never approached the boiling point of the liquid, contained much cadmium, and was turned deep yellow by solution of HS; a curious result which requires confirmation.

16. *Copper.* — Chlorid of copper proved to be difficult of decomposition. After three evaporations with acid of 1.29 sp. gr., water poured on the residue dissolved it but partially, leaving behind a quantity of a bluish-green verditer-like substance, probably the tribasic nitrate of Proust, $3\ CuO, NO^5 + HO$.* The filtrate contained but a faint trace of chlorine, and had a dark greenish-blue color, which upon the addition of chlorohydric acid changed immediately to a widely distinct *greenish-brown* hue.† By the indigo test much nitric acid was found in it.‡

The verditer-looking substance was boiled with water, and washed until the washings no longer reacted with sulphohydric acid. Dissolved then in nitric acid the solution contained no chlorine, but on solution in chlorohydric acid much nitric acid was found.

Crystals of $Cu\ Cl + 2\ HO$ were heated cautiously until converted into the brown anhydrous chlorid, which then slowly dissolved when heated with nitric acid of sp. gr. 1.43, with evolution of brown gas, to a blue liquid. This liquid, evaporated to dryness at a very gentle heat left a blue crystalline residue, which still contained a trace of chlorine, and was altogether identical with the one above described.

17. *Chromium.* — The chlorid of this metal yielded very readily. After the first evaporation with nitric acid no chlorine could be detected.

18. *Uranium.* — The residue after the first evaporation of the terchlorid with nitric acid crystallized beautifully in lemon-yellow prisms of the well-known compound of nitric and uranic acids. They contained no trace of chlorine.

19. *Subchlorid of Mercury.* — According to A. Vogel,§ "calomel dissolves in hot nitric acid with evolution of nitric oxid as protochlorid and nitrate of the protoxid,

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* Gm. Handbuch, III. 417.

† Gladstone (Liebig and Kopp's Jahresb. 1855, p. 415,) describes the color imparted to copper solutions by excess of H Cl as *yellowish green.* To my eyes it appears as above.

‡ The application of the indigo test to Cu solutions, as also to those of the next two metals, Cr and U, requires certain precautions to be described in a special paper on the subject before alluded to, which is in course of preparation.

§ Gm. Handbuch, III. 512.

$$3\,Hg^2\,Cl + 4\,NO^5 = 3\,Hg\,Cl + 3\,(Hg\,O, NO^5) + NO^2."$$

Some pure calomel, which left no residue when heated on platinum foil, was boiled with nitric acid of sp. gr. 1.29. It slowly dissolved, with evolution of red-brown gas to a colorless solution. This when evaporated left a residue mostly soluble in water, the solution containing both chlorine and nitric acid. When acid of 1.43 sp. gr. was used, the solution took place very rapidly. The colorless solution evaporated at a very gentle heat, let fall at first acicular crystals (Hg Cl), and dried up to a crystalline residue, which when treated with hot water dissolved in great part, leaving a small quantity of yellowish residue mingled with some heavy black powder. The solution contained much nitric acid.

20. *Protochlorid of Mercury.*—Experiments were made with corrosive sublimate both in solution and in the solid form. The results with calomel had led me to expect difficulty in decomposing this chlorid, but I was not prepared to find it totally unacted upon. Nitric acid, whether concentrated or dilute, was found to be wholly inert, and after the most prolonged and repeated evaporations, the corrosive sublimate was left behind, in the form of acicular crystals, *entirely unchanged.* When the chlorid was very finely pulverized, boiled for some time, and evaporated with acid of 1.43 sp. gr., the result was the same. In no case could nitric acid be found in the residues, except as a minute trace. In this connection the extremely difficult solubility of corrosive sublimate in nitric acid is worthy of mention. According to J. Davy,[*] it "requires more than five hundred parts of hot nitric acid of sp. gr. 1.41 for solution."

21. *Lead.*—Gmelin says, while treating of the purification of nitric acid, that, "by means of oxyd of lead nitric acid cannot be freed from chlorine, because chlorid of lead is soluble in it, and in the heat decomposable by it."[†] A solution of chlorid of lead left, after one evaporation with excess of dilute nitric acid, a residue containing no trace of chlorine. A quantity of pure precipitated chlorid was then digested with acid of 1.29 sp. gr. on the sand-bath, and was observed to change rapidly during the operation into a granular crystalline powder. The

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[*] Gm. Handbuch, III. 520.

[†] Ibid. I. 804.

acid having been renewed several times until this change appeared complete, a little hot water was added, which dissolved the whole mass, and the solution contained nothing but nitrate of lead, without a trace of chlorid. The decomposition was surprisingly easy and complete, considering the insoluble character of the substance.

22. *Silver.* — The action upon chlorid of lead being so powerful, I was by no means led to anticipate the result obtained with chlorid of silver. On the contrary, I certainly expected to obtain some indications of decomposition. But on boiling for a long time freshly precipitated chlorid of silver with nitric acid of 1.29 sp. gr., renewing the acid frequently, not a trace of silver could be detected in the filtrate, and on evaporating acid of this strength repeatedly to dryness upon the same mass of the chlorid, the residue yielded to water no silver. A quantity of the chlorid (which had acquired a violet tinge by exposure to light) was then finely pulverized and boiled with acid of 1.43 sp. gr., with the addition of a little chlorohydric acid to provide against the possible presence of metallic silver or the subchlorid, for twelve hours. Under the influence of the acid the violet tinge very quickly disappeared, the mass becoming white. The mixture was then evaporated to perfect dryness upon the sand-bath, and water poured upon the residue. In the solution silver was now easily detected with chlorohydric acid, but on examination for nitric acid, *not a trace* could be found in the liquid. The silver was found to be present in the form of *sulphate*, the sulphuric acid having been derived from a minute trace existing in the nitric acid, which had distilled over with it during its rectification.* It may then be asserted that boiling nitric acid of 1.43

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* This nitric acid was prepared originally by distilling saltpetre with oil of vitriol, the first portions being rejected as containing chlorine, and was re-distilled carefully, at the lowest possible temperature, until about two thirds had gone over, under the expectation, which appears to have been fallacious, that the sulphuric acid would remain behind in the retort. No precipitate could be obtained from it directly with nitrate of baryta, and the minute contamination would have entirely escaped discovery, but for the result of the experiment on $Ag\,Cl$. On evaporating a little to dryness, however, after admixture with BaO, NO^5, and redissolving, a distinct cloudiness appeared. Gmelin, in the course of his directions for preparing strong nitric acid (Handbuch, I. 805), recommends that the acid which has been concentrated by distillation with HO, SO^3 should be freed from the latter by re-distillation, "either by itself or over saltpetre." It is evident from the above that dis-

sp. gr. is wholly without action upon chlorid of silver. We have here, then, a new and striking distinction between the two metals, lead and silver, which in many other respects approach each other in their chemical relations and reactions.

23. *Gold.* — According to Pelletier,* "nitric acid, on account of its easy volatility, has no action upon terchlorid of gold." Pelletier's *fact* I find substantially confirmed by careful experiments, but the explanations which he assigns may reasonably be held in doubt. A solution of pure terchlorid of gold was evaporated at an *extremely* gentle heat (much below 100° C.), and the residue, which was crystallized in beautiful radiating tufts, treated with nitric acid of 1.43 sp. gr., in which it dissolved to a clear yellow liquid. This was evaporated, first on the sand-bath, and when concentrated, at a temperature little above that of the air. The residue, which crystallized just as before, as a radiating fibrous mass, was deliquescent, and gave with water a clear yellow solution, leaving behind a very little metallic gold as a brown powder. This trace of metallic gold was the only evidence of decomposition that could be obtained, as the solution gave only doubtful indications of the presence of nitric acid. In fact, even if the decomposition had been considerable, no distinct evidence of nitric acid in the residue could be expected, for, according to Vauquelin and Pelletier,† solutions of the nitrate of teroxyd of gold leave on evaporation a mixture of oxyd of gold and metallic gold.

24. *Platinum.* — Bichlorid of platinum was as slightly acted upon by nitric acid as the terchlorid of gold. Upon operating precisely as with the gold compound, a residue was obtained which yielded to water unchanged bichlorid of platinum, with no distinct nitric acid. A very slight brown residue however was left undissolved by the water, which gave indications of nitric acid, and was probably a basic nitrate; for Berzelius says ‡ that the nitrate of deutoxyd of platinum on evaporation to dryness leaves a residue but partially soluble in water with separation of a basic salt.

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tillation by itself will not answer. Nitrate of baryta might be better than saltpetre, and nitrate of silver probably still better, because it would retain both the sulphuric acid and the chlorine.

* Gm. Handbuch, III. 672.

† Gm. Handbuch, III. 674.

‡ Ibid. III. 738.

25. *Tin.* — According to Gay-Lussac,* "bichlorid of tin, when heated with nitric acid, evolves chlorine and nitric oxyd, and lets fall stannic acid." I have experimented only with the protochlorid. Pure crystals of this, after evaporation repeatedly with acid of 1.29 sp. gr., appeared to be principally converted into stannic acid. Water poured on the residue after the third evaporation and filtered, contained some chlorine (as bichlorid?) and but a trace of nitric acid.

26. — *Arsenic.* — Arsenious acid was dissolved in hot chlorohydric acid, the solution mixed with excess of nitric acid of 1.29 sp. gr., and evaporated to dryness at a gentle heat. The white residue dissolved easily and completely in water, being in fact nothing but pure *arsenic acid*, and the solution contained no trace of either nitric acid or chlorine. It gave the reddish-brown precipitate with nitrate of silver which is characteristic of arsenic acid.

27. *Antimony.* — Terchlorid of antimony, according to A. Vogel,† "evolves, with hot nitric acid, chlorine gas, whilst a white powder (antimonic acid) precipitates." The residue left on boiling the terchlorid with nitric acid of 1.29 sp. gr., and evaporating, was treated with dilute sulphuric acid, which partially dissolved it, the solution containing both chlorine and nitric acid in considerable quantities. The undissolved portion, consisting undoubtedly chiefly of antimonic acid, was washed for a long time, the washings constantly affording a slight chlorine reaction, due to the presence of a difficultly soluble subchlorid or oxychlorid. On dissolving this washed substance in nitric acid, traces only of chlorine appeared in the solution; but on dissolving it in chlorohydric acid, a considerable indication of nitric acid was obtained by the indigo test. It would seem, therefore, that the above reaction of Vogel is not carried out with precision. It may be useful to call to mind here, that, according to Berzelius and H. Rose, both antimonious and antimonic acids are perfectly insoluble in nitric acid.

28. *Bismuth.* — According to Jacquelain, the oxychlorid of bismuth, $Bi\,Cl^3$, $2\,Bi\,O^3$, "dissolves in hot nitric acid, and on evaporation remains behind unchanged." Nitric acid, boiled and evaporated with terchlorid of bismuth, left a residue from which water dissolved little chlorine but abundance of nitric acid, while the insoluble portion dissolved readily in nitric acid, and contained abundance of chlorine.

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* Gm. Handbuch, III. 83.

† Ibid. II. 786.

29. *Cerium* — Chlorid of cerium,[*] which had a faint pink tinge due to contamination with chlorid of didymium, and probably contained also a little chlorid of lanthanum, but was otherwise perfectly pure, left, on evaporation with excess of nitric acid, a deliquescent nitrate which still contained chlorine, and the latter was not wholly expelled by two repetitions of the same treatment.

30. *Lanthanum.* — The chlorid used was also faintly tinted by chlorid of didymium, and probably not entirely free from chlorid of cerium. The nitrate left, upon evaporation with nitric acid, appeared to be much more deliquescent than the nitrate of cerium. Chlorine was retained as obstinately by it as by the latter, a trace remaining after three evaporations with nitric acid.

31. *Didymium.* — The chlorid of didymium, free from all impurities except probable traces of chlorids of cerium and lanthanum, gave similar results. The resulting nitrate (still retaining a trace of chlorine) had a violet color and was deliquescent.

32. *Thorium.* — Some perfectly pure chlorid of thorium [†] was decomposed with difficulty, leaving after the third evaporation with nitric acid a deliquescent nitrate, which still contained a trace of chlorine. This nitrate had a strongly astringent taste.

33. *Zirconium.* — Pure chlorid of zirconium [‡] resisted two operations, still retaining a trace of chlorine.

34. *Molybdenum.* — Some extremely beautiful crystals of pure molybdic acid,[§] dissolved in chlorohydric acid and evaporated with an

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[*] The cerium, lanthanum and didymium compounds used were prepared from extremely beautiful specimens of highly crystallized sulphates, for which I am indebted to the generosity of Mr. T. Sterry Hunt, of the Canada Geol. Survey. The sulphate of didymium, especially, was in crystals nearly a quarter of an inch long, presenting internally a rose-colored transparency and brilliant cleavage, though the faces were ill-defined and dull, apparently from efflorescence.

[†] Prepared from a specimen of *orangite* (thorite) presented to me by Mr. George J. Brush, by solution in muriatic acid, evaporation to separate silica, precipitation with ammonia, solution of the washed precipitate in the smallest possible quantity of dilute sulphuric acid, precipitation of the sulphate of thoria by boiling, washing with boiling water, dissolving in cold water, precipitation by ammonia, washing and dissolving in chlorohydric acid.

[‡] Made from some nearly pure zirconia obtained from Mr. T. Sterry Hunt.

[§] From Mr. Brush.

excess of nitric acid, left a residue also beautifully crystallized; containing but a faint trace of nitric acid and no chlorine. A small quantity of a *blue* substance appeared with the residue, the color of which was destroyed by nitric acid.

35. *Vanadium.* — Pure vanadic acid* gave a beautiful green solution with chlorohydric acid, which, when evaporated with nitric acid, left a fine brown residue containing no trace of chlorine or nitric acid, and insoluble or nearly so in water, being evidently nothing but vanadic acid.

Preparations of the metals *iridium*, *palladium*, *rhodium*, *ruthenium*, and *yttrium* I have been unable as yet to obtain in a state sufficiently pure to afford satisfactory results, and must therefore postpone the consideration of these, together with my general conclusions and deductions from the complete results, until a future communication.

With the chlorids of *sulphur*, *phosphorus*, *selenium*, *tellurium*, *boron*, *silicon*, *titanium*, and *tantalum*, it is of course useless to experiment, for, according to Berzelius, H. Davy, and others, they are all readily converted by contact with water alone, either into the corresponding oxyds, or into combinations of chlorohydric acid therewith which are very unstable and decomposed by evaporation alone. *Osmium* also, according to H. Rose,† is converted by nitric acid into osmic acid, and even by *aqua regia*, no chlorid of osmium being formed in the latter case. The chlorids of *columbium* (pelopium,‡ niobium §), according to the same authority, are immediately and entirely decomposed by water into the corresponding oxyds and chlorohydric acid. So also, according to Woehler, with the chlorids of *tungsten.* ||

In addition to the unsuccessful attempts alluded to in the first part of this paper, to expel all the chlorine from sesquichlorid of iron by nitric acid in conjunction with the deutoxyds of lead and manganese, two others were made, using permanganic acid and chlorate of potash instead of the deutoxyds. On adding to the chlorid nitric acid and a little chameleon mineral, and evaporating to dryness, the residue still

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* From Mr. Hunt.

† Ausführliches Handbuch der anal. Chemie, I. 218.

‡ Ibid. I. 299.

§ Ibid. I. 307.

|| Gm. Handbuch, II. 476, 477.

contained chlorine, and after a repetition of the same treatment, a trace was still found. With chlorate of potash, however, a successful result was obtained; the residue contained no trace of chlorine.*

Separation of Magnesia from the Alkalies.

The only further results of this investigation in condition to be presented, are those of some experiments to test quantitatively the separation of magnesia from the alkalies by evaporating the mixed chlorids with nitric acid, decomposing the resulting nitrates by heat, and dissolving out the potash and soda with water. For these experiments the following preparations were made: —

Chlorid of sodium, by supersaturating pure carbonate of soda with pure chlorohydric acid, recrystallizing and fusing the product.

Chlorid of potassium, by recrystallizing some chlorate of potash, washing with cold water, drying, and fusing carefully and for a long time in a platinum crucible.† The chlorid thus obtained still retained, however, a trace of chlorate or perchlorate, for when dissolved in chlorohydric acid and boiled with a little indigo solution, bleaching took place. To test its purity therefore 0.5809 gram was dissolved in water and evaporated to dryness with an excess of pure chlorohydric acid. The residue weighed 0.5814 gram, and no longer bleached, thus showing the contamination to be unimportant.

Chemically pure *magnesia*, by igniting the pure nitrate very strongly, pulverizing the residue very finely, and washing for a long time. During the washing it absorbs carbonic acid rapidly, cakes together and forms a very hard mass resembling porcelain biscuit, so that in order to continue the washing it is necessary to pulverize it again. It was finally ignited very strongly again. Prepared in this way, magnesia is a dense, heavy powder, of extreme whiteness. 0.4755 gram gave 1.2854 of pyrophosphate of magnesia, corresponding, when Schroetter's equivalent for phosphorus (=31) is adopted, to 0.4362 of magnesia, or only 97.40 per cent. of the quantity taken, the 2.60 per cent. of loss being apparently due to the solubility of the ammonia-phosphate in the dilute ammonia used for washing it. Calculated upon the old equivalent of phosphorus (=31.32) however, the determination gives 0.471 of magnesia = 99.05 per cent., a very fair result. 0.42 gram of the same magnesia was dissolved in chlorohydric acid, the chlorid converted into nitrate by evaporation with excess of nitric acid in a beaker glass, the residue transferred to a platinum crucible, dried and ignited. The residual magnesia weighed 0.4192 gram = 99.81 per cent.

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* When water was poured on the residue, a violet or amethyst color appeared, probably due to a small quantity of *ferrate* of potash.

† In this operation it is necessary to project the chlorate into the crucible in very small quantities at a time, waiting until each preceding portion is entirely decomposed, as the decomposition is very violent and accompanied by great frothing.

0.4392 gram of the above magnesia was then taken, dissolved in chlorohydric acid, mixed with 0.6907 of the pure chlorid of potassium, and 0.9946 of the pure chlorid of sodium, water added sufficient to dissolve the chlorids, then an excess of nitric acid, and the whole evaporated to dryness in a beaker glass on the sand-bath. The complete conversion of the chlorids into nitrates may be ascertained with the utmost readiness in these operations, by the disappearance of the yellow color which the liquid assumes during the evaporation, and which it retains so long as any chlorohydric acid is present. When this color no longer reappears on adding a few drops of strong nitric acid to the hot liquid, all chlorine has certainly been expelled. The vapor from the liquid also, so long as chlorine is present, will bleach litmus paper powerfully.

After expulsion of the chlorine, the liquid was transferred to a large platinum crucible, and evaporated on the sand-bath until the residue was syrupy in consistence. Some caution is necessary during the latter part of this evaporation to prevent spattering. The cover was then placed loosely on the crucible and a high sand-bath heat applied. Red gas was evolved for a considerable time with effervescence until finally the mass solidified. On applying a gentle lamp heat, however, it again fused, and evolved red gas anew with effervescence. My object being to convert the alkalies into carbonates, a piece of platinum foil was placed in the crucible, on which was laid a lump of carbonate of ammonia. Under these circumstances the decomposition proceeded rapidly, and the mass soon presented an appearance of tranquil fusion, with much white sediment, and evolved no more gas. The heat was finally raised to redness for some minutes. On cooling, hot water was added, the solution diluted and thrown upon a filter, together with the washings of the platinum foil and crucible cover; these, together with the crucible itself, being put aside for further use, it being found that a small quantity of the magnesia remained adhering to them. The mass remaining on the filter was washed with boiling water, and the filtrate, which had a strong alkaline reaction, heated in a beaker glass to boiling, and a current of carbonic acid gas passed through, to ascertain whether any magnesia was still held in solution. After a long time a very minute precipitate appeared, but at the same time the liquid became loaded with flitters of silica, proceeding from action of the solution of alkaline carbonates on the glass, and a slight error was thus introduced into the result. The filter with the magnesia was burned in the same crucible, the combustion being found surprisingly easy, and the magnesia weighed 0.44 gram. The precipitate obtained by the carbonic acid (with intermixed *silica*, as well as some *lime*, afterwards found in it, derived also from the glass) weighed half a milligram, making the whole product weigh 0.4405 gram against 0.4392 taken. On dissolving the product in chlorohydric acid, the silica remained behind as a very perceptible residue, and the solution contained also a distinct trace of *platinum*, although the loss of the crucible during the operation was not more than one fifth of a milligram. In these processes, however, it would seem better to use a silver crucible.

In the above experiment no attempt was made to determine the alkalies in the filtrate, but the result with the magnesia being unexpectedly successful, for a first attempt, another experiment, with this in view, was proceeded to. 0.3627 gram of magnesia, 0.6092 of chlorid of potassium, and 0.816 of chlorid of sodium were taken. The mode of proceeding was the same as before, except that the application of the carbonate of ammonia was longer continued, and after the decomposition of the nitrates, the mass was dissolved in hot water, a feeble current of carbonic acid passed through the liquid *while still in the crucible*, and the latter heated on the sand-bath for an hour before filtering. The filtrate was supersaturated with chlorohydric acid, evaporated to dryness, the ignited residue weighed, the chlorine determined as Ag Cl, and the proportions of the two chlorids calculated in the usual manner in indirect analysis. The results were 0.3631 of magnesia, 0.607 of chlorid of potassium, and 0.8111 of chlorid of sodium.

The separation of magnesia from the alkalies in this way may therefore be considered as an accomplished fact, and I venture to think that this mode possesses points of convenience and elements of accuracy rendering it preferable to any of those now in use. For *qualitative* analysis, and particularly for those cases in which it is desirable to combine a qualitative and a quantitative analysis in the same operation, it is particularly convenient. To *mineral analysis* also, it seems to me to be especially applicable. Use and experience will of course increase its reliability and precision, and another communication will be presented on the subject. In conclusion I may remark that similar methods promise to be applicable in numerous other cases in which metallic oxyds are to be separated from the alkalies, at least when these oxyds or their carbonates are insoluble in alkaline carbonates, and their nitrates decomposable by heat, or the vapor of carbonate of ammonia. Such bases are lime, baryta, strontia, zirconia, yttria, thoria, and the oxyds of iron, zinc, cadmium (?), copper (?), cobalt, nickel, lead, cerium, and bismuth (?).

When a comprehensive investigation in which I am now engaged, having for its object to ascertain precisely the action of *heat* upon all the metallic nitrates, shall have been brought to an end, it will be possible to pronounce more definitely upon some of these cases.

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3. On the Action of Nitric Acid in the Cold upon some Metallic Solutions, with New Modes of obtaining Pure Compounds of Barium, Strontium, and Cadmium.

By Prof. Henry Wurtz, of Washington, D. C.

In the course of the investigation which forms the subject of the previous paper, on the action of *hot* nitric acid upon the chlorids, it was discovered that this acid has also a powerful decomposing action upon some chlorids at the ordinary temperature, and this observation rendered imperative the institution of a complete examination of its action upon *all* the chlorids in the cold. This examination is in progress, but some of the results already obtained being of important practical value, I take this opportunity of making them known without delay, in the form of a preliminary notice.

To each chlorid in concentrated solution was added twice its volume, or more, of strong nitric acid. With the chlorids of *barium* and *cad-*

mium, *immediate* precipitates were obtained, so voluminous that unless too much nitric acid is added, the whole liquid becomes almost solid. With chlorid of strontium the precipitate is more slowly obtained. In each of these three cases the precipitate, after being rinsed two or three times with pure nitric acid and dried upon porous earthen-ware, was entirely composed of nitrate, without any chlorid. A solution of *corrosive sublimate* also gave by the same treatment a heavy crystalline precipitate, which appeared however to be *chlorid*, and not nitrate of mercury.

The chlorids of *potassium* and *sodium* did not give immediate precipitates, but on standing for twelve hours in the cold, crops of crystals were obtained which were easily recognized as nitrates. The liquids poured off, these crystals gave, on addition of alcohol, further crystalline precipitates of nitrates of potash and soda. Experiments with the chlorids of *calcium*, *magnesium*, *manganese*, *cobalt*, *nickel*, *zinc*, *copper*, and *ammonium*, the protochlorids of *iron* and *tin*, bichlorid of *platinum* and sesquichlorid of *chromium* have as yet given no results, but are yet to be repeated with modifications. Mitscherlich states* that *nitrate of lime* "is precipitated from its aqueous solution by concentrated nitric acid as a crystalline powder."

The precipitation of solutions of the first-mentioned three metals, *barium*, *strontium*, and *cadmium*, by strong nitric acid, which I find, under favorable conditions, to be almost perfectly complete, the supernatant liquid retaining but traces of the base precipitated, occurs to me to be susceptible of practical use in an important way. It may be doubted whether any method is now known by which barium and strontium compounds, particularly the latter, may be obtained perfectly free from *lime*, or cadmium compounds perfectly free from *lead*, *tin*, *antimony*, *arsenic*, &c. This behavior with nitric acid, however, furnishes us with simple means of accomplishing these desiderata, for the nitrates of the contaminating bases, being present in small quantities, will remain in solution. Nitric acid which has been used for such precipitations is as good for many common uses as before, and may be concentrated and used over again for the same purpose, or made pure again by simple distillation.

It is unnecessary to do more than allude to the importance of being able to obtain compounds of these metals *perfectly* pure, especially for the determination of their equivalent numbers. The chlorids of barium and strontium are precipitated also by muriatic acid, so that when pure chlorids are required, instead of nitrates, this acid may be employed. *Chlorid of cadmium*, however, seems to be soluble in chlorohydric acid.

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* Gmelin, Handbuch, II. 212.

4. ON THE DETECTION OF NITRIC ACID IN SOLUTION, WITH OBSERVATIONS ON THE ACTION OF SESQUISALTS OF IRON UPON INDIGO AND METALLIC GOLD, AND ON THE NEUTRALIZATION OF THE COLORS OF METALLIC SOLUTIONS; WITH EXPERIMENTAL ILLUSTRATIONS. By HENRY WURTZ, of Washington, D. C.

IN the course of an investigation now in progress, the first part of which has already been presented, I have had constant occasion for a good practical method of determining with certainty the presence or absence of nitric acid in small quantities under all possible conditions. The different methods that have been devised for the accomplishment of this important qualitative operation, were therefore submitted to examination, and the results obtained seem worthy of being presented in a collected form. The following are the most important of these methods.

1. The *protosulphate of iron test*, or method of Richemont. This depends upon the well-known reddish or purplish-brown color produced by the combination of the nitric oxyd formed by the deoxydation of free nitric acid by proto-compounds of iron, with the excess of the iron compound. H. Rose, in the last edition of his Handbuch,[*] ranks this test above all the others, and certainly, when manipulated as there directed by Rose, Richemont's test is scarcely surpassed in delicacy by any other; and when the liquid to be tested is colorless, it is extremely convenient; but unfortunately the color produced has too little about it that is *characteristic*, and is therefore not certainly recognizable in the

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[*] I. 659.

presence of many other colors that are of very frequent occurrence. When the quantity of nitric acid is minute, this test requires also a very considerable *length of time*, an important objection.

2. The *iodine test*, or method of Higgin,* dependent upon the setting free of the iodine of iodid of potassium by free nitric acid, and formation of the blue starch compound. When no other free acid than the nitric is present, this mode may be made very delicate and reliable, but when in combination, the nitric must be set free by the addition of sulphuric acid, and as Gay-Lussac long ago observed, sulphuric acid alone gives, with a solution of iodohydric acid, or, which is the same thing, with iodid of potassium, free iodine, by the following reaction, $SO^3 + HI = SO^2 + HO + I$.† Even in dilute solutions this appears to take place after a time, and when but traces are to be tested for, it is difficult to be certain whether the free iodine indicated is due to nitric acid or to this reaction. With great care, however, I believe that this method may in some cases be made useful, particularly when *free* nitric acid is to be tested for.

In the presence of the least trace of a *sesquisalt of iron*, this method is especially fallacious, a fact which has, I believe, heretofore escaped attention. I find that a deep blue color is immediately produced when but a minute trace of any such compound is present. In fact, the conclusion that I have drawn from experiment is, that a mixture of iodid of potassium and starch solutions when used as a test for sesquioxyd of iron in solution, yields, in delicacy and sensitiveness, to no other mode now in use, and I wish, therefore, to propose it as a new method of detecting the presence of small quantities of iron. The reaction takes place both in neutral and acid solutions; with sesquichlorid of iron it is, of course, as follows,

$$Fe^2 Cl^3 + KI = 2\,Fe\,Cl + K\,Cl + I.$$

3. *Schæffer's test;* presented to this Association by G. C. Schæffer at the New Haven meeting, in 1851.‡ It depends upon the conversion of the nitric into *nitrous acid* by the action of metallic lead, and then treating with yellow prussiate of potash and acetic acid, when a deep

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* Chem. Gazette for 1850, p. 249.

† Gmelin's Handbuch, I. 689.

‡ Am. Jour. Sci. [2], XIII. 117; Liebig und Kopp's Jahresbericht für 1851, 625.

golden-yellow color is developed. This test is extremely delicate, indicating, according to Schæffer, the presence of saltpetre in 60,000 parts of water. As, however, the nature of the yellow compound formed is not precisely known, much less the character of the reaction that takes place, we have no data from which to infer how the presence of other substances, particularly of metallic salts, would modify the result, and I should accordingly hesitate to rely upon it without much previous investigation.

E. W. Davy has since given a somewhat similar process,* dependent on the formation of a *nitroprussid* by heating with yellow prussiate and chlorohydric acid, adding an alkaline carbonate when cold, and then sulphid of ammonium to the filtrate, to produce the familiar brilliant purple color, by which he detected the two hundredth part of a grain of saltpetre. It is desirable that this beautiful method should be fully examined, and the modifying influences of all other substances determined, a work which would require much time and labor.

4. The *brucine test.* This depends on the deep blood-red color given by brucine in contact with free nitric acid, and is lauded as extremely sensitive by Fresenius in his last edition.† It is necessary to success, however, that the nitric acid should be in a certain state of concentration.

5. The *gold test;* founded upon the power of mixtures of nitrates with chlorohydric acid to dissolve *gold leaf*, by virtue of the chlorine set free, the gold being afterwards detected in the solution by protochlorid of tin.

An observation of a very surprising and unexpected character, made in connection with this mode of testing, may be appropriately introduced here. I found that solutions containing sesquioxyd of iron (no nitric acid whatever being present) have the power to dissolve gold. A solution of pure sublimed sesquichlorid of iron was boiled with a fragment of gold leaf. The liquid soon became turbid, from deposition of a basic chlorid, as is always the case with neutral solutions of ferric salts, but on acidulating with chlorohydric acid, it cleared up and the gold had disappeared. Another fragment also dissolved, and so on.

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* Lieb. and Kopp's Jahresb., 1853, p. 654.

† "Diese Reaction ist ungemein empfindlich." Fresenius's Anleitung zur qualitiven chemischen Analyse, Braunschweig, 1856, p. 175.

The action is accompanied by the production of protochlorid of iron, as is easily shown by a solution of ferridcyanid. On filtration, dilution, and treatment with sulphohydric acid, the precipitate obtained in the liquid had quite a dark-brown color, and when washed, dried, and burned on platinum foil, left a residue easily proved to be spongy gold by burnishing in an agate mortar.

A solution of pure ferric sulphate was also proved to dissolve a little gold, though less readily than the sesquichlorid, and to be reduced to ferrous sulphate. Even at the ordinary temperature a solution of the sesquichlorid, standing for several days in contact with gold leaf, gave a green tinge with the red prussiate, indicating reduction. An acid solution of the sesquichlorid is also partially reduced by digestion for a few hours on the sand-bath with clippings of *platinum* foil. The solutions of gold are not affected by refrigeration, concentration, dilution, or filtration.

I cannot find that this action of gold and platinum upon ferric solutions has been announced before. If observed, it may have been attributed to traces of nitric acid or chlorine still remaining in the liquid, a cause of fallacy not existing when the sublimed sesquichlorid is experimented with. Metallic silver, however, has been observed to reduce ferric salts. Gmelin says, "metallic silver takes up oxygen from a boiling aqueous solution of the sulphate of sesquioxyd of iron, so that sulphate of silver and sulphate of protoxyd of iron are formed; in the cold, metallic silver precipitates again, and the solution contains only sulphate of sesquioxyd of iron as before." *

It is evident that this behavior of gold vitiates entirely the gold test for nitric acid in the presence of iron.

6. The *indigo test.* This familiar method needs no description. When chlorohydric acid (or chlorid of sodium, as recommended by Liebig)† is added, the bleaching is undoubtedly due to the production of free chlorine, just as the solution of gold under the same circumstances was proved to be by Gay-Lussac.‡

All the five methods previously mentioned depend evidently upon the *production* of a color not previously existing in the liquid, and which in many instances may be produced by the presence of other substances besides nitric acid, whilst the indigo test depends upon the *destruction* of a color previously existing, this color being at the same time a very characteristic one, seldom found in solutions, and belonging to a very stable compound that is well known to be affected only by the most potent oxydizing agents. It seemed to my mind therefore, that, other things being equal, the indigo test possesses elements of greater certainty than the others, and furnishes stronger *positive* evidence of the *presence* of nitric acid, though some others may give stronger proof of its *absence*. I set myself at work, therefore, to investigate the indigo test, and ascertain the circumstances which can modify its action.

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* Gmelin's Handbuch, I. 128.

† See H. Rose's Handbuch, last edition, I. 664.

‡ Liebig and Kopp's Jahresb. 1847–1848, 389.

I quickly discovered, as announced in a former paper, with surprise I must admit, that like the gold test, it is wholly fallacious in the presence of compounds of *iron*. A solution of pure neutral sesquichlorid of iron was found to bleach indigo powerfully, with formation of protochlorid. The iron solution used was freshly prepared from green crystals of the sublimed sesquichlorid.* Excess of chlorohydric acid does not affect the result. A solution of pure ferric sulphate, made by evaporating the sublimed sesquichlorid with excess of pure sulphuric acid until free from chlorine, also bleached indigo powerfully; and when a small excess of indigo was added so that the liquid had a perceptibly bluish tint after long boiling and cooling, crystals of pure chlorid of sodium being added produced no further change after renewed ebullition, thus showing that the bleaching power of the sesquisulphate is little, if any, inferior to that of the sesquichlorid. Red prussiate indicated also in this case a reduction to ferrous sulphate.†

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* It was observed that such a solution, after exposure to the *light* for some weeks, contained traces of protochlorid, confirming an observation of Osann to the same effect (Lieb. and Kopp's Jahresb., 1855, p. 406). Such a solution must therefore be kept in the dark. I may remark here also, that it is advisable not to *filter* such a solution through paper, as it was found, after standing for a short time in contact with purified filter paper, to give a green color with the red prussiate.

† It may be added here that some other vegetable colors were tried, such as acid solutions of litmus, logwood, and cochineal, and found to be likewise readily decolorized by sesquichlorid of iron.

When iron, then, is present in a liquid to be tested by indigo, it is necessary to precipitate it first, and test in the filtrate after addition of excess of acid. For this precipitation I have recommended, in a previous paper, the use of *ammonia*, but since that was written the idea has occurred that the presence of ammoniacal salts may possibly interfere in some way with the reaction, although it may be difficult to understand how. An experiment, however, in which two parallel trials were made, every thing else being equal, but in the one case a large quantity of sal-ammoniac (free from iron) added, seemed, in its result, to justify this apprehension. It is *safer*, therefore, until this point is more distinctly determined, to precipitate the iron in every case with *carbonate of soda.* And when ammoniacal salts are already present, the ammonia may easily be expelled by evaporation with excess of carbonate of soda before testing.

Besides ferric solutions, the following substances were found to have the power of bleaching indigo: —

Terchlorid of gold, after evaporation to dryness with a large excess of chlorohydric acid, to remove all possible traces of nitric acid, gave a solution which bleached indigo very largely when boiled with it, gold being reduced and deposited, partly as a metallic mirror upon the sides, and partly as a black powder, at the bottom of the test tube. When boiled with a large excess of indigo so as to remain still blue, then filtered and the blue color destroyed by nitric acid, the liquid was not affected by sulphohydric acid, and no longer contained therefore any gold.

Bichlorid of platinum, freed in the same way from all nitric acid, bleached indigo powerfully, with production of a very deep brown color (protochlorid of platinum) no precipitate being formed.

Bichlorid of tin, made by passing chlorine gas through a solution of the protochlorid to saturation, adding some chlorohydric acid, evaporating to dryness and heating the residue until it volatilized strongly in white fumes, to expel all excess of chlorine, gave a solution which bleached indigo readily and copiously even *in the cold.* Acidulation with chlorohydric acid did not affect the action.

Pure *arsenic acid*, having been evaporated in solution twice with chlorohydric acid to insure absence of nitric acid, left a crystalline residue which still contained much arsenic acid, for its solution gave with nitrate of silver the brick-colored precipitate, but which apparently retained also chlorohydric acid in some form of combination. On boiling the aqueous solution of the residue with indigo solution no bleaching action could at first be obtained; but on addition of a little chlorohydric acid the blue color, though slowly and with some difficulty, vanished.

It is scarcely necessary to remark that all substances which evolve chlorine, iodine, or bromine, with muriatic acid, will also bleach indigo, such as KO^3, $Ba\,O^2$, $Ce^2\,O^3$, the *chromates*, the *deutoxyd* and salts of the *sesquixoyd of manganese*, *manganates* and *permanganates*, *chlorates*, *perchlorates*, *iodates*, *bromates*, *deutoxyd of lead*, and the *sesquioxyds of nickel* and *cobalt*.

A mixture of deutoxyd of manganese with dilute sulphuric acid bleaches very strongly.

There are a few other substances which interfere, although in another way, with the reaction. These are those bases which give precipitates with the sulphuric acid of the indigo solution, such as *baryta*, *strontia*, *lime*, and *oxyd of lead*. Such precipitates, by making the liquid milky, render the change of color during the bleaching less discernible; and moreover, it was found that the indigo, being insoluble in other acids, all goes down with the insoluble sulphate, so that the filtered liquid is colorless. These bases are readily removed, however, at the same time as the iron, by precipitation with carbonate of soda and filtration, before applying the test.

I now come to a class of difficulties which at first sight seemed less easy to obviate. These are the cases in which the *color* of the liquid to be tested approaches more or less to that of the indigo solution itself. This occurs with solutions of *cobalt*, *nickel*, *copper*, *chromium*, and *uranium*. The mode devised for overcoming this was perfectly successful in every case. It consists of an expansion of the happy idea of Dr. Gibbs (founded upon an observation of Mauméné*) in applying Crum's test for manganese to cobalt and nickel solutions, and has been repeatedly alluded to in a former paper. Some further details, however, of a special character are necessary here.

The chlorids of cobalt and nickel are used reciprocally, after the manner of Dr. Gibbs, to neutralize the colors of each other. The color of the mixture, as I have obtained it, is, when concentrated, rather *grayish* than *brownish*, as it is designated by Dr. Gibbs.* This slight residual color is, however, of no consequence, as all that is necessary is to get rid of all admixture of *blue* tint, before adding the indigo solution. In working with cobalt solutions it must be carefully borne in mind that the ordinary rose-colored hydrate of chlorid of cobalt is converted *in solution* into the blue anhydrous chlorid by admixture with strong chlorohydric acid, or even with the dilute acid on heating, so that in the application of the indigo test to a mixture of chlorids of cobalt and nickel, on heating to effect the bleaching, the liquid usually assumes a blue color deeper than it had before. If nitric acid is present, however, on cooling the hot mixture by addition of a little cold water, or better by a current of cold water on the outside of the test tube, the blue color wholly disappears.

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* Liebig and Kopp's Jahresbericht für 1850, 156.

The green colors of solutions of copper, chromium, and uranium are neutralized in the same manner as solutions of nickel, with chlorid of cobalt. In the case of copper, it must be remembered that, as Gladstone has shown,† chlorid of copper, like chlorid of cobalt, differs in color according to the quantity of water it is combined with, being in dilute solutions *blue*, but on admixture with strong muriatic acid or on heating, assuming a peculiar *greenish* tint. Now it is only the *green* chlorid which neutralizes the chlorid of cobalt, so that it is necessary to have sufficient muriatic acid present to insure the existence of the copper salt in the green modification, at least while the liquid is *hot*. The color produced is then very similar to that obtained with the chlorids of cobalt and nickel. In the green-yellow solution of chlorohydrate of uranic acid, the green tint is perfectly obliterated by chlorid of cobalt, a light *salmon* tinge being produced which does not interfere with the indigo test. A solution of chlorid of chromium is precisely similar in its action to one of chlorid of nickel, the resultant color with chlorid of cobalt being *identical* in appearance. A concentrated green solution of protochlorid of iron is also rendered almost completely colorless by a little chlorid of cobalt, but this is of course a matter of no importance in connection with the indigo test.*

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* The chlorid of cobalt used was chemically pure, having been prepared by the elegant method recommended by Gibbs and Genth, in their late "Researches on Ammonia-cobalt bases" (Am. Jour. Science [2], xxiii. 260), which consists in igniting the violet crystals of the *chlorohydrate of purpureocobalt*, dissolving the residual mixture of chlorid and sesquioxyd of cobalt in dilute muriatic acid, and expelling the excess of the latter by evaporation. The solution has an extremely rich rose color. The chlorid of nickel used was also prepared with care, and was of high purity.

† Lieb. and Kopp's Jahresb. 1855, 414.

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* A few other observations were made upon the effects of mingling colored solutions. The nitrates of cobalt and nickel when mixed in proper proportions neutralize each other just as the chlorids do. With the chlorid of nickel and the nitrate of cobalt, the neutralization is probably more perfect than when the two chlorids are used. With chlorid of cobalt and nitrate of nickel, the resultant color was rather brownish than grayish. The nitrates of copper and cobalt did not seem to be entirely complementary; for on gradually mixing their solutions, the red color of the excess of the cobalt compound began to appear before the blue of the copper solution had entirely vanished, giving a violet tinge to the mixture. On adding a little muriatic acid, however, the copper be[illegible] converted into the green modification of the chlorid, and every trace of blue disappeared, leaving the liquid with the peculiar gray color. With nitrate of copper and chlorid of cobalt also, no perfect neutralization could be effected, but the same gradual passage of one into the other presented itself. Addition of muriatic acid, however, produced the same effect as before. These observations, and others that I have made, have an obvious bearing upon the question of the manner of interdistribution of two acids and two bases in a solution together, and promise to furnish us with a new and valuable means of investigating this important problem. The subject will therefore be resumed at a future opportunity.

There does not seem to be much present prospect of devising any substitute for the expensive chlorid of cobalt for nullifying blue and green colors, at least in the indigo test. The color of the *manganese* chlorid is too feeble. *Permanganic acid* which approaches in color to cobalt solutions, is of course inapplicable. On throwing a little chameleon mineral into very dilute acetic acid, a rich amethyst-colored solution is immediately formed, with effervescence from escape of oxygen. This solution destroys the green color of chlorid of nickel as perfectly as chlorid of cobalt, the resultant tint moreover being extremely similar; with chlorid of chromium in certain proportions an approximate color was produced, but in whatever proportions the two were mixed, a trace of residual red or blue was always left; with chlorid of copper a fine deep violet-blue color, like that of the ammonia-sulphate, was produced.

I desire here to urge upon the attention of chemists this general subject of the mode of interference of colors with each other when in admixture. It seems to me to present a wide and highly promising field of research, the exploration of which would lead to valuable practical results, and probably furnish us with explanations of many phenomena at present considered obscure. Indeed, results of the latter

In conclusion, to describe the general *mode of manipulation* which I adopt, having found it to add materially to the delicacy and reliability of the test. The liquid to be examined is treated, when any interfering substance is present, as above recommended. If neutral, chlorid of sodium is added, instead of muriatic acid, to avoid dilution. If carbonate of soda has been used to precipitate iron, lead, baryta, &c., it is better to *concentrate* somewhat before acidulating with chlorohydric acid. Indigo solution (which should also be strong) is then added until a feeble blue tint (best perceived by holding the test tube *inclined from* the operator on a piece of white paper) is communicated. The mixture is then warmed, and concentrated oil of vitriol added *cautiously* (to avoid explosive ebullition) with constant agitation. If but traces of nitric acid be present the bleaching may not take place for some minutes, or until a considerable quantity of oil of vitriol has been added. If the oil of vitriol be free from iron and nitric or nitrous acid (which latter two contaminations may be removed by previous boiling with a little oxalic acid, after the method of Löwe), it is not necessary that it should be otherwise pure. After the bleaching point has once been reached, if a fresh portion of indigo solution be added, it will generally be bleached *immediately*. In this way I detect practically and certainly one part of NO^5 (as saltpetre) in 25,000 parts of water. In 50,000 parts the reaction is no longer distinct, and the liquid must be concentrated after addition of excess of carbonate of soda. The function of

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kind have already been foreshadowed and exemplified by the investigations of Gorgen upon the colors of manganese compounds (Liebig and Kopp's Jahresb. für 1858, 358; Am. Jour. Science [2], xvi. 416), which explain perfectly the discrepant observations previously made upon these colors, by the interfering colors of impurities. In connection with such investigations a *scale of colors*, founded upon fixed natural standards, if such a thing could be devised, would of course be of great importance. The modifying influences exerted upon the colors of *precipitates* by the presence of other substances precipitated with them, is another branch of the subject fully worthy of special investigation, and would probably lead us to new modes for the qualitative detection of certain substances.

I have made attempts to obtain crystallized double chlorids corresponding to the neutral tinted mixtures of chlorid of cobalt with the green and blue chlorids, but have obtained only negative results. The cobalt chlorid crystallizes out separately or at least only mechanically mixed with a little of the other chlorid. It would be very interesting to determine quantitatively the *proportions* of the several other chlorids which produce the neutral tint with that of cobalt.

the sulphuric acid is not merely, as appeared to me at first, to appropriate the water present, and produce a *quasi-concentration*, for chlorid of calcium cannot be successfully substituted for it. It seems probable that in the presence of a large excess of sulphuric acid, Gay-Lussac's $NO^2 Cl$ and Cl^2 are formed, instead of $NO^2 Cl^2$ and Cl. When this mode of manipulation is used, the presence of ammoniacal salts does not seem to exert any unfavorable influence.*

No. 5. PREPARATION OF SOME PURE SULPHATES, AND PARTICULARLY OF SULPHATE OF COPPER. By Prof. HENRY WURTZ, of Washington, D. C.

THE commercial *blue vitriol* always contains sulphate of protoxyd of iron, which cannot be separated by crystallization, owing to the fact, determined by Mitscherlich,† that sulphate of iron, when crystallizing with sulphate of copper, assumes the pentohydrated composition and triclinic form of the latter, and crystallizes with it in the same crystal. It has appeared to me, therefore, desirable that some special method be devised for the separation of the iron, both in order that the chemist may possess a source of pure copper compounds, and because the presence of iron must be injurious in those processes of dying, calico-printing, &c., in which blue vitriol is used, and in the preparation of pigments such as *verdigris*, *Paris green*, &c.

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* Before leaving the subject I must present some further observations on which I cannot but *hope* to found a method of testing for nitric acid in the presence of iron. I find that metallic iron (*pulvis ferri*) reduces ferric to ferrous oxyd readily even at the ordinary temperature, whilst nitric acid (free from NO^3) unless very concentrated, does not reoxydize the protosalt. In fact it is already well known that in dilute acid solutions containing nitric and no nitrous acid, iron decomposes water just as in dilute sulphuric acid, and ferrous nitrate is formed. If then means could be contrived to obviate wholly the interference of atmospheric oxygen during the operation of *filtration*, it would be very easy, by acidulating the liquid to be tested with chlorohydric acid, adding a little protosulphate of iron and *pulvis ferri* in the cold, filtering, boiling the filtrate (during which last operation ferric salt would be formed from the ferrous, at the expense of the oxygen of the nitric acid) and then testing for sesquioxyd of iron, to detect nitric acid in the minutest quantities. This would also furnish us with a means of separating qualitatively nitric and nitrous acids.

† Gmelin's Handbuch der Chemie, III. 461.

In undertaking this, my aim has been to produce a method by which the iron may be directly separated or abstracted from the solution, without the permanent introduction of any foreign substance. My process consists essentially of two steps, the first of which is the conversion of the protosulphate of iron into sesquisulphate by boiling with a little *deutoxyd of lead*, and the second is the complete precipitation of the sesquisulphate by boiling with a little *carbonate of baryta*. The hot solution is then filtered and allowed to crystallize.* The deutoxyd of lead is prepared for this purpose by boiling minium with dilute nitric or acetic acid in the ordinary way. But minium itself, if free from soluble impurities, may be directly used. Deutoxyd of barium might also be substituted.

The above affords us a process adapted for the use of the analytical chemist or pharmaceutist, by means of which we may obtain pure crystals of sulphate of copper immediately from the commercial article, but of course in the practical preparation of the salt on a large scale, for the use of the calico-printer, and for pigments, modifications somewhat more economical may be made use of. Thus, as for these purposes the presence of lime is of no moment, *carbonate of lime* may be substituted for carbonate of baryta, and common minium for the deutoxyd of lead. In addition it may be remarked that if the blue vitriol contains traces of manganese, as is extremely likely to happen, this contamination, as would appear from the observations of Schönbein † and Wolcott Gibbs,‡ will also be removed. The same treatment is evidently applicable to some other sulphates. I therefore present it as a general method for the removal of iron from sulphates of bases precipitated with difficulty by carbonate of baryta, namely, those of the alkalies, magnesia, manganese, zinc, cadmium, mercury, nickel, cobalt, and protoxyd of iron. Of these the most important one practically is the sulphate of magnesia. The mode of proceeding with this would be precisely similar to that with the copper salt, as it would be also with the sulphates of zinc, cadmium, mercury, nickel, and the alkalies. With regard to the manganous sulphate, it must be remembered that deutoxyd of lead, as observed by Schönbein, wholly precipitates sulphate of manganese from its boiling solution, so that the deutoxyd must be used only in small excess over the quantity *necessary*. The same precaution applies, in a modified manner, to the treatment of the sulphate of cobalt, because Gibbs states that the salts of this metal are also partly precipitated by long boiling with the deutoxyd, passing first to a higher state of oxydation. As to the protosulphate of iron, I have already (New York Jour. Pharm. I. 229) recommended the use of carbonate of baryta for the removal from it of sesquioxyd of iron. In the same paper (which has, I believe, escaped notice from other journals, owing probably to the irregularity with which the exchanges of that now extinct publication were effected), I have given, among other observations upon the purification of copperas, another method of obtaining it free from sesquisulphate, namely, by acidulation with sulphuric acid, and agitation with a little pulverized *protosulphid of iron*, by which the sesquioxyd is reduced to protoxyd.

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* Specimens of the crystals were exhibited to the Section.

† Pogg. Annalen, LXXVII,I 162.

‡ Contributions to Analytical Chemistry, Am. Jour. Science [2], XIV. 204.

To return again to the sulphate of magnesia, before leaving the subject, I must remark that the treatment with carbonate of baryta, according to a former observation of my own, given in a paper published also in the same journal,* must remove also entirely another contamination universally occurring in commercial Epsom salt, namely, *sulphate of lime;* for, as I have there stated, carbonate of baryta totally precipitates gypsum from its solutions, even in the cold; and I have there proposed it as a means of removing the gypsum from spring or sea-water which is to be used in steam-boilers, as well as from the brine of salt-works. I have since found that *carbonate of lead* has the same power of precipitating gypsum, and may possibly be a cheaper agent than carbonate of baryta, when it is considered that from the resulting mixture of sulphate of lead and carbonate of lime, metallic lead might easily be made, or even carbonate of lead reproduced. When we call to mind the fact that the *crystallization* of the gypsum from the boiling water is the *principal*, and usually the *only* cause of the formation of the destructive boiler-crusts, and that leaving out of view the injury sustained by the boiler, the actual *loss of heat* by the non-conducting power of the crust and other causes connected with it, has been shown to amount, in locomotive boilers (in France) in many cases to forty per cent., this matter may be considered worthy of further attention, and I shall therefore present some brief calculations relating to it. Thus in a saturated solution of sulphate of lime (say in round numbers 1 in 400 parts), each 800 lbs. = 800 pints = 100 gallons will contain 2 lbs., and each 1,000 gallons 20 lbs. of gypsum; which is equivalent to 24 lbs. of pure carbonate of baryta, or a little over 31 lbs. of pure carbonate of lead.

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* N. Y. Jour. Pharm. I. 161; also Chem. Gazette for 1852, p. 268; Pharm. Jour. and Transactions. XII. 77; Dingler's Polyt. Jour. 125, 275; also quoted in Liebig und Kopp's Jahresb. 1852, page 360.

But water used for steam purposes, sea-water for instance, is seldom or never a *saturated* solution of gypsum. Let us take Bibra's analyses of the waters of the Atlantic,* collected at four different times, at as many widely distant places. These give in 100 parts 0.205, 0.156, 0.16 and 0.19 parts of sulphate of lime. For simplicity take the highest, 0.2; then 1,000 gallons = 8,000 lbs. contain 16 lbs of gypsum. The quantity of carbonate of baryta equivalent to this is 19.2 lbs., and of carbonate of lead 25 lbs.

Thus one ton (2000 lbs.) of carbonate of baryta, which is sold in England in the form of ground *witherite*, for $20, is theoretically sufficient to separate the gypsum from 100,000 gallons of the water of the Atlantic Ocean. On referring to the evaporative powers of different coals as determined by Walter R. Johnson for the Navy Department of the United States,† we there find that 1 lb. of Lackawanna anthracite, considered in New York a superior coal for steam purposes, produces, on an average, 8½ lbs. of steam from cold water; so that to evaporate 100,000 gallons would require $\frac{8 \times 100{,}000}{8\frac{1}{2}} = 94{,}000$ lbs., or 47 tons. 40 per cent. of such a quantity would be over 18 tons.

I would not, however, by any means recommend the application of this principle to *ocean steamers*, on account of *practical* difficulties which seem insuperable. But *on land* the application is a matter of perfect simplicity. It is only necessary to have the tanks from which the boiler is supplied somewhat larger, and to have more of them. While the process of mixture and agitation with the carbonate, and precipitation by standing, is going on in one tank, water may be used from another which has undergone the process. The idea seems to me to be especially applicable to *locomotives*, for which even *sea-water* may then be used.

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* Liebig und Kopp's Jahresb. für 1850, p. 621.

† Taylor's "Statistics of Coal," Introduction, page lvii.; Knapp's Chemical Technology, Am. Ed. I. 43.

6. ON MODES OF INCREASING THE HEAT OF THE MOUTH BLOWPIPE, AND SOME NEW BLOWPIPE MANIPULATIONS, WITH EXPERIMENTAL ILLUSTRATIONS. By Prof. HENRY WURTZ, of Washington, D. C.

IN the course of some blowpipe investigations which I have in progress, it has been found extremely difficult, and sometimes impossible, to obtain in the ordinary way sufficient heat for the production of certain desired effects. Attempts were therefore made to devise means of increasing the heating power of the instrument, and this object has been so far attained, and by means so simple and efficient, that I take this opportunity to make these means public, that others may also be benefited thereby. To blowpipe analysts it is not necessary for me to detail the advantages to be gained in many cases by a practicable mode of increasing the heat, of which advantages not the least important is the saving of time.

It was first observed that in the ordinary mode of manipulation, a great part of the heat was conducted away from the bead by the cold part of the platinum wire contiguous to it. This is easily prevented by simply bending the wire previously, as represented in the figure, at right angles about an inch or an inch and a half from the loop that is to hold the bead.* On then holding the bead at the point of the blue cone of the flame, and the wire so that the bent portion is coincident with a continuation of the axial line of the flame, this bent portion becomes also heated to high redness, losing thus in a great degree its tendency to abstract heat from the bead. By this little contrivance alone the heat is increased to so important a degree, that I venture to think that no one who has once tried it, will ever use a wire of any other form.

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* At the time the above was read, I had no suspicion that the contrivance of bending the wire had ever been published before, or thought of by any other than myself. Since, however, Prof. Brush of New Haven has directed my attention to a passage in the last edition (1853) of Plattner's "Probirkunst mit dem Löthrohre,"

I next directed my attention to the *combustible* used. An ordinary alcohol flame, as every one knows, gives with the blowpipe a comparatively feeble heat. A gas flame is much superior, and a large *wax candle* gives probably a higher heat than any thing else at present commonly in use among blowpipe operators. It occurred to me that the heating effect was probably proportional to the *density* of the burning vapor, or the quantity of combustible matter contained in the same volume. I therefore searched for combustibles having a high density of vapor, and found that the *paraffine* of Reichenbach, now known from the investigations of Hofstaedter* and Filipuzzi† to be a mixture of different isomeric hydrocarbons, all of which must have very high equivalents, was found by Lewy‡ to have a vapor-density of not less than 11.8. By inquiry, I found that candles composed of this, or a similar material, obtained from the products of distillation of the well-known "Breckenridge Coal," could be bought in New York. On procuring some of these, and using them as *pabulum* for the blowpipe-jet, I found my anticipations fully realized. The flame obtained by means of the paraffine candle is much hotter than that from a wax candle. Unfortunately the candles made in New York are small, and have *extremely* small wicks, which renders them difficult to manipulate with, because the least motion of the jet-piece, by throwing it out of the centre of the flame, deranges the form of the cone. This can of course be easily remedied by having larger candles made with larger wicks.

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page 14, where the same device is identically described as used for fusing platinum wire. I desire, therefore, stating at the same time that I have been in the habit of using it for more than ten years, to disavow all claim to priority, hoping still that some novelty may be found in my modes of making use of the invention. — H. W.

* Liebig and Kopp's Jahresbericht, 1854, 608.

† Ibid. 1855, 630.

‡ Loewig's Chemie der organische Verbindungen II. 564.

Next, and lastly, *the blowpipe itself* seemed to me susceptible of improvement, by the introduction of an agent to absorb the moisture and carbonic acid of the breath, which must necessarily diminish the heating power of the flame. I have therefore sought to eliminate these obstacles by using, instead of the tube of the ordinary blowpipe, a somewhat larger tube filled with fragments of caustic potash. In the instrument which I have heretofore used, and which I now exhibit, this tube is composed of glass, and is united with the jet-piece of an ordinary blowpipe of Berzelius' form by means of a perforated cork; but of course in practice this tube may as well be composed of metal to avoid breakage. I use the ordinary *potassa fusa* occurring in the shops in the form of sticks, broken up to about the size of a split pea, the fragments being confined in the tube by a plug of cotton at each end. It is advisable, when this kind of blowpipe is not in use, to keep the upper end of the tube corked. It remains yet to be seen whether the additional advantage gained by using a blowpipe of this construction will compensate for the concomitant inconveniences, though the latter are far less important, according to my own experience, than many would suppose.

Now as to the effects which may be produced by the combination of these several appliances. Platinum wire of medium blowpipe size is fused with little difficulty, and I have obtained beads of considerable diameter. Fine platinum wire melts down like wax. A borax bead volatilizes rapidly in white smoke. By moistening the end of a bent platinum wire, and dipping it into pure carbonate of lime or pure magnesia, and exposing it to the flame, a very fair exhibition of Hare's lime light, in miniature, may be made. The fusion of a small iron wire, and the combustion with brilliant scintillations of a small steel watch spring may be exhibited as class experiments in a very striking manner.

Berzelius, in his work on the Blowpipe,* says, "I am convinced that the temperature produced by the blowpipe, fed by air from the lungs, has a distinctly defined limit; so that, for instance, *alumina* or *silica* cannot be melted, however small fragments of them be employed." Now although I have not yet succeeded in producing large globules of fused silica, yet any person, by adopting the above expedidients, may convince himself that the degree of heat thus obtainable is adequate for the fusion of silica. My experiments were made with chemically pure precipitated silica, and with a fragment of a colorless transparent crystal of quartz, from Herkimer County, New York, finely pulverized in an agate mortar. By taking a small platinum wire, bent as above, first fusing the end into a globule, then moistening this globule with saliva, or better (in order to avoid the introduction of any trace of *basic* contamination, which might be supposed to form a fusible silicate), with *syrup* made from pure sugar, and dipping it into the powder, then gently heating and incinerating in the flame of a spirit lamp, the silica powder remains loosely attached to the wire, and under the lens appears now perfectly impalpable and devoid of transparency. If now the potash-tube blowpipe, with paraffine candle flame, be brought to bear upon it for a minute, the silica being held a little way *outside* of the point of the blue cone, which in this kind of flame appears to be the hottest point, it will then be found strongly adherent to the wire, as if melted fast, and under the lens presents the appearance of small, transparent, irregularly shaped globules, *fused fast* to the platinum. I have little doubt that with a large paraffine candle, and larger jet, a *splinter of quartz* might be fused.*

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* Whitney's Translation, page 46.

The above mode of obtaining a high heat is essential to the practicability of a peculiar mode of manipulation that I have devised, and which I shall proceed to describe.

Many minerals and artificial products, when dissolved in a borax bead *in large quantity*, cause the bead to become opaque and milky on cooling, forming in fact an *enamel*, instead of a glass. Some of these are *lime*, *magnesia*, *baryta*, *strontia*, *glucina*, *oxyds of zinc*, *cadmium* and *cerium*, *tungstic* and *titanic acids*, *calcite*, *magnesite*, *dolomite*, *witherite*, *baryto-calcite*, *strontianite*, *gypsum*, *epsomite*, *heavy spar*, *celes-*

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* Berzelius, in a note, mentions an announcement of H. de Saussure that he melted *quartz*, supported on a slip of *kyanite*, with a jet of air from a *double bellows* through the flame of a thick *wax candle;* adding, however, that he suspects that "the support may have produced an effect on the assay, and that the air from the bellows, being purer than that of the lungs, may have also contributed to effect a result which cannot be obtained by the mouth blowpipe." — *Whitney's Translation*, p. 54.

tine, *goslarite*, *smithsonite*, *fluor spar*, *apatite*, *sphene*, *aeschynite*, *polymignyte*, *yttro-tantalite*, *scheelite*, *scheeletine*, *xenotime*, *yttro-cerite*, *fluocerite*, *cryolite*, &c. &c. A great many more render the bead of *microcosmic salt* opaque or opalescent on cooling, and as some reactions, for instance that of *titanic acid*, are much more easily obtained in this than in the borax bead, these cases are also frequently of importance.

Now when it is desired to detect another substance occurring in small quantity in any of the above, such for instance as *manganese*, *copper*, *cobalt*, or *titanium*, by the color imparted to the bead after cooling, it is frequently impossible to do so, for the reason that if enough is added to give a decided reaction, the bead becomes opaque on cooling, and the color cannot be seen.* In considering the cause of this loss of transparency, it seemed to me that it must be a granular *crystallization*, and remembering the fact that even *common glass*, if made to cool very slowly from fusion, becomes opaque and enamel-like, with many other familiar facts of an allied nature, it occurred to me that the converse of this phenomenon might also take place, or that an enamel, which becomes transparent and vitreous when fused, might retain its transparency when *suddenly* cooled. I was thus led to dip a bead composed of a highly basic borate of lime enamel into cold water while still fused. The result verified the hypothesis. The bead when cooled in this way remained transparent, and manganese was thus distinctly detected in a purely white marble, in which its presence could be distinctly pronounced upon otherwise only by *Crum's test.*

For a number of years I have had constant occasion to use this expedient, and have found it a highly valuable one. Blowpipe operators will not need any further remarks upon the importance of such an addition to our facilities for investigation. It is necessary, however, that careful examination be made of all special cases, a work of time and patience, which I hope, nevertheless, soon to take up systematically.

As before intimated, it is necessary, or at least highly conducive, to the success of this new manipulation, to have means, such as are above detailed, of obtaining an increased heat; because these supersaturated beads, which *enamelize* (if I may be allowed to coin a new but convenient word) on spontaneous cooling, require a much higher temperature for fusion than those which remain vitreous, and without some additional facilities for obtaining this higher heat, the tax upon the time and patience of the analyst becomes very onerous, and in many cases I have found it in fact *impossible* to succeed by the ordinary mode, for when the bead is very highly basic, it must be heated considerably *above* its point of fusion, or it solidifies to an enamel immediately on removal from the flame. It was this necessity which led me to devise the above modes of increasing the heat. It is by no means necessary, however, in ordinary cases, to use *all* of the expedients described. In fact, for the fusion of most of these borax enamels, the *bent wire* alone, with an ordinary blowpipe and an alcohol or gas flame, will suffice, and most others need only the additional assistance of the *paraffine candle.*

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* Speaking of *red zinc ore*, Berzelius says that it is dissolved by phosphorus salt, but that "the color of oxyd of manganese cannot be obtained till the glass has dissolved so large a quantity that it is no longer transparent on cooling." —*Whitney's Translation*, page 116.

While upon this subject of the blowpipe, I may be pardoned for offering briefly another suggestion. While making the experiments upon the reciprocal neutralization of the colors of metallic solutions, described in a former paper, it struck me that the same principle should have applications in blowpipe analysis. It *must be* that when several metallic oxyds are present in a glass, their several colors must interfere with each other in certain ways, and if we knew how to eliminate one or more of them by adding a neutralizing ingredient, so as to render. others apparent, might it not furnish us with new facilities for research, and enable us to avoid old sources of error? I may conclude by noticing two or three results, which I have obtained, bearing upon this question. A deep amethystine bead of manganese glass acquired, on addition of a little oxyd of chrome, a *gray* color, resembling that produced by a mixture of solutions of chlorid of cobalt and chlorid of chromium, without any tinge of either red or green; a trifling additional quantity of oxyd of chrome giving, however, a distinct green color.* Another manganese bead gave a similar gray color on addition of oxyd of copper, but a slight excess of the latter imparted then a *bluish* tinge, which might easily be attributed to a trace of *cobalt*. The presence of nickel does not affect the blue color of a cobalt glass, as it does the rose color of cobalt in aqueous solutions, unless its quantity, compared with that of the cobalt, is very great.

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* In the decolorization of *ferriferous glass* by the addition of deutoxyd of manganese, "glass-makers' soap," is the action *wholly* due to the conversion of the ferrous into ferric oxyd, or *partly* also to the neutralization of a portion of the green ferrous color by some violet manganese glass formed?

7. Preliminary Notice of an Investigation upon the Action of hot Chlorohydric Acid upon some Metallic Nitrates. By Prof. Henry Wurtz, of Washington, D. C.

It has long been known that chlorohydric acid in excess has under favorable circumstances, for instance at an elevated temperature, more or less power to decompose the nitrates and convert them into chlorids; but no systematic investigation having yet been made of this class of reactions, the idea entertained of their nature has rested mostly upon hypothesis. Thus Gmelin says[*] that "chlorohydric acid in excess resolves itself with salts of nitric acid, into chlorid (or chlorohydrate) hyponitric acid and chlorine," giving as an example the equation $KO, NO^5 + 2\,HCl = KCl + 2\,HO + NO^4 + Cl$; but it is far more probable that Gay-Lussac's $NO^2\,Cl^2$ and $NO^2\,Cl$ are formed instead of NO^4. This question will be examined into in the course of the investigation. It appears from the results given below, however, that the reaction, whether it accords with the above equation or not, is in many cases far from being as easy to obtain with precision as the decomposition of the chlorids by nitric acid. This fact would appear to have been remarked before by Dr. Wolcott Gibbs, as is evident from his note quoted in a previous paper,[†] and he there recommends the addition of protochlorid of tin to assist the chlorohydric acid, by its reducing power, in the destruction of the nitric acid; the tin being removed afterwards by sulphohydric acid.

The nitrates of the following bases, after one evaporation with excess of concentrated muriatic acid, yielded residues still containing much nitric acid, and after two repetitions of the same treatment, the indications of its presence were still marked in each case; baryta, strontia, lime, magnesia, glucina, zirconia, thoria, potash, and soda. Nitrates of alumina, lead, nickel, cobalt, and uranic acid yielded to two evaporations; nitrates of copper and zinc to one.

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[*] Handbuch, I. 83.

[†] Am. Jour. Science [2], XVI. 416.

It has seemed to me desirable to substitute for the protochlorid of tin in Dr. Gibbs's method some other reducing agent more easily to be got rid of. Oxalic acid and alcohol seemed to promise best. Nitrate of baryta, therefore, which appeared the most difficult to decompose of the above nitrates, was evaporated as before, with the addition, in one case, of a few crystals of oxalic acid; in a second case, of a little alcohol; and in a third, of some protochlorid of tin. In the first case the liquid turned yellow during the evaporation, as also in the third case, but in the second it remained colorless, giving off with effervescence a strong odor of *nitrous ether*. The residue in the first case still contained nitric acid, in the second and third cases none, but in the third case, a large quantity of white insoluble powder (stannic acid) was formed. Like experiments with the nitrates of potash and soda led to precisely similar results.

8. SOME IMPROVEMENTS IN THE PREPARATION OF HARD MINERALS FOR ANALYSIS. By Prof. HENRY WURTZ, of Washington, D. C.

NOTHING, it may be asserted, tries the proverbial patience of the chemist more than the preparation of hard minerals for analysis. The following suggestions are therefore offered in the hope that they may be of service in lightening this kind of unappreciated toil.

The ordinary mode of reducing to fine powder by first crushing to coarse powder in a diamond mortar, and then triturating in an agate mortar, is open to several serious objections. In the first place, the diamond mortar, of the usual construction, in three pieces, is difficult to manipulate with, and extremely liable to get out of order. Its preservation from *rust*, which immediately destroys its efficiency by causing friction, is almost impossible in the acid atmosphere of a laboratory; and moreover, the quantity of material that can be operated upon at once with it is very small. An obvious improvement, adopted by many chemists, is to use in its stead a *deep* mortar and pestle of the common form, composed of steel, or, far better, of *white cast-iron*, which, besides being harder, is less liable to rust. An instrument of this latter description I have used for several years. Another still more momentous drawback, however, to this as well as to the diamond mortar, is the inevitable introduction into the powder of more or less *iron* in small particles chipped off by the sharp angles and edges of the fragments crushed. The agate mortar is also objectionable, on account of the very small quantity that must be operated upon at a time, and the contamination of the product with *silica*, proceeding from the abrasion of the agate. Some analysts have proposed to overcome this latter objection by *weighing* the agate mortar before and after the operation, to ascertain the quantity of silica ground off; but this involves numerous impracticabilities, one of which is the necessity, with our ordinary balances, of using a mortar of the smallest size, and the others it is not necessary to detail.

It has occurred to me, therefore, that, if the agate were dispensed with entirely, the levigation being completed in the hard white iron mortar; and that, if some means could be devised of subsequently removing the abraded metallic iron from the powder, without otherwise affecting the composition of the latter, a step in improvement would have been accomplished. When the mineral contains no ingredient acted on by *diluted acids*, these means are so obvious that no doubt every analyst has adopted them upon occasion, but the cases in which this occurs are not often met with. Especially when working upon *iron ores*, magnetic, hematitic, and silicious, containing *carbonates*, *phosphates*, &c., is the use of acids inadmissible.

We possess, however, agents which, while readily dissolving metallic iron, are supposed not to act upon its oxyds. *Chlorine water* is such an agent; and in cases where the mineral contains no carbonates, chlorine water may be used without fear of dissolving any *anhydrous* oxyd of iron present. *Limonite*, if present, would undoubtedly be dissolved in small quantity by the sesquichlorid of iron formed. It is scarcely necessary to remark, that chlorine water used for this purpose must be freshly made, and made in the dark, and kept in the dark before and during the operation, in order that it may contain no *acid.*

Aqueous solution of *bromine*, or still better, of *iodine*, may be substituted for that of chlorine. It was hoped that with iodine water, even minerals containing carbonates might be treated, and in the course of an extensive series of analyses of magnetic iron ores (all containing both carbonate and phosphate of lime), upon which I have been engaged, many trials were made in reference to this point. It was found that even when the experiment was made without access of air, and in the dark, traces of lime were dissolved. In fact it could not be otherwise, for an excess of the solvent being present, sesquiiodid of iron must be formed, which is then decomposed by the carbonate.* With chlorine water the quantity of lime dissolved was large.

There is another solvent that may be used to accomplish the same purpose as the above, namely, a *neutral* solution of (sublimed) *sesquichlorid of iron*. This is, however, unfortunately open to the same objections, because, while dissolving the metallic iron, it will also dissolve the carbonates and limonite.

It is plain, therefore, that some additional precaution must be contrived to enable us to attain completely the object proposed. This is fortunately furnished us by the well-known power which solutions of ammoniacal salts have to dissolve the earthy carbonates when boiled with them. The method, then, which I at present propose is to boil a weighed quantity of the powder first with a solution of sal-ammoniac or nitrate of ammonia, until the lime, magnesia, &c., present in the form of carbonates, are dissolved. These are then determined separately, and the remaining powder is treated with iodine water to remove the metallic iron.

Of course the *carbon* contained in the metallic particles will remain, but the quantity of this will be so small that it may usually be neglected.

Other substances, of an oxydable character may be present in some cases to interfere with complete success, such as metallic sulphids, and particularly *pyrrhotine*. A portion of pyrrhotine from the apatite locality near Hurdtown, New Jersey, was digested in fine powder in the dark with a brown aqueous solution of iodine (freshly prepared from iodine which had been ground to fine powder and washed repeatedly to remove all free acid) for forty-eight hours. The liquid had then become colorless, and gave a large green precipitate with ammonia, and a black one with sulphid of ammonium. Pulverized *pyrites* treated in the same way did not remove the brown color of the liquid, and the latter dissolved but a trace of iron.

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* Other results, however, not so easily explicable, were obtained. When iodine water, apparently perfectly free from iodohydric acid, was digested at the ordinary temperature with pure carbonate of lime, both in the dark and in a tight stoppered bottle, the filtered liquid gave distinct traces of lime. This matter will be made the subject of special investigation.

I must not omit to remark, that the iodine solution should not be allowed to stand upon the powder for an unnecessary length of time, especially if air has access to it, for in that case a portion of the iron dissolved will sooner or later be deposited again as a basic salt.

Some further suggestions as to details may be acceptable. The mode which I adopt is to reduce the whole mass of substance to be analyzed first to a coarse powder in the white iron mortar, with a fine sieve in the ordinary way, and then to elutriate it (also in the iron mortar) in small quantities at a time, using, however, in this operation *alcohol* instead of water. I may remark here, that *water* should never be allowed to touch the iron mortar. It should be always washed with *alcohol* and dried perfectly on the sand-bath after use, thus preserving it from rust. If it should have become rusted internally, however, by exposure to acid vapors, some sand wet with alcohol should be ground up in it. An acid should never be used to remove the rust, because cast-iron (as well as steel) is always more or less *porous*, and a portion of the acid will inevitably be absorbed as a protosalt of iron into the pores, to reappear afterwards as sesquioxyd, or basic sesquisalt, on the surface.

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9. CHEMICAL EXAMINATIONS CONNECTED WITH A BULLET WHICH HAD BEEN IMBEDDED FOR MORE THAN FORTY YEARS IN THE LUNG OF A MAN. By Prof. HENRY WURTZ, of Washington, D. C.

WHILE delivering the course on Chemistry in the medical department of Queen's College, Kingston, Canada West, during the past winter, the case in question came under my observation, and seemed to me a rare opportunity for a chemical examination of the action undergone by metallic lead when imbedded in a human viscus; and although the examination made, from want of time and other causes, was very imperfect, the results obtained are probably worthy of being recorded. The following history of the case was drawn up at my request by the gentleman who made the dissection and found the ball, Dr. Alexander Bethune, now a graduate of the institution:—

"William Kelly, aet. 76, a pensioner, a native of Ireland, was admitted into the Kingston General Hospital, September 25, 1855. He had *hemiplegia*, and as it was considered incurable he was sent to the House of Industry, where he remained only a few days, and then returned to the hospital, in which he continued until the day of his death, December 17, 1857.

"Post-mortem appearances: On removing the calvarium more than a pint of bloody serum gushed out. The dura mater was found much thickened and closely adherent. The gray matter of the right hemisphere was softened, and contained a clot the size of a pea. The lining membranes of the ventricles were thicker than natural, and they contained serum and organized lymph, with traces of recent inflammation. When the thorax was opened a prominence was observed in the middle lobe of the right lung, and on cutting into it about the depth of an inch, a bullet was found, enclosed in a cyst, and firmly adhering to its wall at several points. On examining the walls of the thorax, a cicatrix was found between the fourth and fifth ribs, corresponding to the prominence produced by the bullet.

"In the early part of his life, the deceased had been a soldier in the British army, and was engaged in the Peninsular war. In one of the engagements there he received a wound from a musket ball, which entitled him to a pension. As he was engaged in no war but that of the Peninsula, and had no marks of any other wound upon his body, the bullet found in his lung, more than forty years after, must have been that which produced the wound for which he received a pension.

"ALEXANDER BETHUNE.

"*Queen's College, Kingston, Feb.* 1, 1858."

I may add that the wound is believed to have been received at least as early as the last siege of Badajoz, which occurred in the year 1812, and consequently forty-five years ago; and that the hemiplegia was of the right side, or on the same side as the ball.

The ball, as delivered to me, appears to have been a common ounce musket ball, but is irregular in shape, having evidently undergone corrosion. It now weighs 370 grains, indicating a loss, if originally an ounce in weight, of more than one hundred grains. Two spots appeared on opposite sides, where adhesion to the walls of the cyst had existed. These spots are coated with a hard, whitish substance, which cuts like horn, and blackens when heated, with a smell of burning animal matter, leaving afterwards an infusible white ash. Near one of these spots was a small incrustation of a chalky white color, about one eighth of an inch in diameter, which when detached was found to penetrate somewhat beneath the surface of the lead, and when submitted to careful qualitative analysis, was ascertained to consist wholly of *chlorid of lead*, containing no trace of either phosphoric or sulphuric acid. The ball was then boiled in distilled water and gave a liquid hard to filter clear, which contained considerable chlorid of lead. In places the ball had a very thin brown coating, too small in quantity to be satisfactorily determined, but which was probably also chlorid of lead, colored with organic matter.

A piece cut from the wall of the cyst was treated with chloro-hydric acid and chlorate of potash, and the solution examined for lead without success, but the material was so small in quantity that the result is not satisfactory. A piece of the lung itself, taken from the neighborhood of the cyst, was then treated in the same way. The solution obtained gave with sulphohydric acid a small brown precipitate, which was collected on a small *purified* Swedish paper filter, and the latter washed, dried, and burned to ashes. The ash, fused with carbonate of soda on charcoal, and manipulated in the ordinary way in the agate mortar, gave a multitude of spangles of a soft white malleable metal, which being collected on a watch-glass, dried, dissolved in a drop of nitric acid and the solution evaporated to dryness, gave with a drop of a solution of sulphohydric acid a deep brown coloration.

I was very desirous to obtain a portion of the *liver* (which was not yet exposed when I viewed the body), and left directions that it should be saved, which were unfortunately not complied with, the liver having been thrown among a number of like viscera from other subjects in the dissecting-room, so that it could not again be identified. A piece of *muscular tissue*, however, taken from the muscle of the diaphragm, was examined and lead found, apparently in greater quantity than in the lung.

It is evident from these results, that the lead of the bullet has undergone solution in the cyst as chlorid, and passed as such into the circulation, to be deposited again in insoluble forms in other parts of the system, as is usual with the so-called *cumulative poisons.*

The question whether the paralysis and post-mortem appearances had any connection with the presence of the lead, I shall leave for the consideration of medical men and toxicologists.

10. SUGGESTIONS REGARDING ECONOMICAL APPLICATIONS OF GLYCERINE.

By Prof. HENRY WURTZ, Washington, D. C.

IT must be apparent to every one who considers the peculiar qualities of the substance, glycerine, namely, its resemblance to oils in not being volatile at ordinary temperatures, while, unlike them, it is miscible with water, alcohol, &c.; its resistance to congelation, not being perfectly solid even at the freezing point of mercury;* its unchangeability; its agreeable taste when pure, and harmless action upon the system; its wide range of solvent powers; together with the quantity in which it may be cheaply procured; that it must in future fulfil important purposes, not only in pharmacy, but also in the arts. Accordingly, we find that technical applications have already been proposed for it. Barreswil's method† of preserving clay, which is to be used for moulding purposes, in a moist and plastic state, may be alluded to as an example.

Some uses, which are probably new, have occurred also to me.

In the first place, its conjunction of the property of compatibility with human digestion and assimilation, with that of non-evaporation and even *absorption of water* from the air, suggests applications in the preservation of articles of food and luxury which are injured by desiccation. As an example of minor importance, if *mustard*, for table use, were mixed with diluted glycerine, instead of water or vinegar, the usual vehicles, it would retain its liquidity indefinitely without drying up. So of many other condiments.

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* Berthelot, in Lieb. and Kopp's Jahresb. 1854, p. 448.

† Nickles' Correspondence in American Journal of Science [2], XVI. 100.

A more important application, however, of a similar kind, would be in the preparation of articles of *confectionery* composed of sugar, preserved fruits, chocolate, &c., which are frequently met with enveloped in *tinfoil* to prevent their desiccation. The same object might be accomplished more effectually, and probably more economically, by admixture in the process of manufacture with a certain proportion of pure glycerine.

Another article of luxury, of still more extensive consumption, the consumers of which demand that it should be preserved for them in a *moist* state, is that known as "chewing tobacco;" and a vast consumption of tinfoil arises from this requirement of the tobacco chewers. I have repeatedly prepared small quantities of chewing tobacco (the variety called "fine cut") for persons addicted to its use, by admixture with a little glycerine, and always very much to their satisfaction. In the preparation of this drug, the manufacturer must also please the palate of the consumer by introducing some *dulcifying* ingredient. Common sugar or molasses, however, will not answer the purpose, because they render the mass liable to ferment and turn sour. An infusion of the root or extract of *liquorice* is therefore usually resorted to. This does not, however, keep the tobacco *moist*, as molasses would do, and to attain this it is necessary to press into solid, compact masses and pack into tight cases, or, in the case of the finer qualities, to enclose in wrappers of tinfoil. In view of these facts, glycerine will be seen to supply every requirement of the tobacconist, as it will not only keep his product *moist* for an indefinite time, even when exposed to the air, but will also *sweeten* it. He may almost look upon glycerine as made specially for its use.

The common *water meters*, used for measuring the consumption of illuminating gas in houses, are open to two strong objections, namely, when in a warm situation the water rapidly *evaporates*, and when in a cold place it *freezes*. To avoid congelation, the usual expedient is to fill the meter in cold weather with *alcohol* or whiskey, thus rendering the first-mentioned difficulty, that of evaporation, still more inevitable.

Now what liquid do we possess which is practically free from both these objections of evaporation and congelation? Evidently *diluted glycerine.* I propose, therefore, as a substitute for both water and alcohol for filling gas meters, *glycerine* (sufficiently diluted to prevent its absorption of more water from the gas, and increasing in volume to any important extent), thus rendering the meter independent of attention within the ordinary limits of temperature.

For lubricating the bearings of fine machinery also, and particularly of *chronometers*, glycerine seems to me worthy of a trial, as it is unchangeable by the atmosphere, and remains fluid at temperatures which few or none of the oils will resist. For chronometers, pure *oleine* and *oleic acid* have been used, but the former thickens on exposure to the air,* and the latter congeals at a few degrees below the freezing point of water.†

Other uses occur to me, such as in the preparation of *copying ink*, in *water color painting*, and in the preservation of dried plants for *herbaria* in a flexible state; mere allusions to which may at present be sufficient.

11. ON THE THEORY OF THE POLYACID BASES. By Prof. WOLCOTT GIBBS, of New York.

1. A THEORY of the polyacid bases is a necessary complement to that of the polybasic acids. If we define an acid to be a body containing one or more equivalents of hydrogen replaceable by a metal or other zincous radical, we may similarly define a base to be an oxyd, sulphid, &c., containing one or more equivalents of oxygen, sulphur, &c., capable of readily combining with the hydrogen of an acid. From this point of view acids and bases form special cases of the general theory of chemical polarity. If, for the sake of simplicity, we restrict ourselves to the oxygen bases, we may say that in any base the oxygen outside of the radical determines the character of the base, while in any acid the hydrogen outside of the radical determines the character

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* Loewig's Chem. der organ. Verbind. II. 511.

† Ibid. II. 200.

of the acid. That a metallic oxyd usually reacts with as many equivalents of an acid as the oxyd itself contains equivalents of oxygen is a fact with which chemists became familiar at an early period. Thus when sesquioxyd of iron, teroxyd of antimony, and deutoxyd of platinum, come in contact with chlorhydric acid, we have the reactions indicated by the equations

$$Fe^2O^3 + 3\,HCl = Fe^2Cl^3 + 3\,HO. \quad SbO^3 + 3\,HCl = SbCl^3 + 3\,HO. \quad PtO^2 + 2\,HCl = PtCl^2 + 2\,HO.$$

In each of these cases all the oxygen in the metallic oxyd must be considered as outside of the radical, and as this oxygen determines the character of the base, two of the above-mentioned bases are triacid while the third is biacid. Peligot's investigation of the salts of sesquioxyd of uranium proved that this oxyd reacts with but one equivalent of acid, and is consequently uniacid. This peculiarity was easily explained by the ingenious theory of the French chemist, which assumed in the sesquioxyd of uranium a radical consisting of U^2O^2, so that the oxyd is really $U^2O^2 . O$, and necessarily reacts with one equivalent of a unibasic acid. This theory, which was warmly opposed by Berzelius, has never been generally adopted, though it has found many supporters, and has been extended to other metallic oxyds.

2. The constitution of a salt is necessarily determined conjointly by the acid character of the base and by the basic character of the acid. Thus a biacid base XO^2 must react with two equivalents of a unibasic acid, according to the general equation

$$XO^2 + 2\,YH = XY^2 + 2\,HO,$$

or with one equivalent of a bibasic acid, according to the equation

$$XO^2 + ZH^2 = XZ + 2\,HO.$$

3. Similarly a bibasic acid must react with two equivalents of a uniacid base, or with one equivalent of a biacid base. The same laws hold good for tribasic acids and for triacid bases, and we shall have obviously,

$$XO^3 + 3 . YH = XY^3 + 3\,HO.$$
$$XO^3 + ZH^3 = XZ + 3\,HO.$$

4. When, however, an m-basic acid is brought into contact with an n-acid base, the reaction is more complicated, and may result in a change in the character of the acid or base in the act of reaction. In

many cases, however, the reaction takes place in such a manner that the quantity of oxygen in the base outside of the radical is still equal to the quantity of hydrogen in the acid, expressed of course in equivalents. Thus for a triacid base and a bibasic acid we may have the equation

$$2\,.\,XO^3 + 3\,.\,YH^2 = X^2\,Y^3 + 6\,HO.$$

We shall return to this point further on.

5. As a polybasic acid may react with more than one oxyd, so as to form a salt containing two or more different metals or radicals, so a polyacid base may react with different acids forming salts of two or more chlorous radicals. In this point also there is a perfect analogy between acids and bases.

6. Finally, we may remark that a base containing one equivalent of oxygen outside of the radical is sometimes capable of eliminating two equivalents of water from an acid. Thus bisulphate of potash may be considered as formed according to the equation

$$KO + 2\,SO^3, HO - 2\,HO = KO, 2\,SO^3.$$

From this it must follow that a biacid base may eliminate *four* equivalents of water, and a triacid base *six*, so that we may have the reactions indicated by the equations

$$XO^2 + 4\,SO^3, HO - 4\,HO = XO^2, 4\,SO^3.$$
$$XO^3 + 6\,SO^3, HO - 6\,HO = XO^3, 6\,SO^3.$$

7. In the case of terchromate of potash we have a base containing one equivalent of oxygen eliminating three equivalents of water, according to the equation

$$KO + 3\,Cr\,O^3, HO - 3\,HO = KO, 3\,Cr\,O^3.$$

It is therefore conceivable that under certain circumstances a biacid base may eliminate *six*, and a triacid base *nine*, equivalents of water, since we should have by analogy

$$XO^2 + 6\,SO^3, HO - 9\,HO = XO^2, 6\,SO^3.$$
$$XO^3 + 9\,SO^3, HO - 9\,HO = XO^3, 9\,SO^3.$$

I have taken sulphuric acid in these formulas merely for the sake of convenience of representation.

8. A memoir on the ammonia-cobalt bases read before this Association at its second Albany meeting in August, 1856, contained a description

of four bases, of which two are biacid and two triacid. In this memoir it was shown that the biacid base Purpureocobalt unites with two or with four equivalents of acid, so that we have the compounds

$$5\,NH^3 . Co^2O . O^2, 2\,C^2O^3,$$
$$5\,NH^3 . Co^2O . O^2, 4\,SO^3,$$

and, furthermore, that in the salts of this base there may be two different acids in accordance with the canon laid down above. Thus we have

$$5\,NH^3 . Co^2O . O^2 \left\} \begin{matrix} SO^3 \\ C^2O^3, \end{matrix} \right.$$

$$5\,NH^3 . Co^2O . O^2 \left\} \begin{matrix} 2\,SO^3 \\ 2\,C^2O^3. \end{matrix} \right.$$

I have since resumed the subject with the view of completing the theory of the polyacid bases, and have selected the chromates of the four bases alluded to as being for the most part easily prepared in a state of purity, and as containing an acid too feeble to change the character of the base. I shall reserve the description of these chromates for another occasion, and will here therefore only communicate the results which possess a theoretic interest. In the case of each base I have prepared and analyzed a chromate and a bichromate.

9. The neutral chromate of Luteocobalt is isomorphous with the sulphate, and has the formula,

$$6\,NH^3 . Co^2O^3, 3\,CrO^3 + 5\,HO.$$

The acid chromate (or, as I prefer to call it, the bichromate) is precipitated from the nitrate by a solution of bichromate of potash, and has the formula,

$$6\,NH^3 . Co^2O^3, 6\,CrO^3 + 5\,HO.$$

Both salts are finely crystallized and perfectly well defined. As the oxyd of Luteocobalt forms the best instance of a triacid base with which I am acquainted, it is interesting to remark that it forms an acid as well as a neutral salt in exact accordance with theory, the acid salt containing not less than *six* equivalents of chromic acid, and thus furnishing the first well-established instance of a perfectly definite and highly crystalline salt, containing more than three equivalents of an anhydrous acid. It will be remembered that in the memoir above alluded to, a double carbonate of water and Luteocobalt was described, which has the formula,

$$6\,NH^3 . Co^2O^3, 3\,CO^2 + HO, CO^2 + HO\,;$$

but the distinction between a salt of this description and the bichromate of Luteocobalt is sufficiently obvious.

As the oxyd of Xanthocobalt is a well-characterized biacid base, I have found, as might be expected, that it unites with both two and four equivalents of chromic acid, forming a chromate whose formula is,

$$NO^2 . 5\,NH^3 . Co^2O . O^2, 2\,CrO^3 + 3\,HO,$$

and a bichromate which has the formula

$$NO^2 . 5\,NH^3 . Co^2O . O^2, 4\,CrO^3 + 5\,HO.$$

The bichromate is highly crystalline, and easily prepared in a state of purity.

The analyses of the chromates of Roseocobalt and Purpureocobalt are not yet sufficiently complete to enable us to pronounce with certainty upon their constitution. This arises in the case of the salts of Roseocobalt from the difficulty of obtaining them in a state of purity. The examination of the chromates of Purpureocobalt, on the other hand, has led to conclusions which, though not unexpected, are of a very remarkable character.

When, for example, a solution of chloride of Purpureocobalt is poured into one of the neutral chromate of potash, a fine red precipitate is obtained which proves to contain chlorine, and which has the formula,

$$5\,NH^3 . Co^2Cl . O^2, 2\,CrO^3.$$

The bichromate appears in like manner to have the formula,

$$5\,NH^3 . Co^2Cl . O^2, 4\,CrO^3\,;$$

nevertheless, I have not been able to obtain this salt perfectly free from another bichromate, which has probably the formula,

$$5\,NH^3 . Co^2O . O^2, 4\,CrO^3.$$

From this it appears that there are in fact two Purpureocobalt bases having respectively the formulas,

$$5\,NH^3 . Co^2O . O^2 \text{ and } 5\,NH^3 . Co^2Cl . O^2.$$

These may be distinguished as oxypurpureocobalt and chloropurpureocobalt. The ordinary chloride is the chloride of the chlorinated base, and the platinum salt analyzed by Genth and myself is a salt of the same radical. The other salts which we have described are salts of the oxygen base.

Reasoning from analogy, we may expect to find hereafter a species of Xanthocobalt containing an atom of chlorine in the radical and biacid, so that we shall then have the bases,

$$NO^2 . 5\,NH^3 . Co^2\,O . O^2 \text{ and } NO^2 . 5\,NH^3 . Co^2\,Cl . O^2.$$

I have already obtained some evidence of the existence of this second species of Xanthocobalt, having discovered both an oxalate and a chromate which contain chlorine.

I may here, for the sake of completeness, recall the formula of Gros' platinum base, which also contains chlorine in the radical, and the constitution of which is represented by

$$2\,NH^3 . Pt\,Cl . O.$$

The views which I have brought forward in reference to the polyacid bases may be deduced with the greatest simplicity and facility from the theory of types. Adopting this theory, we may define acids as a class of bodies belonging to the type of two or more equivalents of water or of chloride of hydrogen, in which one or more equivalents of hydrogen are replaceable by other radicals. I may here remark, that I consider the true equivalent of free muriatic acid to be represented by the formula,

$$\left.\begin{matrix}H\\H\end{matrix}\right\}Cl^2,$$

that of water in the free state being

$$\left.\begin{matrix}H\\H\end{matrix}\right\}O^2,$$

while at the same time I admit that both water and muriatic acid may enter into combination as HO and HCl, or in other words with half the equivalents which they have in the free state. Upon this view the formulas of hydrate of potash and fluohydrate of fluoride of potassium are exactly analogous, being, namely,

$$\left.\begin{matrix}K\\H\end{matrix}\right\}O^2 \text{ and } \left.\begin{matrix}K\\H\end{matrix}\right\}F^2;$$

each containing an equivalent of replaceable hydrogen, and each therefore unibasic, while their primitives, water and fluohydric acid, are bibasic. In like manner I consider the so-called double fluoride of antimony and potassium to belong to the type of six equivalents of fluohydric acid, as indicated by the formula,

$$\left.\begin{matrix}\text{Sb}\\ \text{K}^3\end{matrix}\right\}\text{F}^6.$$

The application of this view to the very numerous double chlorides, iodides, bromides, and fluorides, is too obvious to require further illustration. I may, however, remark here that it appears to me more strictly in accordance with observed facts than the theory which refers all these bodies to the type of two or more equivalents of hydrogen, and which by so doing ignores entirely the difference between the chemical characters of chlorine and hydrogen.

Taking the definition of an acid above given as our starting point, we may define a base as a body belonging to the type of two or more equivalents of water, or of chloride of hydrogen, in which one or more equivalents of oxygen or chlorine are capable of reacting with an equal number of equivalents of hydrogen in an acid, so as to form a new acid.

Thus in the reaction between the basic oxyd of silver and chlorhydric acid, we have the equation,

$$\left.\begin{matrix}\text{Ag}\\ \text{Ag}\end{matrix}\right\}\text{O}^2+\left.\begin{matrix}\text{H}\\ \text{H}\end{matrix}\right\}\text{Cl}^2=\left.\begin{matrix}\text{Ag}\\ \text{Ag}\end{matrix}\right\}\text{Cl}^2+\left.\begin{matrix}\text{H}\\ \text{H}\end{matrix}\right\}\text{O}^2,$$

in which water is the new acid formed.

It is easy to see that acids and bases are only particular species of salts, which we distinguish by separate names for convenience, and not from necessity. The character of replaceability (if I may use the word) is not confined to hydrogen, but only exists in this element in a higher degree than in any other. According to the definitions which I have given, the polybasic or polyacid character of acids and bases depends simply upon the number of equivalents of active oxygen or chlorine or of replaceable hydrogen which they contain.

The salts of the ammonia-cobalt bases are easily reduced to the types of water and chloride of hydrogen, provided that we consider the equivalent of oxygen as eight and not as sixteen, as is done by most chemists of the new school. The adoption of the equivalent sixteen complicates almost all the formulas, by rendering it necessary to double them in order to arrive at rational expressions.

In a report on the progress of organic chemistry read before the Association at its Providence meeting, I have alluded slightly to the theory of water types, and have attributed it to Gerhardt and Williamson. I at that time ascribed but little importance to the theory in question, and as I did not consider it to have exercised a sensible influence upon the progress of the science, I did not examine its history minutely. In this manner I find that I have not done justice to the claims of our associate, Mr. T. Sterry Hunt, to whom is exclusively due the credit of having first applied the theory to the so-called oxygen acids, and to the anhydrids, and in whose earlier papers may be found the germs of most of the ideas on classification which are usually attributed to Gerhardt and his disciples. The acknowledgment is due to myself, and is, I think, appropriate to the present occasion.

12. PRELIMINARY NOTICE OF NEW BASES CONTAINING METALS ASSOCIATED WITH AMMONIA. By Prof. WOLCOTT GIBBS, of New York.

IN the joint memoir of Genth and myself on the ammonia-cobalt bases, it was stated that Xanthocobalt is not the only product of the action of nitrous acid vapors upon ammoniacal solutions of cobalt or of the salts of Roseo and Purpureocobalt. I ventured some years since, in my report on the recent progress of organic chemistry, to suggest the probable existence of a similar base containing *two* equivalents of deutoxyd of nitrogen and five of ammonia, as couplets of the sesquioxyd of cobalt. Recent investigations have fully confirmed this view, and I propose for the new base the name of Flavocobalt. The salts of Flavocobalt are occasionally formed in small quantities during the action of nitrous acid vapors upon ammoniacal solutions of chloride or nitrate of cobalt. It is, however, difficult in this manner to obtain them in a state of purity, as other nitrous products are always formed at the same time.

A much better method consists in boiling solutions of sulphate or nitrate of Roseocobalt with nitrite of potash. When sulphate of Roseocobalt is employed, the proportions to be used should be two parts of the sulphate to three of the nitrite. The solution should be previously acidulated with sulphuric acid, and then boiled for half an hour or longer in a flask. Care must be taken to keep the liquid acid by occasionally adding a little sulphuric acid. If this be not done, much ammonia is given off and partial decomposition ensues. During the boiling, a large quantity of colorless gas is given off. The liquid gradually becomes orange-red, and often deposits a deep orange-red crystalline salt. On cooling the new sulphate separates in small but very distinct and brilliant crystals, which may be purified by re-solution and crystallization. The mother liquor contains much sulphate of ammonia and sulphate of cobalt. The sulphate of Flavocobalt forms the most convenient starting-point in the description of the salts of this base. The chloride, nitrate, &c., are easily obtained from it by double decomposition. The salt itself is remarkably stable and well defined. Its color is deep orange. It is nearly insoluble in cold water, but hot water dissolves it, giving an orange colored solution, from which the salt crystallizes on cooling in very brilliant small crystals. Like most of the salts of the ammonia-cobalt bases it is dichrous by reflection, the ordinary image being rose-red, while the extraordinary image is bright orange. The salt has the formula

$$2\,NO^2.\,5\,NH^3.\,Co^2O^3,\,2\,SO^3 + 2\,HO.$$

The reactions of this salt are in some respects similar to those of the salts of Xanthocobalt, but are sufficiently distinctive. They are as follows: —

Alkalies give no precipitate.

Ferrocyanide of potassium gives a yellow precipitate which soon becomes dirty orange yellow.

Ferricyanide of potassium after a short time fine orange-red needles.

Chromate of potash gives a fine yellow precipitate.

Bichromate of potash gives orange needles.

Bichloride of platinum gives a beautiful orange colored crystalline precipitate.

Picrate of ammonia gives a beautiful yellow precipitate.

Oxalate of ammonia, after long standing, gives fine brown yellow prismatic crystals.

The action of nitrite of potash upon solutions of chloride and nitrate of Roseocobalt is much less simple. In the former case a large quantity of Fischer's salt is formed, with a much smaller proportion of a salt which is probably the chloride of Flavocobalt. In the latter, nitrate of Xanthocobalt is one of the products of the action, together with large quantities of Fischer's salt and of a new salt in brown-red prismatic crystals, which appears also to be a double nitrite.

When a solution of the sesquichlorid of ruthenium is treated with an excess of ammonia and a current of nitrous acid gas passed into the solution, the liquid undergoes various changes of color, and finally becomes orange-yellow. The liquid is to be evaporated to dryness upon a water-bath, and the residuum washed with cold water to remove sal-ammoniac. A brick-red mass is obtained, which on re-solution in hot water yields crystals of the chloride of a new base containing ruthenium. These crystals, according to Prof. Dana's measurement, are regular octahedrons. They have a brown-yellow or sherry wine color, and are but slightly soluble in cold water. The solution gives a fine red precipitate with oxalate of ammonia, and yields well characterized salts with other re-agents.

When a solution of the double sesquichloride of iridium and ammonium is treated with an excess of ammonia and allowed to stand for some weeks in a warm place, the liquid becomes, as Claus has stated, pink or rose-red, and contains the chloride of an ammonia-iridium base, which has the formula $5\ NH^3.\ Ir^2Cl^3$, and which is therefore analogous to the chloride of Roseocobalt. If the liquid be evaporated to dryness on a water-bath, the sal-ammoniac may be removed by washing with cold water. The residue, dissolved in much boiling water, deposits on cooling a small quantity of the chloride of Claus' base in fawn-colored scales, which require a very large quantity of boiling water for solution. The mother liquor yields on evaporation beautiful pale yellowish crystals, apparently regular octahedra. These crystals are the chloride of a new iridium base. They are nearly insoluble in cold, and not very soluble in hot, water: the solution may be boiled without decomposition even after addition of a solution of caustic potash. The reactions of the solution are as follows: —

Bichloride of platinum gives a beautiful orange crystalline precipitate.

Chromate of potash gives a lemon-yellow precipitate.

Bichromate of potash gives a fine orange precipitate.

Oxalate of ammonia gives, after a short time, beautiful colorless crystals.

Picrate of ammonia gives lemon-yellow feathery crystals.

Ferrocyanide of potassium gives, after standing, colorless crystals.

Ferricyanide of potassium gives no precipitate.

These reactions are sufficient to establish the basic character of the new compounds. A more detailed description with analyses must be reserved for another occasion.

I am indebted to the kindness of Dr. Torrey and Prof. Bache for a liberal supply of material derived exclusively from the California gold. The iridosmium from California contains an unusually large proportion of iron, and, I am disposed to think, a larger percentage of ruthenium than the Siberian ores. Different samples, however, vary greatly in composition. As large quantities of the mineral have been offered for sale in New York, it is to be hoped that the study of the platinum metals will occupy the attention of American chemists, and that useful applications will soon create a demand which must lead to an abundant supply.

V. METEOROLOGY.

1. On the Currents of the Atmosphere. By Prof. James H. Coffin, La Fayette College, Pa.

It may be recollected by those members who were present at our first meeting in Philadelphia in 1848, that, in a report on the Winds of the Northern Hemisphere, compiled in pursuance of a previous appointment by the American Association of Geologists and Naturalists, and read at that meeting, after pointing out the mean line of division between the trade-wind system and the system of westerly winds, that prevails further to the north, I alluded to an anomaly that I had noticed in the former system, near its northern limit, and which I illustrated by diagram; namely, that the mean direction of the wind was not from the *north-east*, as we should naturally expect it to be, and as we find it further to the south, but from the *south-east.* I also invited the attention of the Association to indications that I pointed out, and illustrated by diagram, of a third distinct system of winds, never before recognized, so far as I know; namely, a current from the polar regions, which gradually turns toward the west, something after the manner of the trade-winds, and breaks up or disappears at about latitude 56° or 57° on this continent, and at about latitude 64° to 67° in Europe.

Since that time, through facilities afforded by the Smithsonian Institution and elsewhere, much additional matter has been examined, all tending to confirm the truth of the views then presented, in regard to the general movements of the atmosphere, and seeming to establish it upon an unquestionable basis. But while the facts remain the same, I have been led to modify my views on a single point, in my interpretation of them.

In that report, as also in a more extended memoir on the subject, since published by the Smithsonian Institution, I drew the line of division between the trade-wind system and the system contiguous to it on the north, upon the principle of assigning the winds from the easterly points of the compass to the former, and those from westerly points to the latter. According to such a mode of division, the south-east winds, near the limits, belong to the trade-wind system, as I placed them; but I am now inclined to think that this is a mistake, and that they constitute the incipient movement of a current, which, as it proceeds further north, is developed into the system of westerly winds. If this is so, the limit between the two systems should be drawn a little further south. The following diagram will illustrate my idea, in which the dotted line represents the limit as formerly drawn.

In the report referred to, I confined myself entirely to the facts developed by a rigid mathematical resolution of those atmospheric currents that are within the reach of observation, and did not touch at all upon any theory or mode of accounting for them. Still it was impossible, in prosecuting the investigation, not to inquire how these currents originate, whence they are supplied, and what becomes of them, when they reach the lines where they appear to break up and be lost. In my published volume I ventured to suggest such solutions of these questions as at that time seemed to me most probable, and as a mode of disposing of the current that comes down from high northern latitudes. I inquired whether it might not be possible, that, notwith-

FIG. 1.—*Horizontal Movement of the Atmosphere.*

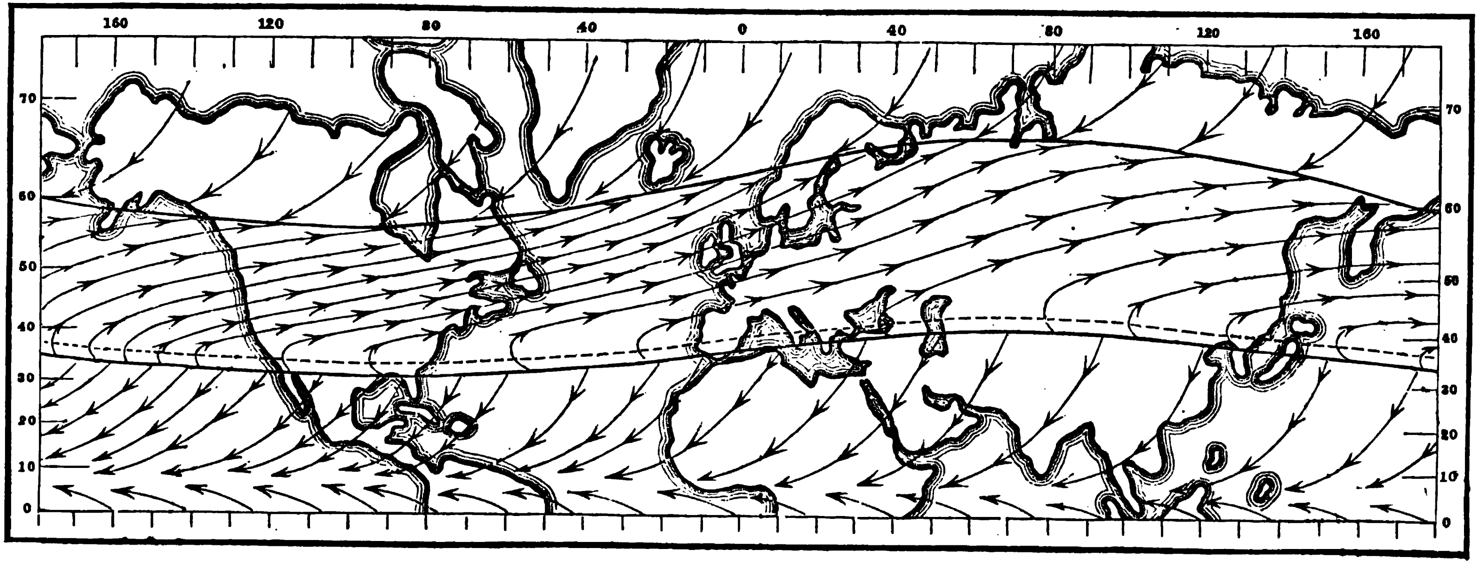
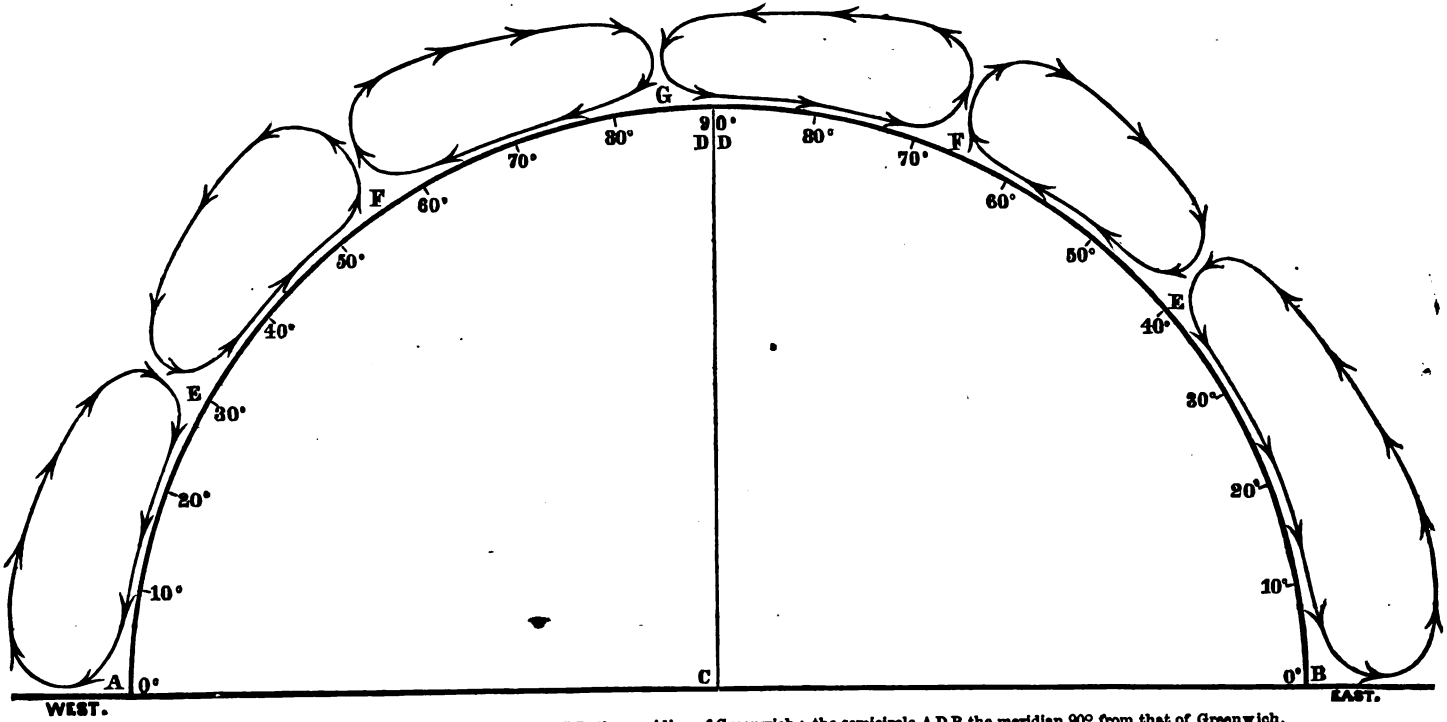


Fig. 2. — *Vertical Movement of the Atmosphere in the Plane of the 90th Meridian from Greenwich.*



The line A B represents the plane of the Equator; the line C D the meridian of Greenwich; the semicircle A D B the meridian 90° from that of Greenwich.

standing observations seemed to indicate that in the zone of westerly winds the main current, while nearly parallel to the zone itself in its different parts, seemed to incline rather toward its northern than its southern limits, still, owing to the greater density of the cold air from the north, the actual transfer of air across the zone might not be more southerly than northerly. But the change I now propose in the limits of the systems will render this view untenable. For if the wind sets inward towards the zone of westerly winds, all along its southern limit, there can obviously be no egress for it over the same line in the contrary direction. I am therefore shut up to the conclusion of Prof. Espy, that there is an upward movement of the air near the 60th parallel of latitude, — a movement rendered probable, if not certain, from the nature of elastic fluids, since it is an ascertained fact, that a belt of low barometer exists there, indicating a less pressure of the atmosphere, and consequently the air must rise by expansion. The general theory is as follows: —

Observation shows that there are three belts around the globe, where the mean pressure of the atmosphere is less than the average for the entire surface, — one near the 60th parallel of latitude north and south, and one near the equator, while in the polar regions, and also near the 30th parallel of latitude, it is greater than the average. From the known laws of fluids the air must flow from regions of greater pressure toward those where the pressure is less; and such a motion, combined with that due to the effect of the earth's rotation on its axis, affords a satisfactory solution of all the phenomena of a general character observed in the winds. Thus near the parallel of 30° north latitude, the current of air, which would otherwise flow toward the west or west-south-west, is split asunder by the superincumbent pressure, one branch taking a south-westerly and the other a north-westerly direction. In consequence of the earth's rotation, the former, crossing parallels of latitude whose easterly motion is continually increasing, will change its direction relatively, so as to blow more and more toward the west, and become the trade-winds; while the latter, which in my report I regarded as anomalous, but which now appear to blow as they should do, will, from a like cause, though with a contrary effect, since the parallels which it successively crosses diminish in velocity, turn easterly (i.e. from the west), and thus be developed into our system of westerly and south-westerly winds. In like manner, the greater pressure in the polar regions forces the inferior strata toward lower latitudes, where, as in the trade-winds, and for a like reason, the current turns toward the west. The expansion under the belt of low pressure near latitude 60°, as already remarked, will dispose of all the surplus air that blows inward there from both sides.

The existence of these belts of high and low barometer has been satisfactorily accounted for by Prof. Espy, and if more extended and carefully collated observations shall show that they are not parallels of latitude, but that they coincide with the lines that define the different systems of winds, the soundness of the theory I have here advocated will be established beyond any reasonable doubt.

The following diagram will serve to illustrate what I conceive to be the vertical movement of the atmosphere, in the plane of the 90th meridian east and west from Greenwich; A B representing the plane of the equator, C D that of the meridian of Greenwich, — A D and B D meridians 90° from Greenwich, — E and E the intersections of these meridians with the southern boundary of the zone of westerly winds, F and F their intersections with the northern boundary of the same zone, and G the region where the polar system of winds originates.

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2. WINDS AND RAINS OF CALIFORNIA. By H. G. WARNER, of Rochester, N. Y.

THE alternate dry and wet seasons of California, especially along the sea-coast, while they are so well known as to need no evidence of their constant recurrence, and nearly uniform continuance, have not always elicited the same reasons to account for them. The climatological theories that are generally acknowledged in explaining similar phenomena in other localities, seem there to be insufficient, or rather, the facts connected with these alternating seasons will not support, in that section of our hemisphere, the general theory respecting the course of winds in other regions of the same latitude, and the results that usually follow.

The *dry season* generally commences some time in May, and continues about six months, though the commencement and termination of this period are not quite uniform or well defined. The other half of the year is called the *wet season*, because, during that whole period occasional rains are experienced, and sometimes heavy and long storms, with probably not more wet days than occur in the vicinity of New York in the months of spring.

However it may be, that the vapor, taken up by the south-east trades, is borne to the north, and finally precipitated in the form of rain upon the California coast, to thirty-five and forty degrees of north latitude; the other branch of the theory, as laid down by the superintendent of the National Observatory, in his "Physical Geography of the Sea" (sec. 189, ed. 1856), that "it descends and appears on the surface *with* the *south-west winds of those latitudes*," cannot readily command assent, when we consider well-authenticated facts. That author is not alone in the maintenance of such a position. The same statement is made by others, whose authority is also entitled to high consideration. The theory is plausible, but do the facts warrant the conclusion?

The winds on the Pacific, in those latitudes, are undoubtedly most prevalent from the westward, varying from north-west to south-west, for a period of six or seven months, or during the dry season, and, according to the results of a long series of observations made by the direction of the Superintendent of the United States Coast Survey, they rarely travel out of that range, at any season of the year; that is, in the neighborhood of San Francisco, which is situated between the parallels of thirty-seven and thirty-eight degrees north.

Two facts may be stated that conflict with the above theory of rains, and the declared result of observations just mentioned as to the *course* of the winds. 1. It is admitted that the winds in summer, or the dry season, are as represented; but when the rains occur, they come, almost uniformly, from nearly a *south-east* direction. This is the concurrent testimony of all travellers who have spent a winter at San Francisco. It is so laid down by writers, and entirely corresponds with my own observation, having spent in San Francisco a portion of the wet season of 1849–50, all of 1850–51 and 1851–52, and a considerable part of 1855–56. The most violent winds occur in the wet season, and those that do most injury to the shipping in the harbor of San Francisco are south-easterly, up the bay.

2. The rains never fall when *south-west* or *westerly* winds prevail. If these rains are borne upon "the *south-west* winds," they certainly do not appear with the surface winds in that direction.

In Blodgett's Climatology, page 194, that author remarks, in speaking of the periodic rains on the Pacific coast: "This periodicity appears in the phenomena attending the rains, the nearly constant *south-east* winds, &c." Again, on the same page, "The rains of this best known portion of the Pacific coast are, as has been said, peculiar in regard to the attending winds, which, from San Diego to Puget's Sound, are, in nearly all cases, from the *south-east* and *south*, with a strong and steady force."

Dr. Gibbons, on whose observations upon the winds and rains at San Francisco much reliance is placed, reports the whole number of rains, at San Francisco, from December, 1850, to March, 1852, including two winters, to be seventy-nine; of which forty-four were from the south and south-east; and he states, that "the most rainy point is in a direct line with the southern coast, or about south south-east."

It may be well here to call attention to the fact, that this coast runs nearly in a north-west and south-east direction from thirty-five to forty degrees north latitude; so that if the course of rain winds be parallel with the line of the coast, they must come from the south-east. The same authority proves, that, during the three winter months of 1851, the mean percentage of sea winds was forty-eight to that of fifty-two of land winds, showing that they came oftener from the land or south-east than the rains appeared, for the whole number of rains in sixteen months, including one entire wet season, and three months of another, was seventy-nine.

More testimony, to prove that the rain winds are from the south-east nearly, is not wanting. The author of "What I saw in California," Edwin Bryant, Esq., well known to Californians of 1846 to 1852, in speaking of the climate of that country, especially in the neighborhood of San Francisco, remarks at page 326: "In the winter months the wind blows in soft and gentle breezes from the *south-east*, and the temperature is agreeable, the thermometer rarely sinking below fifty degrees. *When the winds blow from the ocean it never rains;* when they blow from the land, as they do during the winter and spring months, the weather is showery, and resembles that of the month of May in the same latitude on the Atlantic coast."

It is beyond contradiction, that when the rains prevail upon the coast of California, and during the wet season, so called, the winds come from the south-east. In whatever direction the clouds may pass, Dr. Gibbons says, "the rainy point is in a direct line with the southern coast." And here it may be proper to remark, that nearly all writers and observers on this subject agree, that the sea winds there are never found to blow from a point above the north-west or below the south-west; and it is worth noticing, that the most southerly wind thus mentioned is in a direction exactly perpendicular to the line of the coast.

It is stated by the Superintendent of the Coast Survey, that, in the winter, the principal direction of winds on that coast is from the south-west. A series of observations reported by him, taken under his direction at Fort Point, in the vicinity of San Francisco, is made the basis of an assumption by others, that these south-west winds alone bring the rains upon the coast; and this seems to accord with the generally accepted theory of Lieut. Maury, of the "circuit of the winds," and that the vapor taken up by the south-east trades, after having crossed the calm belt of Cancer, is carried to the north-east, and large quantities of it discharged upon that coast. But theories, to be valuable, must rest upon facts.

It appears that the winds prevailing along the coast of California, bringing the rains, come (if the evidence is to be credited) from the south-east; contradicting the position, that during the wet season they come from the south-west (ninety degrees asunder), and of necessity bring the rains. Let us see if this difference cannot be reconciled; and then offer some reasons why the theory is improbable, that south-west winds, if they exist, produce the rains in question.

The topography of Fort Point and the vicinity must be known in order to account for the error, if any there be, in the observations upon the winds above mentioned. The point of observation is sixty or eighty feet above the level of the ocean, and is situated on the south hinge of the Golden Gate, so called, which opens upon the easterly side of a small semicircular bay, indenting the coast several miles. Upon the southern curve of this bay, inland, the ground rises much higher than at Fort Point, and in some places more than four hundred feet above the sea. The highest elevation is believed to be Point Lobos, which is in a southern direction, perhaps three miles distant from Fort Point. Point Lobos is the commencement of a range of mountains running south-easterly on the peninsula that separates San Francisco Bay from the sea. On the north of the Golden Gate, and westerly to the coast, run high mountains, rising several thousand feet above the sea, — Point Boneta standing sharply out at the angle made with the line of the coast, and being a very high point. It seems quite probable that the south-easterly winds, blowing parallel with the coast, are deflected in the direction of Fort Point, by the particular conformation of the highlands upon the northern curve of this small bay, and that the wind, appearing to the observer to come so continually from the south-west during the winter and rainy season, was really no other than the south-east rain wind that blows from this "rainy point in a direct line with the southern coast."

It must be noticed further, that this ridge of highlands on the south of Fort Point, extends easterly to the city of San Francisco, about six miles, being there over three hundred feet high, and intervenes in such a manner as to cut off from that point the south-east winds, that sweep along the great bay towards the north-west. They could not be felt from that direction at Fort Point, and it is very easy to imagine how this "strong and steady current" of winds from the south-east could, in its uninterrupted course along the great bay, draw from seaward, through the Golden Gate, and around Point Lobos, a breeze, that in passing over Fort Point would come from the south-west.

That these are surface winds, observed at Fort Point, and not higher currents that sometimes are said to bring the rain, must be admitted, in order to be consistent with the theory, that in these latitudes, the winds blowing northwardly are of that species.

It is a well-established fact, that a current of that ocean, similar to the Gulf Stream of the Atlantic, is continually setting along the coast from the northward, and coming from high, cold latitudes. This current, according to Dr. Gibbons and others, has a temperature of 55°. That it rises much higher than this, is not claimed, though there is a slight variation, in different seasons amounting to perhaps two degrees.

The mean temperature of the atmosphere at San Francisco for three years and upwards, from Dec. 1849, was 57°.2. The average in the different seasons being, in spring, 57; summer, 60.1; autumn, 60.1; and winter, 51.5; and it will be readily perceived, that the difference between the temperature of the sea winds (about the same as the water) and the atmosphere on land is a mere trifle. If "the vapor that is taken up by the *south-east* trades," according to Lieut. Maury, "is borne along over the region of the *north-east* trades to latitude 35° or 40° north, where it descends and appears on the surface with the south-west winds of those latitudes," how can the statement be reconciled with the fact, that these south-west winds, blowing in winter, and supposed to bring the rains, are at the low temperature of 55° or 57°? They would appear to be incapable of bearing such an amount of vapor as "is condensed and precipitated during this part of the year, almost in constant showers." The mean temperature on land falling not much below in the rainy season, it seems improbable that the rains occurring in those latitudes, on that coast, can be thus produced; and especially since in March for the years mentioned, that temperature was 54.5, and in April of the same years 53.6, — months in which there were considerable falls of rain, and during which period, according to Dr. Gibbons, much more rain fell in the spring months than any other, — while the temperature on land was as high at least as these south-western winds.

The author of "Physical Geography of the Sea" assumes, that, "in the winter and spring," the land in California is cooler than the sea air, and is quite cold enough to extract moisture from it." We think we have proved this an error, that is, as a general proposition.

Again, if these south-westerly winds bear along a part of the same vapor that is taken up by the south-east trades, they become surface winds at the calm belt of Cancer, and, long before they reach the coast of California, have been condensed to such a degree that they could not produce the phenomena of heavy rains that occur in that locality. There might occasionally be some "moisture extracted," but not the copious precipitation that is so often experienced.

Another inquiry here becomes pertinent as to the soundness of the position, that the southern trades and these south-west currents spoken of, take north-west and north-east directions in the different hemispheres, in consequence of the "diurnal rotation of the earth," as an adequate cause for the uniform course of those winds, and more particularly the south-west wind in question. It is not denied that the rotation of the earth on its axis towards the east, may cause some deviation of the winds and sea currents when "put in motion in a straight line towards the equator," or that this deviation would be to the left in the southern and to the right in the northern hemisphere. But assuming this to be the fact, it does not appear that the force thus exerted would cause any such deviation from a direct meridional line, as is sometimes supposed.

Suppose a particle of atmosphere, upon the theory of Lieut. Maury and others, to start from the calm belt of Capricorn for the equator. It will be seen, that, rejecting fractions, this thirtieth parallel is 2,000 miles from the equator. The velocity of any point upon the equator is about 1,000 miles per hour, and on the thirtieth parallel it will be sufficient for the illustration to rate it at 800 miles — 200 or one fifth less than at the equator. Now, if it be true that this difference in velocity is equivalent, by the effect of *vis inertiæ*, to the force contended, propelling the particle westward, or to the left of a direct line, it would strike the equator as far west of the meridian whence it started as the difference between the velocity of the earth at the equator and that of the latitude from which it came, and instead of striking the equator 2,000 miles west of the original meridian of starting — which would be required to make it a south-east wind — it would fall upon it 400 miles west, making the variation only one fifth of what the theory would seem to require, and so little as scarcely to be noticed — nine degrees, instead of forty-five — the actual course of the wind. It seems, then, that some other cause must be sought for this great deviation, and it may or may not be the same that will account for the south-west winds on the California coast, and the west and north-west currents that prevail there in the dry season. The same reasoning would seem applicable to the Gulf Stream, in what is claimed to affect its current, and the drift-wood on the south of it, as well as to all currents of water and wind. But the effect of this motion of the earth would be greater upon the winds at a given point north, in winter than in summer, and the direction of the winds would be more westerly — which is not the fact — because the whole system of trades and calms is then removed further south, and the difference proportionally greater between the velocity of the earth at the starting point and that of observation.

If the winds on the Pacific, then, do not necessarily come from the south-west, in obedience to this law of motion, we should seek a reason why these west and north-west winds that are periodical and so uniform on the coast at another season — and certainly not caused to blow in that direction by this motion — cease to blow, and should endeavor to account for the changes and the causes that produce the rains of winter and the droughts of summer upon some other hypothesis.

These westerly winds are all admitted to have an exceedingly low temperature, on account of the cold ocean current over which they pass, and to carry but little vapor. While they continue, the mean temperature on land in summer is above theirs, though it often sinks below at evening, and then the west winds cease to blow. At mid-day, when these winds are strongest, the atmosphere is heated and rapidly rarified and elevated, and the colder, heavier air from the ocean rushes in to supply the vacuum thus created. The mists that are sometimes produced, accompanying this cold westerly wind, are the effect of sudden contact of this denser current with the light, dry atmosphere on land; but no rain is ever the result of this admixture, because the vapor-bearing current takes an increased temperature.

When the sun retires to the south, and the whole system of calm-belts and trades moves off, the temperature of the Pacific coast for a great distance is reduced to about the same degree, or a fraction lower, than the waters of the sea. This appears from the authorities above quoted. The same causes, then, no longer exist to induce a westerly current of winds landward. They cease to blow in such steady currents.

But we have seen that the motion of the earth to the east cannot produce a south-west wind upon the coast, and it would appear quite improbable, that, by any acknowledged law governing currents of the atmosphere, a wind could long prevail upon the coast in those latitudes, directly perpendicular to it, continually producing rain. Whence, then, come the rains that constitute the wet seasons in California?

It is not, perhaps, material to fix upon the point or the chief localities at which the vapor is taken up that is finally precipitated on that coast; but by inspection of the map, it can easily be traced to a source that might be generally admitted as the true one. The southern trades along the western coast of South America come loaded with vapor, and in their passage to the north they cannot be robbed of much of it before they pass beyond the tropic of Cancer, where they are supposed to become, a second time, surface winds. It will be seen, that near this point on the Pacific coast is the entrance to the great Gulf of California, which at all seasons has a very high temperature, and is completely guarded from the westerly winds of the Pacific by a lofty coast range of mountains extending the entire length of the peninsula not only, but up to the northern line of California. This surface wind, whatever may be its course when striking the mouth of the gulf, must be deflected in the direction of the line of that body of water, and continue to blow fifteen hundred miles over it, holding nearly the same temperature till it reaches the head of the gulf at about thirty-three degrees north. This course is exactly parallel with the line of the coast, and in the direction of San Francisco Bay, whence the current passes, without interruption, up the valley, guided on one side by the continued coast range, and on the other by the immensely lofty Sierras Nevadas — the direction of the wind being dependent, as Mr. Blodgett well says, "on the trend of the coast, and the mountain ranges near it."

As the temperature further north begins to sink in autumn, this current is condensed in the higher latitudes, and the rainy season there first commences. It continues and moves to the south, as the sun recedes, and is experienced during a part of the autumn, sometimes, and through the winter and spring, with more or less rain, until, by the return of the sun, the temperature of the atmosphere, in the latitudes where these periodic wet seasons are mostly known, becomes again raised to such a degree as to induce a change in the current of winds from the ocean towards the land, when the rains cease.

B. NATURAL HISTORY.

I. GEOLOGY, PALEONTOLOGY, AND GEOGRAPHY.

1. ROCKS OF KANSAS. By G. C. SWALLOW, of Columbia, Missouri.

AT the last meeting of this Association at Montreal, I had the honor of presenting a Geological Map of Kansas, the eastern part of which was colored according to the facts derived from my own observations; but the remainder and much the larger portion was made out according to data derived from Major Hawn. Since then Major Hawn has had opportunities of making more thorough examinations of the places before observed, and of extending his researches into more distant localities.

His notes and fossils show the existence in that territory of the following formations: —

No. 1. — 200 feet *Quaternary.*
No. 2. — 75 feet *Cretaceous.*
No. 3. — 420 feet *Triassic.*
No. 4. — 820 feet *Permian.*
No. 5. — 1070 feet *Coal Measures.*
No. 6. — 170 feet *Mountain Limestone.*

The Quaternary deposits are similar to those rocks in Missouri, and the cretaceous afford no new features. The Triassic * is a very interesting group of Sandstones, Limestones, Clays, Marls, and Gypsum. But few imperfect fossils have been observed in these strata. They resemble both the Triassic and Liassic forms. The Upper Permian is made up of nearly the same materials as the doubtful beds above. They abound in fossils of well-known Permian types belonging to the genera *Monotis*, *Schizodus*, *Bakerellia*, *Synocladia*, *Thamniscus*, &c., some twenty species. But one known Carboniferous fossil was observed — the *Terebratula subtilita.* The *Lower Permian* contains fifty-seven species of Permian fossils and fifteen Carboniferous. Although the Permian species are so much the more numerous, the individuals of the Carboniferous types are much more abundant, about as ten to one to the Permian forms.

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* By this name we mean simply to indicate that these beds are related to the Trias. They are between the Permian and the Cretaceous.

Among these Permian fossils I have identified fifteen European species, and described thirty-four new ones. Mr. Meek has described ten.

I am indebted to Major Hawn for the following section of the rocks of Kansas :—

SYSTEM I. — QUATERNARY.

No. 1 — 150 feet Bluff, the same as in Missouri — 2d Ann. Rep. Mo. Survey.
" 2 — 4 feet white clay — 2d Ann. Rep. of Mo. Survey.
" 3 — 15 feet local drift.

169 feet of Quaternary.

SYSTEM II. — CRETACEOUS.

No. 4 — 45 feet light gray crystalline limestone.
" 5 — 27 feet slope strewn with light gray calcareous concretions.

72 feet of Cretaceous.

SYSTEM III. — TRIASSIC. (?)

No. 6 — 12 feet light gray arenaceous limestone.
" 7 — 10 feet blue pyritiferous clay.
" 8 — 15 feet dark brown ferruginous sandstone.
" 9 — 8 feet, like No. 7.
" 10 — 18 feet flesh-colored quartzitic sandstone.
" 11 — 14 feet variegated, red and white, clay.
" 12 — 8½ feet white granular gypsum. (Local.)
" 13 — 12 feet, like Nos. 7 and 9.
" 14 — 20 feet dark brown ferruginous sandstone
" 15 — 13 feet variegated, white and red, clay.
" 16 — 50 feet soft, coarse, buff sandstone.
" 17 — 30 feet pyritiferous clay.
" 18 — 10 feet yellowish-brown argillaceous sandstone.
" 19 — 10 feet thin silico-calcareous strata, containing fragments of trees.
" 20 — 3 feet brown, impure lignite.

No. 21 — 10 feet black, pyritiferous clay, containing numerous stellated crystals of selenite.

" 22 — 60 feet gray, blue and brown clay, with thin seams of fibrous selenite, and flesh-colored nodules of gypsum.

" 23 — 75 feet soft, crumbling, brick-red sandstone.

" 24 — 17 feet white clay, with soft concretions of oxide of iron.

" 25 — 15 feet conglomerate of coarse sand and small brown pebbles.

410½ feet of Triassic. (?)

SYSTEM III. — PERMIAN.

UPPER PERMIAN.

No. 26 — 100 feet brown and yellow, cellular and brecciated limestone, alternating with brown, blue, and white pyritiferous clay, containing a bed of white granular gypsum, 5 feet thick.

" 27 — 18 feet conglomerate, of angular, water-worn fragments of limestone, cemented with white argillaceous matter. This bed is local, and may not have its true position.

" 28 — 15 feet, resembling No. 26, but more compact.

" 29 — 60 feet dark brown, silicious limestone, alternating with coarse, impure, brown clays, containing crystals of chalcedony and agatized quartz.

" 30 — 25 feet dark buff, compact limestone.

" 31 — 45 feet red clay.

LOWER PERMIAN.

No. 32 — 25 feet brown shale, containing geodes whose drusy cavities are filled with crystals of quartz, and reniform nodules of variegated white and red quartz, often agatized.

" 33 — 30 feet gray limestone and flint, with beds of brown clay.

" 34 — 25 feet massive, cherty, magnesian limestone and brown clay; the lower magnesian beds contain angular fragments of jasper.

" 35 — 20 feet of brown clay, fossiliferous.

" 36 — 18 feet red clay.

" 37 — 60 feet silicious, yellow, magnesian limestone, with heavy beds of brown clay.

" 38 — 10 feet massive bed of flint and limetone.

" 39 — 25 feet gray and yellow limestone, containing small globular and pear-shaped nodules of chert, and geodes with crystals, alternating with beds of brown and blue clay.

" 40 — 75 feet brown magnesian limestone, alternating with beds of brown, olive-green, and red clays.

" 41 — 25 feet light buff amygdaloidal-magnesian limestone and chert.

" 42 — 8 feet heavy-bedded yellow magnesian limestone.

" 43 — 3 feet blue fossiliferous slate.

" 44 — 15 feet brown slate.

No. 45 — 10 feet brilliant yellow, magnesian limestone.
" 46 — 17 feet dark brown limestone, with numerous joints of large *crinoidal* columns.
" 47 — 4 feet light yellow silicious limestone.
" 48 — 30 feet red clay.
" 49 — 8 feet silico-calcareous slate.
" 50 — 5 feet olive-green clay.
" 51 — 4 feet red clay.
" 52 — 10 feet dark gray limestone.
" 53 — 6 feet olive-green slate.
" 54 — 8 feet dark blue slate.
" 55 — 6 feet buff limestone.
" 56 — 16 feet brown slate.
" 57 — 3 feet dark blue slate.
" 58 — 13 feet gray clay.
" 59 — 3 feet compact, light buff limestone.
" 60 — 5 feet brown clay.
" 61 — 15 feet blue shale, fossiliferous.
" 62 — 3 feet compact, drab, silicious limestone, with small nodules of chert.
" 63 — 4 feet brown limestone.
" 64 — 3 feet soft, light-brown clay.
" 65 — 15 feet soft, olive-green clay, with a band of red clay one foot thick.
" 66 — 4 feet gray oolitic limestone.
" 67 — 4 feet bright olive-green clay.
" 68 — 6 feet dark buff oolitic limestone.
" 69 — 5 feet dark blue slate.
" 70 — 7 feet drab limestone.

Total, 820 feet of Permian Rocks.

SYSTEM IV. — CARBONIFEROUS.

1,073 feet coal-measures, a continuation of, and probably above, the Upper Coal Series of Missouri. (See 2d Mo. Rep. Part I. p. 78.)

NOTE. — The data for the above section were obtained amidst onerous duties connected with the lineal surveys of the territory, confining my observations to arbitrary lines and localities; this, together with a want of conformability in the strata near No. 26 of the foregoing section, renders some parts of it hypothetical, but I believe it is sufficiently accurate for general illustration. W. HAWN.

Catalogue of Fossils obtained by Major Hawn from the Permian Rocks of Kansas.

NAMES OF SPECIES.	Coal-Measures.	Lower Permian.	Upper Permian.	Triassic (?)	LOCALITIES.
PLANTÆ.					
A trilobate leaf of an unknown exogenous plant	. .	.	. .	*	No. 14 (?) of the foregoing section.
ZOOPHYTA.					
Stenopora crassa, *Lonsdale.*	. .	*	. .	. .	Valley of Cotton-wood.
Stenopora spinigera, *Lonsdale.*	. .	*	. .	. .	Rock Creek, Santa Fé road.
Chætetes, three species, (*undetermined*).	. .	* ?	. .	. .	Valley of Kansas, west of Fort Riley.
BRYOZOA.					
Fenestella flabellata (?), *Phillips.*	. .	*	. .	. .	Near Council Grove.
Synocladia virgulacea (?), *King.*	. .	*	. .	. .	Valley of Cotton-wood.
Thamniscus dubius, *Schlotheim.*	. .	*	. .	. .	Valley of Cotton-wood.
Acanthocladia anceps (?), *Schlotheim.*	. .	*	. .	. .	Near Hay's Ranch.
Phyllopora Ehrenbergi, *Geinitz.*	. .	*	. .	. .	Valley of Cotton-wood.
ECHINODERMATA.					
Archæocidaris Verneuilliana, *King.*	. .	*	. .	. .	Near Lost Spring, Santa Fé road.
Cyathocrinus ramosus (?), *King.*	. .	*	. .	. .	Near Council Grove.
ANNELLATA.					
Serpula (Spirorbis) valvata, *Goldfuss.*	. .	*	*	. .	Near Smoky-Hill Fork.
Spirorbis orbiculostoma, *Swallow.*	. .	*	. .	. .	Valley of Cotton-wood.
CRUSTACEA.					
Phillipsia, *species not determined.*	. .	*	. .	. .	Near Council Grove.
BRACHIOPODA.					
Productus Calhounianus, *Swallow.*	. .	*	. .	. .	Valley of Cotton-wood.
Productus semireticulatus, *Martin.*	*	*	. .	. .	Valley of Cotton-wood.
Productus Rogersii, *Norwood & Pratten.*	*	*	. .	. .	South Fork Cotton-wood.
Productus æquicostatus, *Shumard.*	*	*	. .	. .	Valley of Kansas, W. Ft. Riley.
Productus Norwoodii, *Swallow.*	*	. .	. .	. .	Valley of Cotton-wood.
Spirifer cameratus, *Morton.*	*	*	. .	. .	Road Creek, Santa Fé road.
Spirifer planoconvexa, *Shumard.*	. .	*	. .	. .	Valley of Cotton-wood.

Catalogue of Fossils obtained by Major Hawn from the Permian Rocks of Kansas.

Names of Species.	Coal-Measures.	Lower Permian.	Upper Permian.	Triassic (?).	Localities.
Chonetes Flemingii, *Norwood & Pratten.*	*	*	. .	. .	Near Council Grove.
Orthisina umbraculum, *Buch.*	*	*	. .	. .	Generally diffused.
Orthisina Shumardiana, *Swallow.*	. .	*	. .	. .	Red-water&Cotton-wood.
Orthisina Missouriensis, *Swallow.*	*	*	. .	. .	Red-water.
Rhynchonella Osagensis, *Swallow.*	*	*	. .	. .	Valley of Big Blue River.
Terebratula(?) subtilita, *Hall.*	*	*	. .	. .	Generally diffused.
ACEPHALA.					
Monotis speluncaria(?), *Schlotheim.*	. .	*	. .	. .	
Monotis speluncaria(?) var. Americana, *Swallow.*	. .	. .	*	. .	Near Smoky-hill Fork.
Monotis radialis, *Phillips.*	. .	. .	*	. .	Near Smoky-hill Fork.
Monotis variabilis, *Swallow.*	. .	. .	*	. .	Valley of Cotton-wood.
Monotis Halli, *Swallow.*	. .	*	*	. .	Valley of Cotton-wood.
Avicula gryphæta(?), *Munster.*	. .	. .	*	. .	Near Smoky-hill Fork.
Pecten Cleavelandicus, *Swallow.*	. .	*	. .	. .	Valley of Cotton-wood.
Pecten ringens, *Swallow.*	. .	*	. .	. .	Valley of Cotton-wood.
Pecten acutialatus, *Swallow.*	. .	*	. .	. .	Valley of Kansas.
Mytilus (Myalina) Permianus, *Swallow.*	. .	. .	*	. .	Valley of Kansas.
Mytilus (Myalina) concavus, *Swallow.*	. .	. .	*	. .	White Water.
Mytilus (Myalina) squamosus, *Sowerby.*	. .	*	. .	. .	Valley of Cotton-wood.
Mytilus (Myalina) rectus, *Shumard.*	*	*	. .	. .	Valley of Verdigris.
Myalina subquadrata, *Shumard.*	*	*	. .	. .	Valley of Verdigris.
Myalina Kansasensis, *Shumard.*	*	*	. .	. .	Near Council Grove.
Bakevellia antiqua (?) *Munster.*	. .	. .	*	. .	Valley of Kansas.
Bakevellia pulchra, *Swallow.*	. .	*	. .	. .	Valley of Kansas.
Edmondia gibbosa, *Swallow.*	. .	*	. .	. .	Valley of Cotton-wood.
Edmondia Otoensis, *Swallow.*	. .	*	. .	. .	Valley of Cotton-wood.
Edmondia semiorbiculata, *Swallow.*	. .	*	. .	. .	Council Grove.
Nucula (Leda) Kazanensis, *Verneuil.*	. .	*	. .	. .	Valley of Cotton-wood.
Nucala speciosa(?), *Munster.*	. .	. .	. .	*	No. 18 of foregoing section.
Nuculu (*species not determined*).					
Solemya Biarmica(?), *Verneuil.*	. .	*	. .	. .	Valley of Kansas.
Solemya (*species undetermined*).	. .	. .	*	. .	Smoky-hill Fork.
Solen(?) Permianus, *Swallow.*	. .	. .	*	. .	Smoky-hill Fork.
Cardiomorpha rhomboidea, *Swallow.*	. .	*	. .	. .	Gouncil Grove.
Cardiomorpha Kansasensis, *Swallow.*	. .	*	. .	. .	Valley of Cotton-wood.
Cardinia cordata, *Swallow.*	. .	*	. .	. .	Valley of Cotton-wood.
Cardinia subangulata, *Swallow.*	. .	*	. .	. .	Valley of Cotton-wood.
Cardinia (*species undetermined*).					
Cardinia Listeri(?), *Sowerby.*	. .	. .	. .	. .	
Pleurophorus(?) Permianus, *Swallow.*	. .	*	. .	. .	Smoky-hill Fork.
Schizodus obscurus, *Sowerby.*	. .	*	. .	. .	Valley of Cotton-wood.
Schizodus triangularis, *Swallow.*	. .	*	. .	. .	Valley of Cotton-wood.
Schizodus Rossicus (?) *Verneuil.*	. .	. .	*	. .	Smoky-hill Fork.
Lyriodon (Myophoria) orbiculare(?), *Goldfuss.*	. .	. .	. .	*	No. 18 foregoing section.

Catalogue of Fossils obtained by Major Hawn from the Permian Rocks of Kansas.

NAMES OF SPECIES.	Coal-Measures.	Lower Permian.	Upper Permian.	Triassic. (?)	LOCALITIES.
Allorisma lanceolata, *Swallow.*	. .	*	. .	. .	Near Council Grove.
Allorisma curta, *Swallow.*	. .	*	. .	. .	Near Council Grove.
Allorisma Minnahaha, *Swallow.*	*	*	. .	. .	Near Council Grove.
GASTEROPODA.					
Murchisonia subangulata(?), *Verneuil.*	. .	. .	*	. .	No. 26 foregoing section.
Murchisonia Kansasensis, *Swallow.*	. .	*	. .	. .	Valley of Cotton-wood.
Murchisonia perversa, *Swallow.*	. .	*	. .	. .	Valley of Cotton-wood.
Loxonema fasciata, *King.*	. .	*	. .	. .	Valley of Kansas.
Macrocheilus spiratus, *McCoy.*	. .	*	. .	. .	Valley of Kansas.
Naticopsis Pricei, *Shumard.*	*	*	. .	. .	Valley of Kansas.
CEPHALOPODA.					
Nautilus Permianus, *Swallow.*	. .	. .	*	. .	Smoky-hill Fork.
Nautilus occidentalis, *Swallow.*	. .	. .	. .	. .	Valley of Cotton-wood.
Orthoceras Kickapooense, *Swallow.*	. .	. .	*	. .	Smoky-hill Fork.
Cyrtoceras dorsatum, *Swallow.*	. .	. .	*	. .	Smoky-hill Fork.

The accompanying geological map of Kansas shows the Mountain Limestone covering a small area only in the south-eastern part of the territory. The Coal-Measures occupy a belt along the eastern end of the territory, extending westward as far as Fort Riley. West of the Coal-Measures the Permian strata are developed over a wide zone stretching across the territory from north to south; while beyond these are the doubtful beds (Triassic?) which overlie the Permian on the east, and dip beneath the Cretaceous to the west and north.

In an economical point of view these Permian beds are very important. The gypsum they contain will enable the farmer to convert the vast sandy plains of central Kansas into the most productive regions of the West, and fill the wilderness with a teeming, happy people. It will also supply the commercial demands of the Mississippi valley, which are now met by importation.

Since my first announcement of the discovery of Permian rocks in Kansas, in the March (1858) number of the Journal of Science, several communications have appeared in rapid succession, disclosing the existence of that formation in various parts of the West.

My paper declaring the discovery of several well-known Permian fossils in the valley of the Cotton-wood, in Kansas, was read in the St. Louis Academy of Sciences, on the 22d of February.

On the 2d of March, Messrs. Meek and Hayden announced the discovery of Permian fossils, by Major Hawn, on the Smoky-hill Fork in Kansas, and by Dr. Cooper, near Helena, in the same territory, in the Albany Institute. On the same day their announcement was read in the Academy of Natural Sciences at Philadelphia.

On the 8th of March, Dr. B. F. Shumard announced the discovery of Permian rocks by Dr. G. G. Shumard in the Guadaloupe Mountains, in New Mexico.

Dr. Norwood's letter showing the existence of Permian rocks in Bureau, La Salle, and Henry counties in Illinois, was read in the St. Louis Academy, on the 5th of April.

It is but just to state in this connection, that, so far as I know, Mr. Meek first suspected the Permian character of the Kansas fossils, and communicated to Major Hawn his impressions on the 3d of September, 1857. He also mentioned his discovery to some friends at the Smithsonian Institute, on the 17th of January, 1858, and communicated the same to Prof. Leidy, on the 16th of March.

Major Hawn frankly declares, that his first impression that the rocks in question might be Permian, is due to Mr. Meek.

Such are the facts as I understand them. If I am wrong, let the correction now be made, while all interested are present or represented by personal friends.

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2. On the Occurrence of Cobalt and Nickel, in Gaston County, North Carolina. By Prof. Henry Wurtz, of Washington, D. C.

In an exploration of a large tract of land in Gaston and Lincoln counties, N. C., during the last summer, I found indications of the existence of ores of cobalt and nickel in important quantity throughout a considerable extent of country.

The region explored comprised a range of rocks composed of alternating strata of talcose and quartzose schists, which crosses the south fork of the Catawba river, a little south of the line between Lincoln and Gaston counties, and just above the falls known as the "High Shoals of the Catawba." The general direction of this range, which forms a well-defined metalliferous belt of many miles in longitudinal extent, is about N. 20° E. (varying, however, in places between due N., and N. 35° E.), and at the High Shoals it is three or four miles wide. It is everywhere traversed by "veins" of quartz, carrying pyrites or other sulphids, and showing at the surface the limonite *gozzans* derived from their oxydation.

These "veins" in some places have all varieties of strike and dip, although the most important ones appear to *conform in strike*, or very nearly so, to the containing rocks themselves. The dip of these rocks is nearly vertical, usually a little westerly, although considerable local variations of dip and contortions were observed in a few places. At the place where this metalliferous belt crosses the river, its boundaries appear to be, on the north-west side, a thickly bedded granitoid schist, and on the south-east side, forming the barrier over which the water falls at the High Shoals, a massive range of peculiar feldspathic rocks, also thickly bedded in structure, and characterized by having the feldspar crystals, which are numerous and large, all arranged with their longer diameters parallel to the bedded structure, or generally about N. 20° E. On the southwesterly prolongation of this latter rock, which forms a good geological *meridian*, wherever it crosses any of the tributaries of the river, a waterfall is found. It is probably the same range which towers up into *Crowder's Mountain*, about a dozen miles in a southerly direction.

Proceeding to the north-eastward from the High Shoals into Lincoln county along this belt of talcose and quartzose schists, many places are encountered where gold has been mined, or washed out from the beds of the small streams, among which may be especially mentioned the Shuford and Cansler Gold Mine. In the distance and apparently on the same range is seen also the high elevation in which is situated the iron mine called the "Graham Ore Bank." Fragments of limonite *gozzan* and honey-combed quartz are constantly encountered on the surface, sometimes isolated, and sometimes strewed along for considerable distances, marking outcrops. In this part of the range the quartz veins are usually found to contain, wherever they have been opened, more or less *galena*, *blende*, and *chalcopyrite*, usually with native gold. In one place specimens of *rutile* were obtained.

Going south-westwardly from the river, we find the rocks presenting similar indications, and in the course of about fifteen miles we encounter successively the "Long Creek Gold Mines," (from one of which, known as the "Asbury Shaft," much gold has been obtained); and a number of places where iron ore is or has been mined, known as the "Costner Ore Bank," the "Alison Ore Bank," the "Ormond Ore Bank," the "Ferguson Ore Bank," and "Briggs' Ore Bank." A few miles beyond the latter, on the same range, lies the celebrated "King's Mountain Gold Mine." So called "greenstone-trap dykes" are occasionally encountered, sometimes running parallel to, and sometimes across, the strata. The beds of the streams frequently contain pebbles of *black tourmaline*, and the black sand, so common throughout this country, was found to consist *here* principally of tourmaline. Immense veins, or rather *strata*, of black tourmaline were also observed in two or three places, usually veined with milky white quartz. This would form a magnificent material for monumental and ornamental purposes. A turnpike-road was found at one spot completely covered and blackened for some distance with fragments and blocks of this material. In one place was found the outcrop of a large vein of *ilmenite*. Veins of pyrites were found crossing the beds of streams, where the current appeared to have washed them bare, and one was observed which had a well-marked *schistose* structure, similar and conformable to, that of the imbedding rocks, forming a true *pyrites schist*. In other places solid banks of *limonite* were found, standing in place above the surface of the ground, indicating veins (or strata?) of pyrites below. Specimens were seen of a crystalline *hematite schist*, said to form a large vein (or stratum?) in Crowder's Mountain, but this was not visited.

It was also perfectly evident, at most of the iron ore beds before mentioned, that the ore was merely the gozzan, or product of oxydation, of large strata of *sulphids*, probably pyrrhotine, existing below. In some of these places, as at the Ormond, Ferguson, and Briggs' Ore Banks, the mining operations have penetrated in places to the unaltered, or only partially altered, pyrites. At the Alison and Costner Ore Banks, which are excavations into *strata* of ore from thirty to forty feet in width, the material thrown out is a true *magnetite schist*, mixed however with much limonite.

Throughout the whole range, wherever examined, the talcose schists were found to contain in numerous places small seams, incrustations and stains of a black substance, which gave blow-pipe reactions for *cobalt*. At every one of the mines above mentioned, the ore, or refuse material thrown out, was found to be more or less coated with this substance. At the Ormond Ore Bank, particularly, so much of this substance was found mixed with the ore, that it is probably connected with the reputation of the iron produced from the latter, for hardness and toughness, throughout the surrounding country. At the Asbury Shaft of the Long Creek Mines also, the masses of quartz thrown out from the vein were found thickly incrusted with mammillary masses of this *wad*, or earthy cobalt. It cannot be doubted that it is the gozzan of some cobaltiferous sulphid existing unaltered in the rocks below. If this substance has ever attracted the attention of mineralogical explorers in this section, it has probably been taken for earthy manganese, from which, however, it may readily be distinguished by being very soft, smearing the fingers, and, when cut with a knife, exhibiting on the section a bright black lustre like that of massive graphite; and to these properties it owes the designation of "black lead," by which it is known among the people of the country (to whom it is familiar).

At a spot about a mile in a north-easterly direction from the Long Creek mines I found, crossing at right angles the road from Lincolnton to Yorkville in South Carolina, where the latter passes over an elevation called the "Cross" or "Paysour Mountain," the outcrop of a large "vein" or stratum of the rock, which contains very much of this black gozzan or wad. It can scarcely escape the attention of a person travelling along the road, as it appears like a broad black band, at the side of the latter. At this spot it measures about fifteen feet in width. A small opening was made into it three or four rods from the road on the southern side, and it was found to be about twelve feet wide, included between walls of talcose slate, but so highly decomposed that no satisfactory evidence could be obtained of its character where unchanged. It was traced, and opened again about half a mile to the southward from the road, and found to consist there of a number of parallel strata, separated by seams of talcose schist one or two feet wide. The largest of these strata was ten feet wide, presenting a solid bank of limonite mixed with a little quartz, so compact that it was with difficulty broken by a pick. Following the Yorkville road southerly from the point where the vein crosses it, there are encountered, interstratified with the talcose schists, several narrow bands of a granular quartzose schist, which call to mind the *itacolumite* of Humboldt. In many places this is highly granular and friable, and might be called a saccharoid quartz schist.

Following the vein northerly from the road, the outcrop was found to descend rapidly along the western slope of the Cross Mountain, and at about a quarter of a mile from the road was found a spot where the ground consisted in great part of fragments of the cobaltiferous gozzan. Openings made here would probably lead to interesting and valuable developments. A determination of the quantity of mixed oxyds of cobalt and nickel, contained in the substance found at this spot, gave 13.26 per cent. The presence of oxyd of nickel in considerable quantity in this mixture was proved by the method of Liebig,* that is, by passing a strong current of chlorine, for some time, in the cold, through the solution, to which has been previously added cyanid of potassium and caustic soda in large excess, by which $Ni^2 O^3$ is thrown down as a black precipitate. This Cross Mountain gozzan was found, by qualitative analysis, to contain, besides *cobalt*, *nickel*, *manganese*, and *iron*, small quantities of *copper*, *bismuth*, *zinc*, *lime*, *alumina*, *magnesia*, and *glucina*. Traces of *sulphur* were also found by Mr. James R. Brant, who assisted in these examinations. No indications of *arsenic* could be obtained.

In the mineral from the Asbury shaft, qualitative examination detected *iron*, *manganese*, *cobalt*, *nickel*, *copper*, *bismuth*, *zinc*, *alumina*, *silica*, *lime*, and *magnesia*, besides doubtful traces of *tellurium* (?) *Sulphur* in traces was found by Mr. Brant, but no trace of *arsenic*.

The substance from the Ormond Ore Bank may be called a cobaltiferous earthy manganese, or granular and amorphous *hausmannite*. It gives with chlorohydric acid a deep brown-black solution, with evolution of chlorine (like hausmannite), which solution contained *iron*, *manganese*, *cobalt*, *nickel*, *copper*, *bismuth*, *alumina*, *glucina*, and traces of *baryta*, *lead*, and *magnesia*. There were also traces of *sulphur*, but no *arsenic*.

With regard to the presence of *bismuth* in these substances, I may recall to memory the recent announcement by Dr. Genth † of the discovery of *bismutite* in specimens received by him from Gaston county, N. C., although the precise locality of the specimens is not given by Dr. Genth. The Asbury Shaft and Cross Mountain minerals give deep, beautiful grass-green solutions in chlorohydric acid with evolution of chlorine, which become yellow-brown on adding water, a behavior characteristic of solutions containing considerable quantities of cobalt with iron, and by which these wads may generally be distinguished from earthy manganese.

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* Liebig & Kopp's Jahresb. 1853, 674; Annalen der Chemie und Pharmacie, LXXXVII. 128.

† Am. Jour. Science [2] XXIII. 427

As to the nature of the unaltered mineral, from which these cobaltiferous gozzans are derived by oxydation, it is possible to form a very probable hypothesis. The absence of arsenic, not only from these but from many other minerals that I have examined from this region, leads to the conclusion that this mineral must be a *sulphid*, and not an *arseniet* of cobalt and nickel, and the resemblance of these substances to the cobalt and nickel ore from *Mine la Motte* in Missouri, which is also a product of oxydation, is presumptive evidence that the original mineral may be identical with, or at least similar to, the one existing there, which was found by Dr. Genth* to be *siegenite*, containing 30.53 per cent. of nickel and 21.34 of cobalt, together with iron and traces of lead, copper, and antimony. The fact that the decomposed substance contains but thirteen per cent. of oxyds of cobalt and nickel is not against this hypothesis, for in the oxydation of such a mixture of sulphids, the produced sulphates of cobalt and nickel, particularly the latter, would be in great measure washed away, whilst the iron and manganese, passing to higher states of oxydation, would remain behind in insoluble forms. I cannot, therefore, resist the belief that these veins, when opened to a depth below the line of oxydation, will be found to contain either siegenite, or some species very much like it.

The only place in this neighborhood which has been opened to a sufficient depth to expose the character of the unaltered ore is what is called the "Bronson Shaft," at the Long Creek Mines, which has been recently worked for gold by a New York Company, and is situated about half a mile southwesterly from the Asbury Shaft, and either on the same vein, or on one running closely parallel with it. At this spot, however, unfortunately, none of the black cobaltiferous substance was found with the ore. I examined this mine, and found that near the surface and above water level, the mineral mixed with the quartz gangue of the vein is principally limonite, as usual. Below water level, which is about forty feet from the surface, the limonite gradually disappears, and the vein assumes the character of a porous mass of quartz, filled with strings and bunches of pyrites, and "vugs" or cavities lined with crystals of the same, sometimes of the octahedral variety. Beautiful specimens were found containing contiguous cavities, some lined with cubic, and others with octahedral crystals, in the same specimen. The whole mass of the vein is here saturated, of course, with water. On descending to the bottom of the shaft, about one hundred feet below water level, the vein is first found *compact* and unaltered, being here about eight feet wide, and composed of a mixture of quartz and pyrrhotine. This pyrrhotine, however, contains but traces of cobalt and nickel. I would ask here whether the pyrites, as well as the limonite, found above, is not plainly a product of the action of oxygeniferous water upon the pyrrhotine?

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* Am. Jour. Science [2] XXIII. 419.

In Emmons's recent Geological Report on North Carolina (pp. 167, 168), this pyrrhotine of the Long Creek Mines is called "arsenical pyrites." It contains, however, no trace of arsenic.

3. A Contribution to the Geologic Chronology of the Southern Alleghanies. By Oscar M. Lieber, State Geologist of South Carolina.

It has long been asserted, that lithologic or petrographic characters of rocks should never be employed as means for the determination of their geologic age. Under ordinary cases this is certainly correct, but when we investigate azoic rocks we are necessarily forced to avail ourselves of other auxiliaries than paleontology to determine their age. Within the first quarter of this century Humboldt, thus already in his "Essais sur le Gisements des Roches," availed himself of the petrographic characters of rocks to establish and systematize their general distribution in the two hemispheres.

With the crystalline slates of our Southern Alleghanies, where paleontology is altogether at fault and where thus all ordinary means of establishing their age is withheld, it occurred to me to avail myself of the assistance, which seemed to be offered by the itacolumite or elastic sandstone, a rock which is so rare of distribution and so marked in its features, that there appeared much inducement to believe *a priori* that the evidently very peculiar circumstances which led to its formation at different points were not improbably contemporaneous. This it has been sought to establish more fully in a still unpublished monograph. At present I must content myself with a very cursory glance at the conclusions elicited by these investigations.

The itacolumite was discovered in the Brazils by D'Eschwege, and was closely investigated both by him and by Humboldt. Helmersen and Hoffman found it in the Ural. Jaquemont describes a rock in Hindostan, which from later information from the seat of recent war at Delhi is clearly proved to be true itacolumite. In all the diamond regions this rock occurs, but it was chiefly with the Brazilian occurrence that a comparison of the more minute attendant circumstances was rendered possible, and singular and striking are the various similarities that present themselves when placing that occurrence side by side with our own. Not only are the superincumbent and subjacent rocks identical, but in both these localities a series of associated strata present themselves, which I have ventured to designate as the itacolumitic group. It embraces various strata of the itacolumite, properly so called, a stratum of an itacolumitic conglomerate — proving the clastic origin of the rock, — strata of specular schist and itabirite, and also of catawbarite (a rock composed of magnetite and talc) and limestone. Overlying these is a limestone, which, in a private letter to me, Sir Roderick Murchison suggests may possibly be carboniferous. Underlying them is first clay slate, then talcose slate, then mica slate, and lastly gneiss. At the other localities the same rocks are associated with the itacolumitic series, although the records of the observations at my command have paid too little special attention to this particular fact to enable a comparison to be developed with equal care. This is particularly unfortunate in regard to the Ural, as Sir Roderick Murchison has been able to prove that there the talcose slates, limestones, and silicious schists are silurian. The same has been established, however, of the crystalline slates of the Andes.

With all these remarkable and specific identities as to petrographic features at these different localities, can it then be premature, in the total absence of the positive and undeniable proof afforded by fossil remains in these rocks in our part of the world, to express the belief that we have strong reasons to suppose these rocks with us to be of synchronous origin with those of other regions? But there still remains another argument to be discussed, although a collateral one.

In investigating the distribution of gold geologically, or rather petrographically, we observe, that it has sometimes been found in the true beds of certain rocks. Omitting the occurrence of this metal in the seams of the Jurassic Karpathian sandstone of Vöröspatak, as clearly furnished by injected trachytic dykes, we find gold distributed in the true beds (we have no reference here to veins) of clay slate in the Ural, according to Helmerson and Hoffman, and in South Carolina according to my own observations, of itacolumite in the Brazils according to Humboldt and Eschwege, and in Alabama, where I have seen it myself, of talcose slate, of which I know but one definite occurrence observed by me in South Carolina, although the Gold-Hill mine in North Carolina may perhaps be another instance. The rock is there chloritic.

Now, the universally observed succession of these rocks, commencing with the uppermost, is itacolumite, clay slate, and talcose slate, and it is worthy of remark, that in the Brazils the lower portion of the itacolumite is alone auriferous. In the talcose slate gold veins are most abundant in all gold regions, although I cannot here furnish the table I have made out. In the itacolumite the number is least, in the mica slate underlying these rocks no auriferous true beds occur, and the number of gold-bearing veins is again greatly diminished from what has been discerned in the talcose slate, while the still lower gneiss contains far less again. All this points to a distribution of gold contemporaneously with the deposition of the talcose slate, clay slate, and older itacolumite, and has led me to venture to question the theory of the illustrious Murchison, that gold was introduced into these rocks immediately preceding our epoch; a theory which, to say the least, would induce us to inquire why the injection did not occasionally spread upwards into overlying formations, and why the underlying rocks were less liberally supplied with this metal.

That the gold *collected together* at a later period is very clear, and of this agglomeration, by whatever force it may be produced, we have evidences of an unquestionable character with every metal occurring in its native form in nature. Wherever we find a mass of native silver or of copper we must admit such an action, if circumstances induce us to regard them as derived from compound minerals, for otherwise they would be distributed in microscopic particles. This may also furnish an explanation of the occurrence of the two native metals at Lake Superior. With reference to gold in particular I have sought to collect all the different evidences, but the subject is too broad a one for discussion in this already somewhat extended paper.

Before closing I would merely add, that if the proof at our disposal is sufficient to establish the introduction of the gold as synchronous with the origin of the rocks themselves, then we certainly have an additional proof to produce in favor of the contemporaneous origin of the auriferous and petrographically identical rocks at their different localities, and hence also are in possession of important additional grounds for supposing that, in all probability, the crystalline slates of the Southern Alleghanies are silurian.

4. THE CHEMICAL CONSTITUTION OF CERTAIN MEMBERS OF THE CHATHAM SERIES IN THE VALLEY OF DEEP RIVER, NORTH CAROLINA. By Dr. E. EMMONS, of Williamstown, Mass.

IT has been maintained by members of this Association, that the chemical constitution of a formation furnishes important evidence respecting its age. That there is truth in this doctrine we may be assured, when we compare certain well-known members which belong to several systems, of whose relative position there can be no doubt. Thus, the calciferous sandstone of the Silurian system is universally a magnesian rock, and many other examples might be cited of a similar import, but I deem it unnecessary to occupy the time of the section with details of this kind.

I shall take it for granted, then, that in many instances where the series are known to be parallel, the chemical constitution is also similar, though it by no means follows that it is necessarily so; yet the instances are sufficiently numerous to warrant the conclusion, that chemical constitution should have some weight in making up a decision respecting the age of a formation, when other kinds of evidence are not fully satisfactory. I shall now proceed to state the composition of certain members of a series which I have denominated the *Chatham series* in my reports and communications to this Association, and which I have also referred to the Permian system. In the communications referred to, I pointed out that the series in question consist of red and gray sandstones below, and then in the ascending order, black bituminous slate, coal-iron, and fire-clay, and green calcareous shales, interlaminated with thin beds of limestones, all of which are overlaid by red and gray sandstone conglomerate, &c., and which are more or less mottled by various colors. The masses between the sandstones, namely, the slates, shales, and limestones, constitute the parts of the series whose composition I wish to relate, inasmuch as they contain an element which is common to one division of the Permian system as it exists in Europe, and which is so constant as to have conferred upon it the name, '*Magnesian Limestone.*' In the Chatham series, the Permian of this country, *magnesia* is also an important element. Thus the green shales are composed of

Carbonate of lime	35.50
Carbonate of magnesia	9.25
Alumina and protoxide of iron	15.70
Silex	36.38
Water	2.59

We have, then, over nine per cent. of magnesia in the green shales, whose aggregate thickness is probably two hundred feet. In the black bituminous shales it is also present. It is, however, in the limestones that it becomes more important. Thus, a bed at Ivander McIvers, of a blue color, consists of

Carbonate of lime	46.00
Carbonate of magnesia	29.70
Silex	10.40
Protox. iron	5.00
Water	2.40
Bitumen and loss	

Another limestone of a grayish or drab color, and finely granular, is composed of

Carbonate of lime	42.60
Carbonate of magnesia	16.00
Carbonate of iron	19.38
Silex	16.20
Alumina	.75
Water	2.00

According to the foregoing analyses, we have a formation between the Sandstone which is strictly magnesian; and the conclusion to which I have arrived from this fact is, that these limestones and shales are parallel with the magnesian series in the Permian system. I admit that this evidence is to be taken in connection with another fact; that still above and overlying this series is THE TRIASSIC, a conclusion to which I had arrived at least four years ago, and which is now confirmed by the decision of Prof. Heer of the University of Zurich. Having, then, the Triassic above, and a magnesian series two thousand feet below, beneath which there still remains a sandstone three thousand feet thick, I think we may feel a certain degree of confidence, that we have not greatly erred in assigning the Chatham series to the Permian system, seeing also that the same series contains *Thecodont saurians*, whose place is truly in this system, and from which are excluded all those forms which characterize the Triassic and Liassic series.

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5. REMAINS OF THE AMERICAN MASTODON FOUND ON LONG ISLAND, NEAR NEW YORK. By J. C. BREVOORT, of Brooklyn, New York.

FIVE molar teeth and a few fragments of bones belonging to the American Mastodon were found on Long Island towards the end of last March, by the workmen engaged in removing the pond muck at the head of Baiseley's pond near Jamaica. This pond is an artificial one, covering some forty acres in a shallow valley, and is one of the series of five similar ponds which are now being prepared and cleaned out for the purpose of affording a water supply to the city of Brooklyn. In Baiseley's pond a deposit of mud, in some places six feet in depth, has to be removed before exposing the gravel bottom.

This deposit is continued for some distance up the valley and beyond the flow line of the water, thus proving that most of this deposit has been the result of vegetable growth in the open air. In this last locality the boggy surface is covered with a thick carpet of moss interspersed with a few trees which are able to flourish in such a soil.

The remains in question were discovered in this last-mentioned spot, about twenty yards from the channel of the stream which runs into the pond, and resting on the gravel, covered by about four feet of the muck or peat.

The bones and a few fragments of an ivory tusk are much broken up, and crumble on exposure to the air. The top of the lower jaw is very distinct. Two of the pieces of bone show articulating surfaces, but their exact position in the skeleton has not been determined. They appear to belong to the fore leg. The five molar teeth were in excellent preservation, the enamel being colored black, but perfect, while the roots were less complete, but nearly whole.

According to Owen's Odontography, where the series of teeth is figured, it would appear that the present molars were those of a young adult Mastodon. The anterior ones are worn on every cusp, while the large posterior molar is only slightly worn on two of the anterior cusps.

From the fact that so few bones were found, it would seem that the animal (as in some other localities has been supposed to be the case), was not mired in the spot where the fragments were found. Neither could they have been washed by floods into their position, for the Long Island streams are not subject to freshets, their course being too short and the soil too porous to allow the accumulation of waters overflowing their banks. It is, however, probable that the creature died not far from the spot where these remains were found, and that they were gradually covered up by the vegetable growth of centuries.

Remains of the American mastodon have not been hitherto found *in place* on Long Island. Dr. DeKay, in the Zoölogy of the State of New York, mentions the finding of a jaw with some teeth attached on the south beach near Riverhead. These had probably been washed out of some vegetable deposit similar to the above, by the gradual encroachment of the sea on the land.

It will not be considered necessary, in this brief notice, to allude to the geological formation of the island, which is well known to be a submarine drift deposit, as more fully described by Prof. W. W. Mather, in the Geology of New York, part 1.

The occurrence of these fragments *in situ* is very interesting, as proving the island to have been upheaved at a very ancient date.

6. OBSERVATIONS UPON THE PRACTICABILITY OF REACHING THE NORTH POLE, WITH A MAP. By ISAAC I. HAYES, M. D., of Philadelphia.

I VENTURE to trespass upon the time of the Association for the purpose of directing attention to a profitable field of scientific inquiry.

The announcement made by Dr. Kane of the discovery of an open sea within the Arctic Ocean was received by the scientific world with many regrets that circumstances had not afforded an opportunity for a further investigation of the subject. The phenomenon was witnessed at the most remote point reached by his exploring parties, and at a period of the cruise too late to admit of further effort. I wish to call attention to the facilities for doing what could not then be done,—for continuing the explorations begun by American citizens upon the most northern lands of our planet. It is for this purpose that I have appeared before this representative body of American science.

Arctic geography has been slowly developed; and while wonderful revelations have been made every year in the torrid and temperate latitudes, until scarcely a square mile of the earth's surface within those limits remains untraversed by the explorer; while time after time the earth has been circumnavigated, the Poles, about which the whole fabric revolves, are enveloped in mystery.

The northern coasts of Asia, Europe, and America, have been determined with reliable accuracy; but whether the Russians are correct in their belief in the existence of a great continent lying to the northward of Asia, or whether the Archipelago to the northward of America continues even to the pole, we do not know; yet, while in the arbitrary divisions of the aqueous portions of the earth's surface, the "Arctic ocean," embraces the watery area within the parallel 66°

33′, we may, I think, safely suppose that, throughout the greater part of the actual coast line circumference of this great Mediterranean basin, we have determined the polar land limit. Of the continental masses of land, any portion of which has hitherto come within the range of exploration, the only one remaining unknown upon its northern face is that of Greenland, and by referring to the rude diagram which I have projected for the inspection of members, it will be seen, that this continent, with the adjacent land of Grinnell to the westward, continuing as already observed further towards the Pole than any other upon our globe, gives us the most central position for exploration.

I need hardly say, that the great difficulty which has been encountered by all navigators who have attempted to penetrate within the polar basin, whether for the accomplishment of the north-west, or the north-east passage, or to reach the Pole, has been the great *ice barrier*, which invests with an irregular line the entire area, and extends in the western Atlantic considerably to the south.

Although the position and movements of this ice barrier are well known to the members, having been frequently defined, I have nevertheless thought it necessary briefly to sketch its outline.

It is sometimes brought by the great polar current of the Greenland sea and Davis' Strait as low as the forty-eighth parallel of north latitude off Newfoundland. Continuing thence parallel with the Labrador coast to the sixty-fourth or sixty-sixth degree, its outline forms a bight in Davis' Strait, conformable with the remarkable curve of the Greenland current; thence along the coast southward, it rounds Cape Farewell, and springing over the Greenland sea to Nova Zembla, it invests sometimes Iceland and Jan Mayen; thence eastward along the coast of Asia, it joins America over the Strait of Behring; and still following closely the tortuosities of the shore line, it dips down into Hudson's Bay.

The area invested by this irregular ice coast-line is not far from six millions (6,000,000) of square miles.

This is not, however, one continuous ice margin, but its southern edge presents a broken outline, penetrable at many points; and through the partial openings caused by currents, tides, storms, and changes of season, the Pole has been approached from several different quarters. The northern edge, which has hitherto appeared as a solid and impenetrable barrier, is much less irregular than the other, and its outline invests a much smaller area. In the line of Baffin's Bay it is not reached until we have advanced nearly two thousand miles to the northward of the point where the ragged border of the drifting ice presents the first obstruction to free navigation. Dr. Kane was arrested by it in Smith's Strait, in latitude 78° 37′. Scoresby, in the whale ship Baffin, penetrated in 1822 to the east coast of Greenland, through the broken belt of the polar current, but his northward progress was stopped by an unbroken field, commencing at Gaal Hamke's Land, latitude 75°, and stretching east and north to the regions above Spitzbergen. To the northward of Spitzbergen we lose its continuity, and the conflicting reports of the early Dutch navigators leave us in doubt as to the actual position of the impenetrable barrier. There can be no doubt, however, but that the closest approaches to the Pole by water have been made through this remarkable indentation, known of old as the "Fisherman's Bight." To the eastward of Spitzbergen we meet the solid barrier resting on the northern face of Nova Zembla, about latitude 77°, where it was met by Barentz and others of the earlier navigators; and eastward again from Nova Zembla it follows so closely the trend of the shore that the Russian navigators and traders have been able to accomplish but little more than a slow and precarious passage through the varying channels which, during the summer, separate the coast lines of the ice and land. Above Behring's Strait, Cook, Kellet, and Rogers were checked at latitudes 70° 45′, 72° 51′, and about 73° respectively, and Sir Robert McClure, following the line of the coast eastward from Icy Cape, found an impenetrable barrier continuous to the north coast of Bank's Land. Parry could neither pass to the west nor to the north of Melville Island, and Sir Edward Belcher was arrested in the Queen's Channel, in latitude 76° 52′.

The area invested by a line intersecting these several points of extreme navigation would equal about three millions (3,000,000) of square miles.

Whether this extensive area is a uniform sea of ice (as it has appeared from whatever point observed) has been a fruitful source of conjecture. We were almost wholly wanting in positive knowledge until the present century.

Failing in every effort to penetrate to the centre of the polar basin by water, and discouraged by the fruitless attempts of Baffin, Hudson, Poole, Phipps, and others, it was determined to alter the plan of operations, and boldly push out with sleds upon the frozen sea. From four several points such efforts have been made, — by the Russians from the Kolyma and Jana; by the English from Spitzbergen; and by the Americans through Smith's Strait.

The lead was taken by the Russians. In 1810, Hendenström and Pschenizyn started from the mouth of the Jana with the dog sledges of the native Samoides, in search of a continent supposed to lie to the northward. Ten years later they were followed by Anjou and Wrangel, who were despatched by the Russian government for the solution of the same interesting geographical problem. The ice over which these several parties travelled was found mostly smooth, both in the outward and return passage, but their northward progress was checked by their coming suddenly upon open water, — "a vast, immeasurable ocean," as described by Admiral Wrangel, — to the northward of Kotelnoi, stretching eastward in the direction of Cape Schelagskoi. The time of observation was the cold months of February, March, and April.

A similar attempt was made from Spitzbergen in 1827, by Sir Edward Parry, using boat sledges and men, instead of dogs, for draught. Parry was carried by the Hecla to Hakluyt Headland on the northern side of the island, and starting upon the solid ice from the Rocks known as the "Seven Islands of Spitzbergen," a little above the 80th parallel, he made his descent in a due north line. Reaching to latitude 82° 45′, he discovered the whole body of the ice upon which he travelled to be drifting southward, and finding that he was losing ground, he was forced to return.

The attempt of Dr. Kane to penetrate to an "open Polar Sea," was made from a point nearly upon an opposite meridian from the line of descent of Hendenström and Wrangel. Like the Russians, he used the dog sledges of the natives. Having been arrested with his vessel by the solid field ice, as already observed, he passed the winter of 1853–54 at Rensselaer Harbor, in latitude 78° 37′, and early in the spring commenced his sledge operations. The first parties failed, in consequence of the roughness of the ice and want of provisions, to reach beyond latitude 80°; but later in the season, a final and more successful effort was made by William Morton and Hans Hendrick, an Esquimaux hunter. Early in June they left the brig with a single team of seven dogs, and replenishing their stock of provisions at a *cache* previously placed at the base of Humboldt Glacier, in latitude 79° 12', they made from this point a final start. After encountering more than the usual share of difficulties and dangers peculiar to ice travel, all of which were overcome with manly energy and determination, they entered on the 20th a new channel, opening nearly due north from the north-west angle of Smith's Strait. This channel, which now bears the honored name of that Secretary of the Navy,* who, obedient to the dictates of patriotism, humanity, and gallantry, gave all the weight of his official position and personal influence toward the organization of the expedition, was found open from latitude 80° 20′ to 81° 15′, the extreme limit of exploration. Here, from an elevation of about four hundred feet, Morton looked out to the northward upon a boundless, iceless sea.

Although these several explorations cover only a comparatively small area, yet their results are harmonious, and point quite clearly to this result, namely, that there is an open sea within the boundaries of the ice barrier as already delineated, — a watery area encircled by a belt or zone of ice. That this is the case in summer, there can, I think, be no doubt.

Viewed alone, however, the discovery of those open spaces would indicate nothing. The open area of Dr. Kane did not exceed four thousand (4,000) square miles. That seen by the Russians was at no time as great. Parry did not reach to open water at all, and its existence to the northward of his terminus is only conjectured from the fact that the whole body of the ice upon which he travelled was moving from the pole. But the many observations made in important localities give, when viewed together, a positive value to the results separately reported by the explorers. A multitude of causes, local and accidental, might operate to produce the result of open water as above noted; but water seen as a part of a great open polar sea, stands toward other observed phenomena in the relation of cause and effect.

These observations I have thought of sufficient importance to dwell upon at considerable length.

I will first call attention to the bird-life. Morton reports having seen the cliffs, sea, and air about the open water of Kennedy channel, literally swarming with Brant geese (*Anas Bernicla*), Eider ducks (*Somateria mollissima*), and the Fulmer (*Procellaria glacialis*), and glaucous and tridactyl gulls. Sea swallows (*Sterna Arctica*), dovekies (*Uria Grylle*), and other varieties of waterfowl, easily recognized as familiar friends of other days in Baffin's Bay, crowded the rocky shores and cliffs of the expanding strait. Representatives of these same families have been observed flying northward in the spring from almost every quarter; at Nishne Kolymsk, at Koola in Lapland, at the mouths of the Mackenzie and Coppermine, and at intermediate points. At Rensselaer Harbor and other stations within Smith's Strait I have seen large flocks flying in the same direction. These birds migrate north to bring forth their young. They are essentially marine, since they draw their subsistence entirely from the ocean, feeding mainly upon *molluscan* and *crustacean* life. Their breeding places are hence selected where instinct directs to open water.

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* Hon. John P. Kennedy.

We are wanting in any connected series of observations, beyond a certain line, which will give us conclusive results as to the climate about the pole, but there are many isolated facts which point to considerable modification of temperature, as we pass beyond a point, not yet determined with positive, although within reasonable limits. If it can be shown, as has been assumed, that a milder climate exists at the pole than a few degrees further to the south, it must be mainly owing to the influence of open water; if not permanent nor uniform, at least to a greater degree, and for a longer annual period, than further to the south, for an ice-covered sea would have all the frigorific effect of a solid body of land.

That a milder climate does exist at the pole than a little further south is clearly shown, or at least strongly indicated, by the isothermal curves, which bend up over the Greenland sea and Behring's Strait, exhibiting a disposition to join together from either side, and to revolve around two centres — one above Siberia, the other above America — giving us the same temperature at latitude 90° as at the arctic circle. Sir David Brewster, when establishing his centres of cold and magnetism, used as a basis the isotherms of Baron Humboldt — which cease at 32° of Fahrenheit's scale — and the material principally of Giesecke, Parry, Erman, and Wrangel, placing them upon latitude 80°, giving them the temperature at zero. Subsequent observations, and many of them of very recent date, in the hands of such able philosophers as Kupffer, Berghaus, Prof. Dove, Capt. Duperrey, and others, have brought these points of maximum cold and magnetic intensity nearer to the continents, and although not yet accurately determined, they have doubtless movable centres near the crossing of the 78th parallel of latitude with the 95th meridian of western and the 120th of eastern longitude.

That the first named of these positions is not far from the true American pole of cold is shown by the elaborate isothermal charts of Baffin's Bay and vicinity, projected by Charles A. Schott, Esq., of the U. S. Coast Survey.

An observation, valuable in this connection, will be found recorded in the able report of Mr. Durand upon the Floral Collections of Dr. Kane's Arctic Cruises, published in the Journal of the Academy of Natural Sciences for 1856. By an examination of this report it will be seen that two plants (*Hesperis Pallasii* and *Vesicaria Arctica*) were brought from the lands lying to the northward of the Humboldt Glacier, latitude 81°. These plants do not appear either in the collections made by Dr. Kane during his first voyage, or by myself during his second, further south upon the Greenland coast. The first named was found by its discoverer, Pallas, in Russian America, and, by the authority of Hooker, we meet with it upon the same isothermal curve in Labrador. It has not been discovered in the cold zone lying between Labrador and Washington Land, from the 60th to the 80th parallel, either on the Greenland or American side of Baffin's Bay.

The migrations of human life also furnish us valuable data. The traditions of the Esquimaux point to the north as the central home of the race. Erasmus Yorke spoke of his mother having come from the north, and when Sacheuse told the inhabitants of Prince Regent's Inlet that he came from the south, they replied that it was impossible, since in that direction there was nothing but ice. Egede and Crantz both assign to the Esquimaux a northern origin, and the inhabitants of Smith's Strait have repeatedly told me of the day when their forefathers used to hunt the reindeer and the musk ox — the *Tooktook* and the *Oomeak* — far away to the northward. These traditions would seem to be borne out by positive observations. The remains of their old encampments are found along the east shore of Smith's Strait, from the 78th to the 81st parallel, and I have discovered the same relics of a numerous colony on the opposite coast near latitude 80°. I believe the northern shores of the lands of Grinnell and Washington will be found numerously peopled, and that the link within the channel has been broken, in consequence of the destruction of the hunting grounds by the gradual enlargement of the great Humboldt glacier, and the enormous discharge of icebergs therefrom, producing a local increase of climatic severity. A similar glacial accumulation in Melville Bay, between latitudes 74° and 76°, has cut off communication between the inhabitants of the head waters of Baffin's Bay and of southern Greenland.

That the central home of the race is nearer the pole than yet explored, would seem to be indicated by the fact, that as we advanced southward the race deteriorates. The most northern known branch of the family is that with which Dr. Kane had constant communication, at the mouth of Smith's Strait, and these people exceed in physical development any others hitherto discovered. In south Greenland they are quite diminutive, so much so that the Northmen who discovered the continent in the tenth century, named them, from their diminutive appearance, "Skrællings." The same fact is observed in Labrador; and the old Norwegians, who in their wild wanderings came early in the eleventh century several degrees lower on the coast, found the same people, with the same characteristics. My excellent comrade, the late Mr. McGeary, has assured me that the natives about Pond's Bay are far superior in physical development to the inhabitants of Labrador. In some of the southern settlements I have found the height five feet four inches (5 feet 4 inches) very unusual; five feet one inch was thought by Crantz to be a fair average. The Smith's Strait Esquimaux reach frequently to five feet six inches; several measured by me gave five feet seven inches, and one, five feet eight and a half inches. These facts are valuable, if we accept the theory that races deteriorate as removed from the parallel of their nativity, and if they inhabit the lands to the north of Smith's Strait, there is there continual open water, for they, like the birds, draw their subsistence entirely therefrom, and like them, too, supply their wants from day to day through summer and winter.

But the most important facts which we have to consider are of direct determination — I mean the observed water temperatures and their known influence. The facts in our possession, although few, and confined to an extremely limited number of observers, are nevertheless conclusive as to this one point, namely, that the great body of the waters of the Polar Basin has a temperature considerably removed from the freezing point; and to the immense frigorific influence acting continually upon them, there is evidently a calorific force antagonistically operating. That the latter holds sway during the summer, and clears the greater part of the basin from ice during that period, there can be no doubt. How far its influence is felt during the period of the year when the atmospheric temperature sinks below the freezing point of mercury, we have, as already stated, little positive information. There are some observations, however, upon this point, which have interest and value.

Baron von Wrangel, at his winter quarters at Nishne Kolymsk, records that the temperature rose in winter and fell in summer with the north winds, and that the "N. N.W., and sometimes the N. E. winds were damp to a degree which was sufficient to wet their clothes;" and according to Mr. Tooke, at Koolo "the cold diminishes during the severest weather with the northerly winds." Barrentz at Nova Zembla found a heavy swell coming from the northward, clearly indicating the close proximity of open water; and the same fact has been noted at the Seven Islands, though I should observe that this only relates to the summer.

Sir Edward Parry, upon whose observations we may always properly rely, found at Winter Harbor, Melville Island, that the north winds were often warm. The same fact was noted by Dr. Kane at Rensselaer Harbor, and although the north and north-east winds were frequently cold during the winter, yet the summer winds from that direction brought sometimes warm and foggy weather, and throughout the year a most curious phenomenon was witnessed upon the north horizon. A heavy mist bank floated there almost continually, and "mist bank on the north horizon" became a stereotyped entry in our meteorological tables.

I give these facts without comment or opinion, and will content myself with grouping them with other observed phenomena.

The water to the south-west of us was open throughout the winter as well as summer. I saw, in December, 1854, a large watery area off Cape Alexander, and as late as the 29th of the following month, Dr. Kane discovered a broad sheet, nearly free from ice, stretching out from Cape Hatherton, forty-five miles to the south-west of Rensselaer Bay. This water was frozen over during February, but we have the testimony of the Esquimaux that to the south of Cape Alexander, at a point opposite Ak-oot-loo-nik, latitude 77° 30′, there was an open sea as late as the middle of March, and in closing his journal entry of the fact, Dr. Kane "questions very much, if such extensive areas as the so-called north water ever close completely;" and this "north water," as the whalers call that portion of Baffin's Bay lying to the northward of the "middle ice," has a superficial area of less than eighty thousand (80,000) square miles. The Esquimaux inhabiting its shores are dependent upon its remaining open throughout the winter for their existence. A sudden or long continued closure would be the signal for their total destruction by starvation, for these improvident creatures have rarely reserved stores sufficient to last them during two weeks.

I will not hazard any opinion as to the mid-winter condition of the sea to the north of Rensselaer Harbor, in which direction the mist bank was seen; but we know, that one hundred miles in a due north line from it there was an open sea in June, 1854, as shown by the well-authenticated report of William Morton. The temperature of this sea, as determined by Morton and as recorded by his commander, was 36°; two degrees above the temperature of the air at the time of observation, and eight degrees above the freezing point of sea water. Another observation, made in a different locality, gave a temperature for the sea water of 40°. This water came down from the north at the rate of from two to four knots. From the same quarter blew a strong gale of wind, and no ice was seen, as far as the eye could reach, at an elevation of from three to five hundred feet.

Where this warm water comes from is an important question, and one involving the whole system of Arctic currents, about which so little is positively known. That little I have not the wish, even had I the ability, to use in any speculative hypothesis. There are members of this Association, who, by their devotion to science, have earned a claim to speak upon this topic, and in their able hands I leave it.

I cannot, however, wholly abandon the subject without citing a few observations, and calling attention to the striking tendency which seems to exist towards a great system of revolving currents. In the Greenland sea near Spitzbergen, the deep-sea water is, according to Scoresby, warmer than the surface. A series of observations made by this distinguished navigator and philosopher between the tenth meridian of western and the tenth of eastern longitude, and between the parallels 76° 16′ and 80°, show a constantly increasing temperature from the surface downward. The mean of nine observations with a mean latitude 77° 40′, gave a surface temperature 29.1° with extremes 28.1° and 30°, and at a mean depth of 665 feet the mean temperature was four degrees higher. A single observation, taken in latitude 79°, at the depth of 2,400 feet, gave a temperature of 36°; at 4,380 feet the temperature was 37°; and at 4,556 feet, in latitude 78°, the temperature was 38°. Dr. Scoresby thought this deep warm water to be "a still further continuation of the Gulf Stream." Be this as it may, the current about the new Siberian islands is westward, and we have doubtless a continuation of this flow in the great Polar drift of the Greenland sea; another current sets south through the estuaries leading into and from Baffin's and Hudson's Bays.

That there are northward currents to keep the waters of the Polar Basin in equilibrium must be evident. In Baffin's Bay, it is well known that the deeper icebergs are carried northward, while the surface ice is drifting south, thus showing a deep-sea current setting toward the Pole. The Behring Strait current runs in the same direction. The Gulf Stream, after sending off a bifurcation to the south-east, stretches northward to the Norway coast, and is lost to observation beyond North Cape. The great Arctic water-sheds of Europe, Asia, and America, equalling together nearly five millions (5,000,000) of square miles, pour down their immense floods of warm water into the Arctic seas.

Speculate as we will, the facts in our possession show conclusively what was assumed in the outset, namely, the existing of a cause constantly operating to keep the waters of the north Polar Basin elevated above freezing; and fully believing that this cause, aidėd by the influences of the superficial local action of winds, tides, and currents, is sufficient to produce a constant open sea in the neighborhood of the Pole (that is, assuming the immediate vicinity of the Pole to be water), I have great confidence that it can be successfully navigated. The route most practicable for accomplishing this purpose, I believe to be through the great Polar estuary separating the lands of America and Greenland, assuming severally the names Davis' Strait, Baffin's Bay, Smith's Strait, and Kennedy Channel.

The direct efforts hitherto made for the purpose of reaching the North Pole have been confined to the Greenland sea. The first attempts were made at the time of the origin of the famous Muscovy company, when the commercial nations of Western Europe held rivalry for the quickest passage to the Indies. Their efforts have been continued at intervals until our own time, and have uniformly resulted in failure as to the principal object.

Hudson is said to have penetrated in 1607 to latitude 82°, but the conflict between his nautical observations, and his descriptions of land make it doubtful whether he passed beyond 81°. The brave but unfortunate Barentz, and his companion, Corneliz Ryp, eleven years earlier had been driven back from the eightieth parallel; and Fotherby, a few years later, after repeated efforts, had no better success. Poole, in 1610, reached only to 79° 50′. The greatest latitude of Captain Phipps, in 1773, was 80° 48′; of Tschitskagoff, in 1776, 80° 30′; of Scoresby, in 1806, 81° 30′; of Buchan and Franklin, in 1818, 80° 32′; of Clavering, in 1823, 80° 20′. They were all arrested by the solid ice barrier near the meridian of Spitzbergen.

The closest approach to the Pole, of which we have any well-authenticated record, was made by the late Sir Edward Parry, to whose daring attempt I have already referred. This fearless navigator, abandoning his vessel, boldly pushed out upon the boundless ice-covered ocean, but was forced to retreat after reaching to 82° 45′.

It would be highly important to determine with positive accuracy the progress in this direction of the early Dutch and English fishermen. Barrington declared that the ice barrier was not continuous from Greenland to Nova Zembla, but was broken on a movable meridian above Spitzbergen, allowing a free passage into the centre of the Arctic Ocean, which he maintained was an open sea, and had been frequently navigated. In two papers read before the Royal Society in 1775, he cites twenty-six instances of navigators having reached to high north latitudes, and asserted, that a vessel had gone even to the very Pole. But the testimony upon which these extraordinary reports are founded is extremely loose and unreliable, and subsequent writers have attached very little importance to them. Scoresby and Dr. Kane wholly discarded, and Barrow only partially accepted them. If true, these seas have since undergone remarkable changes in their physical aspects; and after a careful examination of the records of the more recent navigators, I am convinced that the route to the North Pole, eastward of Greenland, is at this time neither practicable nor safe; and I believe that there is very little probability that that Pole of the earth will be reached wholly by water from any quarter. To effect this desirable object, reliance must be placed upon *boats* for the navigation of the circumpolar waters, and upon sledges for the transit of the boats over the investing ice belt. The routes of Parry and Kane are both before us. As already stated, the advantages are greatly in favor of the latter.

I have already shown, that the chief cause of the failure of Captain Parry was the southward drifting of the ice plain upon which he travelled. This difficulty is obviated by adopting the route through the channel to the westward of Greenland, for we have here the land masses of Washington and Grinnell, and the protruding tongue of western Greenland, which terminates in Cape Alexander, to check the equatorial drift of the ice as it breaks up early in the season. This route possesses, besides, other advantages equally important. The lands on both sides of Kennedy Channel have a nearly due north trend, continuing at least to latitude 82° 30′, affording a certain basis of operations upon the direct line of descent. They have a marginal belt of ice at certain seasons always smooth and secure, and they furnish a practicable field for travel, a secure foundation for depot stations, more secure than the treacherous outside ice, and a safe retreat for travelling parties if surprised at a distance from the land by a sudden disruption of the "floes."

A small vessel might be forced into Smith's Strait; and along the western shore, by working through the land leads, could reach, I have reason to believe from personal observations made in the spring of 1854, nearly to latitude 80°. Here the coast affords fine harbors for wintering. Upon the opening of spring, depot stations, or *caches*, could be established with dog sledges at different points along the land, and early in April a boat mounted upon runners might be started towards the open sea, which, enlarging with the advance of the season, would, I conjecture, be met between the eighty-first and eighty-second parallels by the middle of May. The distance thence to the Pole is not very great, and the navigable season continues for four months. I can see no insurmountable obstacle to complete success.

Gentlemen of the Association: — If I may, without presumption, address myself directly to you, I would earnestly call your attention to this subject. It is brought before us, not in the days of Baffin and Hudson, nor in those of Parry and Franklin. It comes to us rich in the experience gained by more than three centuries of almost continuous effort, demanding of us to achieve the climax of discovery, to complete the effort so long and so bravely persisted in.

I have for some time been endeavoring to organize another expedidition to those seas. After more than two years spent upon the route which I have recommended, I am convinced, that, if the experience of the past be properly used, the next attempt will, in all human probability, be successful, and that it can be accomplished with far less expense and danger than is popularly supposed. A vessel of one hundred tons, equipped for two and a half years, and manned by twelve men, is all that is absolutely necessary. To this might, with advantage, be added a small steam tender, not exceeding thirty tons, fitted with a shifting screw. Under certain circumstances this little craft could be made available in the shoal water, close under the land, in the narrow shore leads of Smith's Strait.

At first this might seem inadequate to the service, and, compared with the large force with which the English have latterly gone into the field, is indeed small; but experience has shown that success in Arctic enterprise has not always been in proportion to magnitude of preparation. The brilliant triumphs of Davis, Frobisher, Baffin, Hudson, and others of the early navigators, were made with vessels varying from fifteen to fifty tons. The expedition should leave the United States early in April, and, halting upon the Greenland coast to the south of Melville Bay, should procure at the different trading and missionary posts of the Danish government, and by the hunt, a supply of fresh meats and furs, and two good teams of Esquimaux dogs. This supply would greatly reduce the cost of the expedition, and would be in every way better than to carry all the stores from home. Large quantities of the flesh of the reindeer, water-fowl, and seal, could be obtained. The whalers annually capture great numbers of the Auk and Lumme (*Uria Alle* and *U. Brunnichii*) about Sanderson's Hope and Cape Shackleton, and at the Duck Islands they collect the eggs of the Eider fowl almost in boat loads. About Holsteinsborg and Egedesminde the reindeer (*Cervus Tarandus*) are quite numerous, their skins forming one of the staple articles of revenue from the colonies. Considerable quantities of their dried flesh (cured without salt) could be obtained of the natives and Danish officials. It was the original intention of Dr. Kane to have availed himself largely of these abundant stores, but he arrived too late upon the coast to admit of a halt without great risk of losing the chances of reaching his proposed ground of search before the close of the season. This delay was occasioned, as you are aware, by the unfortunate sickness which, during nearly a month, prevented his departure from New York.

The "Middle Ice" commences to break up about the first of June or late in May, and the passage into Smith's Strait could be effected by the first of August. Running up the coast of Grinnell Land, I should select a safe harbor at some eligible point as far north as the ice would admit, and there pass the winter. I have already expressed my belief in the practicability of reaching nearly to the 80th parallel, or at least I feel well assured, that the little Bight between Capes Frazer and Leidy, latitude 79° 42′, can be attained. An examination of this bay, with the ultimate view of one day being able to winter in it, was made by me in the spring of 1854. Before passing from this point I will call attention to some features of the physical geography of this channel, which have an important bearing upon the proposed operations, and which should govern the selection of a harbor. By a reference to the chart, it will be seen that Smith's Strait runs diagonally to the general direction of the current coming down through Kennedy Channel, and in consequence, the Greenland shore will have the ice continually forced upon it by the pressure of the moving water; the same cause meanwhile freeing the opposite coast. That this side will afford greater facilities for reaching a high north latitude will be evident, and travelling parties would be protected against the chief difficulties which beset Dr. Kane in crossing the ridges of hummocked ice in the centre of the Strait.

It is on this western coast that the small steamer to which I have referred might be made serviceable, and in this opinion I am supported by the judgment of my friend, Mr. Wilson, late the sailing-master of the "Advance." During a mild season, the dissolution of the land margin of the ice fields is considerable, and a vessel of shallow draught propelled by steam, might possibly be able to work through Kennedy Channel into the Polar Sea. But this would be too doubtful a reliance for sole dependence. The main hope of success should be in the *boats.*

Early in the spring, the shores of Grinnell Land should be, as already stated, lined with depots of provisions, as far north as latitude 82°, where a final *cache* should be established for the use of the polar boat party; these stores to be carried forward by the dogs. One of these animals will drag upon a sledge a weight of seventy (70) pounds thirty-two (32) miles per day, upon an average ration of thirteen (13) ounces of pemmican — equal to about three pounds of dried meat — and two teams of seven each could readily carry forward ample stores for a full boat's crew of six persons. This crew should start their boat from the vessel in April. Within one hundred and fifty miles they would probably, as I have said, meet the open water by the middle of May or the first of June.

The chief difficulties which have hitherto beset Arctic travellers can be mainly overcome. The rough ice which baffled Dr. Kane's parties, as above observed, can be in a great measure avoided by avoiding the crossing of Smith's Strait; and Kennedy Channel having a due north trend, and presenting no salient capes like the remarkable projection of western Greenland, will, I have no doubt, be found generally smooth. Such a track presents great facilities for travel. One man will readily walk sixteen (16) miles per day, dragging from one hundred (100) to one hundred and twenty (120) pounds in weight. Dr. Rae conducted a party six hundred miles in twenty-two days, each of his men trailing after him, upon an Iroquois sledge, one hundred and ten (110) pounds. They carried a single blanket and change of clothing per man, but no tent, using for periodic rest the snow hut of the Esquimaux. These huts are readily constructed, and upon them I shall place my sole reliance while upon the ice. Indeed, the amount of labor which can be performed by a skilful use of very simple means is truly astonishing; and in spite of the cold and poverty of the ice deserts, Kennedy, McClintock, Bellot, Sutherland, Pim, Osborne, Richards, and many others of the numerous corps of their gallant co-laborers, have performed journeys, which, for extent, would reflect honorably upon them as foot travellers in a more favored region.

The cold and scurvy are of much less serious moment than formerly. Experience has taught us many valuable lessons. By housing in the upper deck of the vessel, and using the porous snow as an external covering, the winters may be passed in health and comparative comfort, and even travelling may be performed during this season, if the parties are well supplied with heavy furs and sufficient quantities of carbonaceous food. The scurvy, hitherto often a great scourge to the crews of vessels wintering in the Arctic regions, can, with proper precaution, be resisted, and in this opinion I am sustained by the united testimony of the surgeons of Her Majesty's Arctic squadron. The disease has been of very rare occurrence of late years, and wherever it has appeared, it has been owing to accidental causes, but chiefly from the long continued use of a salt meat diet; either in consequence of the parties never having been provided with any other standard supplies of food, or of their having so long remained in the field as to have consumed their fresh stores. Indeed, I am convinced that the climate is one of unusual healthfulness. The suffering from the disease among Dr. Kane's crew was mainly owing to the above-mentioned cause. He started too early to profit fully by the discoveries which have been made in the art of preserving fresh meats and vegetables, and with the exception of a limited quantity of pemmican — intended for use in the field — he had to depend upon the ordinary navy ration, without change or variation. Casual supplies of fresh food were obtained by the hunt or in barter with the natives, and when procured, invariably enabled his men to resist the disease, or, if developed, it acted as an immediate and specific cure. The difficulty experienced in keeping alive his dogs was chiefly owing to the absence of a diet suited to their necessities. The salt of the meat acted injuriously upon them, and the insufficient quantities which they could eat, did not enable them successfully to resist the cold, and a strange epilepto-tetanoidal disease was in consequence developed among them. The same was observable among his crew, and doubtless for the same reason.

I have thus, as briefly as possible, given the reasons for believing in the existence of an open Polar Sea, and that through it the North Pole may be reached. Experience has reduced the scheme to a mere question of time. If not accomplished at this, it will be at no very distant day.

I have already announced to you my intention of laboring for the accomplishment of this object, and to this end I would respectfully solicit the counsel and sympathy of the Association. In this presence it would be useless to enumerate the results to science to be anticipated from its success. Some of them may, however, with propriety be briefly alluded to.

Under all circumstances, valuable additions would be made to our knowledge of Arctic climatology, and I should hope to determine something definite with regard to the condition, during the winter, of the waters beyond latitude 82°, and thus definitely settle the question, as to the *permanent* existence of the open Polar Sea, by passing a part of that season as far north upon the shores of Grinnell Land as possible. After entering winter harbor, and upon the earliest freezing, I should establish a depot of provisions in that direction, and with one companion and a team of dogs, would there pass the months of December, January, and February, in a *snow-house.* The land ice could be travelled during the moonlight period of each month, and observations made upon the situation and temperature of open water. Such a plan of operations is, under ordinary circumstances, quite practicable. That the months of September and October are available for the purpose of establishing provision depots, has been proven by my bold, daring, hardy co-laborer in the late expedition, Mr. Bonsall. That the winter may be passed in the manner above described, is shown by the experience of the enterprising and indefatigable chief factor Rae, who, as you are aware, passed the winter preceding his famous journey upon which he found the relics of Franklin's party, in *snow-huts* at Repulse Bay, and as he assures me, without any serious inconvenience from the cold, and without disease. To the experience of Dr. Rae I will add my own. During the autumn of 1854, while on duty connected with the safe retreat of the late expedition to the south, from Rensselaer Harbor, I passed in a similar manner with seven others of the ship's company the months of October, November, and part of December, and performed a return journey of three hundred miles, in eight days, with the dog sledges of the Esquimaux, guided only by the light of the moon and the stars. We lived during this period entirely without fire for purposes of warmth, and but for a want of food could have passed the winter in safety. The temperature of the hut sank rarely to zero, and was often as high as the freezing point, being kept thus elevated by the heat radiated from the persons of the occupants.

I have already shown the importance of these northern lands as offering positions for observations upon the *human* and *floral* life of the region; and lying almost in a direct line between the two great magnetic centres, the great value of stations in the high latitudes of Grinnell Land for magnetic determination can hardly be overestimated. Our knowledge of the oceanic currents and tides of the extensive waters of the Polar Basin is enveloped in the same mystery which invests the regions of their birth and flow, and as the proposed line of descent runs midway between the seat of Atlantic and Pacific influences, it presents a most important field for the completion of the hydrographic survey of the northern hemisphere. Indeed, there is no department of physical science or natural history to which the expedition could not be made to contribute.

The collections of natural history made during the cruise of the "Advance," which were unfortunately abandoned with the vessel, exhibited a hitherto unknown feature in Greenland zoölogy, containing as they did a number of skulls of the musk ox (*Ovibos moschatus*), found in almost every locality visited between latitudes 78° and 79°, by Dr. Kane, Mr. Goodfellow, and other members of the party on the coast, and discovered by Mr. Wilson and myself near the base of the Great Glacier, about eighty miles eastward from Rensselaer Harbor, on the parallel 78° 40′. These animals seem to have become extinct on the continent of Greenland to the south of latitude 80°, but I have been assured by the Esquimaux that they still exist to the far north, and they speak of a large island in that direction, upon which they live in great numbers.

The northern range of the *Cetacea* is not known, and it would be highly important to determine whether they inhabit the extreme northern limit of our planet. I conjecture that their central *habitat* is about the pole. Whales struck in the Greenland Sea near Spitzbergen have been caught to the southward of Behring Strait, — recognized as the same animals by the harpoons remaining imbedded in their flesh, — thus proving conclusively, that both from the Atlantic and Pacific Oceans they have free communication with the regions of extreme northness. These animals cannot live in an ice-covered sea. The once valuable fishing grounds of the Greenland Sea and Baffin's Bay are now almost unproductive, and the right whale (*Balena Mysticetus*) is fast disappearing from the "north-west coast." It would be interesting to determine whether they have been destroyed, or driven within their ice-locked fortress. This last I think highly probable; and it would seem to be shown by a fact, stated by Dr. Kane in a paper read before the American Geographical and Statistical Society in December, 1852, upon "Access to an Open Polar Sea:" namely, that he observed the white whale (*Delphinapterus Beluga*) passing up Wellington Channel, northward, through a broken sea of ice for four days in October. The barbed seal (*Phoca barbata*) was seen by Morton sporting in the waters of Kennedy Channel. These animals must have been denizens of the north, for since this species is not endowed with mechanical powers for opening breathing places in the ice, they could not have reached this water from the south, for this would have required them to pass underneath an unbroken belt of ice more than one hundred miles in width, which, I need hardly say, would be impossible. In its proper place I might have cited this fact, as well as the capturing by Morton and Hans of two polar bears (*Ursus maritimus*) near the same spot, as bearing strongly upon the existence of continual open water.

The lower forms of marine life have not been studied, and present a broad field for the investigations of the naturalist. Important problems are involved in the determination of their geographical distribution.

The great glacial system of Greenland has been subject to but little enlightened investigation. Giesecke thought this vast ice continent, as I shall venture to call it — which is the birthplace of the untold millions of tons of annual berg-discharge which come down from the north into the Atlantic — was a great congeries of islands, cemented together by ice. This continent, as it now appears, has a superficial area exceeding half a million of square miles, and it is the seat of constant atmospheric precipitation. The excess of this precipitation over evaporation — for evaporation, I may mention, takes place continually — and discharge by the river and icebergs, have an important bearing upon the physical geography and climatology of the region, and, indeed, of the whole northern hemisphere. Many of the geological phenomena of the ancient glacial period can at this time be witnessed almost everywhere on the Greenland coast.

The river system of Greenland, fed by the *Mer de Glace*, is greater than is generally supposed; large streams were discovered by Dr. Kane above latitude 78°. The great glacier of Humboldt, which bounds Smith's Strait to the north-east by a wall of ice sixty miles in length, and from fifty to five hundred feet in perpendicular altitude, is but a great frozen river (if I may so use the term) slowly flowing seaward, and discharging, piece by piece, its congealed fresh water into the salt sea, and rafting millions of tons of detritus (boulders and sand) to more southern latitudes.

Upon all these points the portfolios of Dr. Kane will be found rich in materials; and, when presented to the world, they will, I am sure, give no cause to the distinguished Superintendent of the Coast Survey to regret the words of just and generous praise which I had the gratification to hear bestowed by him, the other day, upon this highly gifted man, for whose untimely death the world now mourns. But his facilities and opportunities were limited, and much remains to be accomplished.

I need not pursue the subject further, and have already detained the Association longer, perhaps, than the occasion warrants. The deep interest which I feel in a field of investigation which has long engaged my attention must serve as my apology.

II. PHYSIOLOGY.

1. NOTICE ON TWO HUMAN CESTOIDEA, NEW TO SCIENCE. By D. F. WEINLAND, of Cambridge, Mass.

DURING my researches regarding the Helminthes or Entozoa of man, I have met with two interesting forms of Cestoids, which are new to science.

The specimens are preserved in the collection of the Boston Society for Medical Improvement, and have been kindly communicated to me by Dr. J. B. S. Jackson for further investigation.

I. *Cysticercus acanthotrias*, WEINLAND.

In Prof. J. B. S. Jackson's catalogue of the collection named above, we read under No. 904: "A *Cysticercus cellulosæ*, from a woman about fifty years of age, who died of phthisis, a dissecting-room subject at Richmond, Va. About a dozen or fifteen of the cystes were found in the cellular membrane of the muscles and in the integuments, besides one which hung free from the inner surface of the dura mater near the crista galli. In the same subject there were also numerous specimens of Trichina spiralis. — *From Dr. Jeffries Wyman*, 1845."

By a careful microscopic investigation of this worm, I found, to my astonishment, not two, but three rows of hooks on the proboscis. Thus the worm cannot belong to the well-known Cysticercus cellulosæ, which has only two rows. Referring to the three rows, I have called the species C. acanthotrias, from ἄκανθα, hook, and τρεῖς, three. There are fourteen hooks to each row, and the hooks of each row have a peculiar shape. The shape of those of the outermost row is quite different from the hooks of Tænioids in general.

A full description and figures of this interesting new worm I shall publish in my *Essay on Human Tapeworms*, now in press (by Metcalf & Co., Cambridge), as well as in my forthcoming Atlas of Human Helminthes.

II. *Tænia* (*Hymenolepis*) *flavopunctata*, WEINLAND.

In the collection named above there was, among many other helminthological specimens, a phial containing a number of pieces of a small tapeworm. According to the catalogue (No. 903) these pieces came "from a very healthy infant, nineteen months old; it had been weaned about six months, and had had the usual diet from that time. The worm was discharged without medicine, its presence having never been suspected. — *From Dr. Ezra Palmer, Jr.*, 1842."

This tapeworm differs from all human tapeworms known thus far. Its genital openings are marginal, and all on one side; this is enough to distinguish it from either Bothriocephalus latus or Tænia solium. But the best characteristic is in its eggs, which are very large, measuring 0,054 millim. in diameter, and under a low power of the microscope appear as transparent balls, with a yellow dot in them. With a higher power we easily distinguish three egg-shells. The outside shell is translucent, elastic, cracking in sharp angles under pressure, and only 0,0007 millim. thick. This shell is folded by application of glycerine. The second shell is membranaceous and irregularly wrinkled, thinner than the first, and immediately attached to it. The large cavity which is formed by these two outside shells contains a fluid, in which swims the globular embryo (measuring only 0,024 millim.), enclosed in a third shell, closely attached to it but of considerable thickness (0,001 millim). From a thorough review of the genus *Tænia*, I have persuaded myself, that this strange structure of the egg-shells is characteristic for a new subgenus, which I have called **Hymenolepis** (from *ὑμηί* membrane, and *λεπίς* egg-shell), to express the peculiar nature of the egg-shells ; other species of tapeworms, which belong to the same subgenus, have been found in shrew-mice, and in rats and house-mice.

None of the pieces which I investigated, and which came from a number of individuals, had the head preserved.

I have called the species *flavopunctata* from the yellow spots, one of which is visible in each joint. They are the testicles. More particulars will be given in the works named above.

2. A Method of Comparative Animal Psychology. By D. F. Weinland, of Cambridge.

There is no true science without a method ; and the progress of any science depends mainly upon the principles of its method, whether they are true to the object, and comprehensive enough to cover the whole field.

Yet there are branches of science in which that method, those first principles and ways of studying, are by no means fixed. This is naturally the case with those branches particularly which have been least cultivated. One of these latter is Animal Psychology, a systematic knowledge of the psychical phenomena observed in the different species of animals.

Let us first review briefly what attention has been paid thus far to the study of the soul* of animals ; then consider some of the principal difficulties under which this study has to labor; and finally we will try to show the proper method upon which the study of animal psychology should be started.

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* We have to use this term also for the psyche of animals. Science, having the right of her own nomenclature, must sometimes extend or confine the meaning of a word; thus the German word "seele" is now used of both man and animals: it was not fifty years ago, when it expressed only the psyche of man. The correspondence between the soul of man and that of animals is just as close as that between the head or brain of man and the respective organs of brutes. Still no man hesitates to call a lion's head "head," and a cat's brain "brain."

When casting a glance over the whole field of zoölogical literature — taking zoölogy in its widest sense, embracing the whole knowledge of animals and including systematic zoölogy, comparative anatomy, physiology, and embryology — from the time of Aristotle until to-day, we notice some remarkable features in reference to the matter in question.

In the classic times of the Greek literature, zoölogical studies were mainly devoted to the bodily structure of animals, their morphology, anatomy, and physiology; of animal psychology we find only traces, and these only in immediate reference to or in comparison with the study of the soul of man.

In the religious mythology of the ancients, it is true, we find animals and their endowments and passions multifariously represented, yet these characteristics of animals, of goats, lions, horses, eagles, dolphins, picture hardly any thing more than features of human animality, exaggerated either in a good or in a bad sense; often hyperbolically true, but as often utterly misconceived, they are of no more scientific value than the symbolic characteristics of pigeons, lambs, serpents, &c., of the old Israelitic and of the Christian mythology. It is quite consistent with this neglect of animal psychology in ancient times, that the best Greek artists were also exceedingly careless when representing animals in works of art. Often, in the most beautiful groups on reliefs or in statuary, where in the human figures evidently every muscle and vein is studied, we find the animals most superficially represented, evidently mainly from the memory and phantasy of the artist.

During the dark middle ages the human mind was so exclusively occupied with supernatural speculation, and so much raised above itself, that it was entirely blind to the close correspondence between the human soul and that of animals.

It was only after the Reformation, when M. Luther had broken the fetters of the old dogmatic authority, when man commenced again to study man, human nature and human psychology instead of that of theoretical saints or devils; it was only then, that the soul of animals was also deemed worthy the attention of the philosopher, and was compared with that of man: and from that time dates also a better understanding of the human soul itself, just as only comparative anatomy and comparative physiology of animals and man could disclose a true insight into the structure of the human body and its organic life. Still, even then the study of animal psychology did by no means advance equally with the rest of science. Though we find in the last and in the forelast century many a good monograph on the habits and the manner of life of animals, particularly of birds and insects, and even some works which tried to treat upon the whole field of animal psychology, yet these essays have failed to attract new students, mainly because they did not adhere to a systematic method of study, and thus could not insure any certain results.

From the time of Linnæus, the legislator of systematic zoölogy, to the beginning of the present century, the interest of far the greatest part of naturalists was absorbed in specific description, and in the classification of animals and plants. Linnæus himself being first and mainly a botanist, investigated, described, and arranged the animals mainly as if they were mere plants from a morphological point of view, and yet more so his pupils and successors. With Cuvier, Meckel, Bauer, Oken, J. Mueller, and L. Agassiz, zoölogists became comparative anatomists, embryologists, physiophilosophers, physiologists, and petrefactologists, and these five great and fertile branches of zoölogy have swallowed the whole labor of the naturalists of the last half century. Though many a naturalist has pointed out that uncultivated field of animal psychology, still we find in those hundreds of zoölogical papers, monographs, pamphlets, books, which are published annually, and which often treat of systematic zoölogy, anatomy, physiology, and embryology, with equal accuracy and ability, hardly ever a trace of animal psychology. Is it not true of many of our best geological writers, what Goethe says in Faust? —

> "Wer will was Lebendig's erkennen und beschreiben,
> Sucht erst den Geist herauszutreiben,
> Dann nat er die Theile in seiner Hand,
> Fehlt leider nur das geistige Band."

That is: —

> "Whosoever wants to study and to describe something living,
> He first tries to drive out the spirit,
> Then he has the parts in his hand,
> But alas! the spiritual band is wanting."

We might justly ask, Is not the soul as much as the body? Is not the latter subservient to the former as much as inverse? Is, therefore, the psychical organization of animals not as much worthy the human inquiry as the bodily? Is not an animal more than a plant? Wherefrom, then, this utter disregard of animal psychology, while the number of zoölogists is annually increasing largely? It will be found useful to discuss, in a few words, the probable causes of this phenomenon.

The first and greatest cause lies certainly in the very nature of this science. The student of zoölogy, when entering into the sphere of the psychical life of an animal, will soon learn how difficult it is not to apply at once human notions to animal actions. Man is very apt to refer every thing that is subject to his consideration at once to man as a standard. Just as we are so easily misled on one hand to make of God a potenced man, by simply raising the human, and particularly our own individual, faculties into the infinite,* in the same way many a philosopher thought and thinks, that the soul of an animal is nothing else but a human soul cut short in some respects. I have quite recently heard the remark, that the soul of a dog or monkey is like that of man, only it lacks imagination. There is certainly truth in this idea, but it is expressed in an onesidedly human way. There is no such a thing as a lacking of any psychical faculty in any animal. Every animal's soul is as whole and perfect as that of man, but whole and perfect only in its own sphere. If we would represent the horizon of psychical life of man by one circle, and that of the orang by another, then the latter does not lie as a smaller circle inside of that of man, thus covering some parts of the latter, and others not; but it lies entirely outside of it as an independent and different circle for itself; and the object of animal psychology is precisely this, to trace that whole circle, that whole sphere of psychical life of each species of animals, without respect to the psychical life of any other animal, and particularly of man. Then only when the whole circle is traced, is it time to compare. On the contrary, whenever we select one psychical faculty, and from that alone start a comparison between different animals, we are sure to commit errors in regard to the psychical gradation; for one psychical faculty may be much higher developed in one animal than in the other, when yet the psychical sphere of the former as a whole is far inferior. I will only remind of the sense of vision of an eagle, or of that of smelling of a dog, when compared with either of these senses in man.

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* "As the man, such is his God." — *Gœthe.*

Besides this first and greatest difficulty there are some minor causes, which may have kept back naturalists from the study of the soul of animals. I will mention, secondly, that many a sober observer will naturally dislike the anecdotical character with which this science unluckily has been stamped, first by a biased popular mythology, but afterwards even by some celebrated naturalists. I allude here particularly to the great French count and naturalist, Buffon, who, partly influenced by ancient popular traditions, but mainly guided by his own brilliant phantasy and endowed with a still more sparkling and popular style of writing, has become the author of, or the authority for, most of those current anecdotical and half true, half fabulous characteristics of a great number of the higher animals, especially mammalia, and has impressed his conceptions so strongly upon the people at large in the whole civilized world, that it has become nearly impossible to eradicate them. The incredible engineering endowments of the beaver, the awful cruelty of the hyæna, the magnanimity of the lion, and many other fables, printed and reprinted in the school-books since half a century, date mainly from that source.

A third cause why the science in question has been less cultivated than the rest of zoölogy seems to lie in the circumstance that naturalists in general see only few animals alive, and it is of course only in the living animal that its psychical life can be well studied. But why does the naturalist not study at least the souls of the animals of his own country? The answer, I think, is simply this: Because he is from his boyhood accustomed to see them, and because it is just the phenomena of their psychical life, for instance their locomotion, that he sees always and everywhere. But think of a man who had never seen a fish swimming or a bird flying, would he not first inquire by what means they swim and fly? Yet, strange to say, these are questions which are by no means so well understood as one might suppose.* When the young student of nature arrives at the age where he inquires into the why, how, and wherefrom, he is most attracted by the structure, physiology, and embryology of the animals of his home, for these features are entirely new to him; or else he will satisfy his curiosity by comparing foreign animal forms with those of his own land, and work assiduously over the dried and alcoholic specimens of the collections. In either case there is little chance for psychological studies.

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* We were astonished to find how little there is really known of the locomotion even of higher animals, and which are not at all rare. During half a year's sojourn

We will now try to bring out in short the fundamental principles, on which a methodic study of the soul of animals can be based.

1. The true difference between plants and animals consists in this, that the animals are conscious of an outer world, while the plants are not.

We are used to distinguish the animals from the plants by their being endowed with free, that is, voluntary locomotion, and with feeling. Already Linnæus says: "*Taxa creseunt, Vegetabilia creseunt et vivunt, Animalia creseunt, vivunt et sentiunt,*" that is, Crystals grow, Plants grow and live, Animals grow, live, and feel. A certain kind of feeling, however, cannot be denied to some plants, for instance to the mimosa; and as to the voluntary motion which modern handbooks generally consider as the standard difference between plants and animals, I will show in the following example that the term "voluntary" would be more correctly exchanged for "conscious of an outer world." In a drop of water taken from a fresh water ditch in midsummer, we meet, among many others, two kinds of minute living forms, one called the Monas and another the Navicula. The former, the monas, is a simple globular body, the other, the navicula, is boat-shaped and has a siliceous cover. Both of them we see moving on in the water; but

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in Hayti (West Indies), I met with a novelty in the locomotion of certain fishes entirely unknown before. I found that the Lophobranchious fishes (sea-needle, sea-horse, and the like) do not move by horizontal bendings of the trunk and tail, as fishes generally do, but with the body entirely stiff, simply by undulations of their dorsal and caudal fins. They use their fins as a screw-steamer does her screw; the whole undulating fin being in fact nothing else than the spiral of the screw developed into an undulating line. When returning from Hayti (June, 1857), I communicated this observation to Prof. Agassiz, and he has since observed the same mode of locomotion in the file-fishes; and Prof. Jeffries Wyman tells me that he has seen the same in eels.

while the monas avoids and dodges every obstacle that comes in its way, the navicula, on the contrary, in its steady, onward course runs foul of every thing that comes in its way. For this difference the motion of the monas is called voluntary, that of the navicula involuntary; and the former called an animal, the latter a plant; but we think that the truth is expressed more fully when we say, the monas is conscious of an outer world, of other beings and things around it, and therefore it is an animal, while the navicula is not, and therefore, though being endowed with locomotion, is a plant.

2. The consciousness of an outer world is the fundamental principle of the soul of animals.

3. The consciousness of self, of *I*, which is rather obscure even in the highest animals, as it is also in the human child,* is proportionate to the consciousness of an outer world; it is a result of the latter, for it is only in opposition to an outer world that the *I* conceives itself.

4. The degree of psychical development in different kinds of animals can be judged from the degree of development of that consciousness of an outer world. The soul of an animal is the higher, the more relations it has to an outer world, that is, the larger the horizon of its outer world is.

I will explain this with some examples, taken from lower animals, in which the psychical life is more simple, and therefore clearer to understand.

What is the outer world of a coralpolyp? With hundreds of its kind it lives on the same coral stock; there it is fixed, and is able only to move mouth and tentacles; thus it awaits its prey, a little crawfish; without eyes, and without touching it, by a sense unknown to us, it notices this prey, throws out its lasso-cells and catches it. Each individual has both senses united. Though closely crowded together, I never could notice a trace of psychical relation between the different polyps of the same stock. Now what is the outer world of such a polyp? Its psyche, its consciousness, is confined to the objects of *food*.

Now let us rise one step higher, to a helminth, an *ascaris* for instance, living in the intestine of some vertebrate animal. There it feeds upon the passing juices; the whole object of its life thus far would be the same as in the polyp. But in these helminthes the sexes are divided, and at certain periods the male seeks the female. There is therefore in this helminth a relation between individuals; the outer world does no more consist merely in objects of food as in the polyp, but there is a consciousness of other individual, living beings.

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* Children usually do not say "I" before the third year.

We will take, as a third step of psychical development, a bee or a wasp, or any other of the social hymenoptera. In the bee the consciousness of other individuals is no more confined merely to the season and to the instinct of reproduction, but to the whole life. At any time the individuals of the bee-hive know each other, give each other signs, help each other, fight for each other. It is evident how much more varied are the relations to the outer world, how much more extensive the latter is for a bee than for an ascaris, and still more than for a polyp.

5. In order to judge how extensive the outer world is, of which an animal is conscious, that is, in order to judge about its psychical horizon, we must investigate the organs of that consciousness, that is, the psychical organs of animals.

6. The psychical organs of animals are of three kinds: (1.) Receptive organs, organs which receive impressions from the outer world; here belongs the whole skin-system, including the senses. (2.) Reflective organs, that is, organs which combine the impressions received by the receptive organs. Here belongs the central nervous system. (3.) Reactive organs, that is, organs which react upon the outer world; they are the servants of the central nervous system, which go from within outward, while the receptive organs go from without inward. These reactive organs consist in the whole system of voluntary muscles, with the skeleton, as far as it belongs to them.

7. The student of animal psychology has mainly to depend upon the third kind of organs, namely, the reactive, not only because the functions of the receptive and reflective organs are more or less hidden, but also because the functions of the reactive organs are in fact the mirror of the whole psychical life of the animal, being also the resultants of the functions of the receptive and reflective organs.

8. The functions of the reactive organs are the voluntary motions. When observing these motions in an animal more closely, we soon per-

ceive two kinds of motions, which are in their end entirely different.*

Let us look at a dog. We see in the first place, that it makes many motions, which have no other purpose than to satisfy the eye of the dog itself. Thus the motions by which it eats, drinks, &c. These motions we will call *subjective*, as having reference exclusively to the eye, to the subject of the dog itself. But besides these, we see other motions in the dog, which have no immediate reference to the eye of the dog, but to other dogs, or to man; we see motions of the head, the eyelids, the tail, of the whole body, by which the dog wants to show to other dogs, or to his master, what it thinks, feels, or wants. This second kind of motions I propose to call *sympathetic* motions.

9. The subjective motions are common to all animals, and must be so. We have seen them in the polyp, and we see them in man. They are, generally speaking, the same throughout the animal kingdom. But the greatest diversity exists in regard to the sympathetic motions with different animals, and it will be evident, from the following illustration, that the degree of their development is the principal standard for the student of animal psychology. The more the organs for sympathetic motions are developed, the more extensive is the outer world of which the animal is conscious, and the larger is its psychical horizon. Let us compare a fish, a lizard, a monkey, and finally man, in regard to the organs for sympathetic motions. The fish, lying horizontally in the water, its head, neck, trunk, and tail forming one continuous massy body; its eyes cold and stiff, turned sidewards, nearly immovable; no voice; hardly traces of an ear, — what organs has this animal to show to its fellow-creatures the processes of its soul? How different a spectacle offers a lizard to the thinking observer! Its body raised upon four legs; a distinct neck, upon which the head plays freely, thus giving at once to the eyes a horizon towards the sides not only, but also upwards and downwards. And how expressive are those eyes! their expression mainly lies in the play of the eyelids (of which the fish is destitute), so that from the eyelids alone a versed observer will per-

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* More particulars about this we have given in the Proceedings of the Boston Society of Natural History, Vol. VI.

ceive whether the lizard is contented or sad or enraged. The tongue, which in the fish is a mere organ for swallowing food, is in the lizard a true organ of sympathetic motions, for we often can see them licking each other in play or in love. The ear is well developed; they like music, and some of them have a voice, as those well know who have spent a night in a virgin forest of the tropics. I will not dwell upon the intermediate degrees of psychical organization as exhibited in birds and the lower mammalia, but will now immediately consider the monkey. How rich at once the organization for sympathetic motions! The front legs (in the lizard mere locomotory organs) are in the monkey arms, with which the mother embraces the young. The foot, a mere organ of support in the lizard, has become a hand, with which he grasps the hand of his mate. The lips, of which there is no trace in the lizard, are in the monkey very perfect organs of sympathetic motions. With the lips and the whole play of the muscles of the face, with the eyelid, with the tongue, with sounds, &c., the monkey shows to its fellow-creatures what it likes and what it hates, what it wants and what it thinks. Finally, let us consider man. The natural position of the monkey is on four legs; in consequence, his head is naturally half bent downwards, thus confining the horizon of his eyes, and his front legs, though used as arms, are at the same time still organs of locomotion, mainly of climbing. On the contrary, man, standing upright on his legs, has his arms and hands free; they are perfect organs of sympathetic motions, locomotion being confined to the lower extremities. His head stands free upon the neck, thus giving to all the senses, and particularly to the eyes, the largest possible horizon. His eye is the mirror of his soul, in which the fellow-man reads his innermost thoughts and feelings. His lips, tongue, and the whole apparatus of the larynx, produce by their motion the most perfect of all sympathetic motions, the language. These, and many more not less interesting points, are suggested by a comparison of the organs for sympathetic motions, and particularly also principles of practical importance for educational purposes may be derived, but what I mentioned is sufficient as argument; that owing to the great perfection of organs for sympathetic motions, the relations of man to his fellow-beings are by far the most diversified, and at the same time the most intimate, but not only to his fellow-beings, but to the outer world generally. Whatever our civilization has performed, has been done by improving those natural psychical organs; the outer world of the polyp is confined to the objects of its prey; the outer world of the civilized man is the Universe. Our steam vehicles, on land and on sea, what else are they than improved organs of locomotion? Our letters, our books, our journals, our telegraphs, what else are they than organs of human language on a more extensive scale? Our telescopes, our microscopes, what else are they than the receptive sense of the eye extended? Thus all the inventions of our civilization tend to enlarge the horizon of the individual man. And this is the true destiny of man. I do not know of a greater motto or life-principle than that which was written on the temple of the oracle of Delphi in ancient Greece, *Γνῶϑι Οεαυτόν*, "Know thyself;" but another is equally great, written by Wilhelm von Humboldt, the great philologist (brother of the author of the Cosmos), it is this: "I wish to leave when dying as little as possible behind me in this world, with what I have not come in contact;" that is, what I have not mastered with my mind. Humboldt wanted the most perfect knowledge of the outer world, while the Greek philosopher wanted the deepest knowledge of himself. One of these sentiments is only the reverse of the other, or rather it follows immediately from the other. The most thorough knowledge of the outside world involves the deepest insight in ourselves, just as in morals, he who loves his neighbor the truest is the happiest, and thus loves himself the truest.

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3. ON A NEW SOURCE OF ORGANIC HEAT. By G. HABICH.

[Communicated by D. F. WEINLAND.]

IT is a general doctrine of organic chemistry, that every decomposition of sugar, by the process of alcoholic fermentation, is accompanied by a production of heat. But what agent produces the heat has never been established.

The solution of this problem seems to me to be nearly approached by the results of a number of experiments, lately made by a friend of mine, G. Habich, of Roxbury, Mass.

In a solution of *pure* cane-sugar, which did not contain any proteinous substance, he started the alcoholic fermentation by adding yeast (which latter, as is well known, consists of microscopic unicellular plants, the so-called yeast cells). Fermentation took place, but no heat was developed, nor were new yeast-cells formed. On the other hand, whenever he started fermentation in wort, or any other solution, which contained proteinous substances, then always heat was developed, and the yeast-cells propagated largely, and the production of heat was proportionate to the amount of new yeast-cells formed. From this it appears conclusive, that it is the formation of yeast-cells (and not fermentation generally) which is accompanied by the development of heat.

As an explanation of the experiments related above, we may add that all organic cells, being partly built up of proteinous substances (albumen), cannot be formed in a pure sugar solution destitute of these substances.

The importance of the results of Mr. Habich's experiments from a physiological point of view is striking, though it remains to be ascertained how far the law holds true throughout the organic world.

We know well that respiration alone can hardly account for the organic heat constantly produced in the living animal organism. Now, since there is always an extensive new-formation of cells going on in the latter, it seems natural, from Mr. Habich's experiments, to infer that a part of the heat developed is owing to this formation of cells. The development of heat which accompanies the flowering and the germination of plants might also be accounted for in this way, an extensive formation of cells taking place in both of those cases.

Mr. Habich will soon publish his observations in full, with extensive tabulæ, either in an American or in a German scientific journal.

C. PRACTICAL SCIENCE.

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1. Grape Culture in Missouri. By G. C. Swallow, of Columbia, Missouri.

There is perhaps no department of husbandry in which cultivators find so much difficulty, and meet with so many failures, as in the cultivation of the vine; and yet, while some fail, it is equally true that others meet with eminent success. It is quite obvious, that the most of those who have failed in their efforts must attribute their failures to the want of adaptation, in their modes of culture, to the habits and wants of the vine; as others, on the same soils and under the same suns, have been most successful.

Notwithstanding the true principles of grape culture are so little understood by the community at large, no department of agriculture has been more carefully investigated, more distinctly defined and reduced to scientific principles. Since Virgil wrote his masterly treatise upon the habits and cultivation of the vine, the principles which should govern its culture have been within the reach of all who would investigate the structure of this plant, and learn the soil and climate adapted to its perfect development. And, indeed, it could scarcely be otherwise, as the vine has occupied so prominent a position in the husbandry of almost all the enlightened nations of ancient and modern times. Since Noah planted a vineyard, the vine has followed the progress of husbandry and civilization throughout India, Arabia, Palestine, and Southern Europe. It holds an important place in the history of those seats of ancient civilization and progress. The "vine-clad hill" occupied a conspicuous position in every landscape, and the juice of the grape had its place at the social board, and ruled the joys of the banquet hall. While it held so important a position among the nations, its value led the ablest minds to investigate its habits and deduce the best modes of culture from the experience of the many engaged in the pleasant pursuit. Solomon investigated the properties of the vine, and Virgil gave so excellent a treatise upon its habits and culture, that the investigations and experience of the last two thousand years have added but little to the knowledge then possessed. Since, then, the habits of the vine and the modes of culture best adapted to it have been so carefully determined, and so thoroughly established by the experience of the last four thousand years, it only remains for the cultivators of our times to investigate the modes of culture so long and so successfully practised in India and the countries bordering upon the Mediterranean; to inquire how far the varieties there cultivated, and the culture there adopted, will succeed in other localities; to determine whether some new varieties may not succeed better in other climates and soils; and what modifications of culture will secure the highest degree of success in the various soils and climates to which we would introduce the vine.

It is obvious that the success of the grape depends upon the mutual adaptation of both soil and climate. In places where the soil has all the requisite properties, the climate may be such as to prevent full success; as in many parts of New England, where the climate is too cold, and in England, where it is too moist. In many localities in Southern Europe the soil is such as to prevent the full success of the vine, though the climate is all that could be desired.

Soil. — According to Virgil,* and the best authors who have followed him, the soil should be *warm, light, dry, and rich in alkalies* and *alkaline earths* — especially *potash, soda, lime,* and *magnesia.* The best vines have been grown † upon soils of this description, and when any of these qualities have been wanting, the most skilful vine-growers have carefully supplied them by artificial means. Hence Virgil directs to place "porous stones and rough shells" in the trenches, — the stones and shells to loosen the soil and perfect the drainage, and the shells to supply the defect of lime. The vine has ever succeeded the best, other things being equal, in a calcareous soil. The best vineyards, upon the Rhine, the Ohio, and the Missouri, are upon soils rich in lime; and according to D'Orbigny, the wines from such in France are more lively and spirituous.

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* Geor. Lib. II. lines 217–221 and 262, "*Optima patri arva solo.*"

† The great vine at Windsor Park was planted about fifty years ago. "In 1850," says Prof. Lindley, "it produced 2,000 large bunches of magnificent grapes, filled a house 138 feet long and sixteen feet wide, and had a stem two feet nine inches in circumference. The border in which it grows is *warm, light, dry,* and *shallow.*"

The chemical composition of a plant also gives us sure indications of the mineral ingredients of the soil required for its perfect development. The following table, from Johnston's Agricultural Chemistry, contains the compositions of five vines grown on five different soils. The result shows most conclusively what mineral substances are demanded for the perfection of the vine.

	By Leibfrauen.	By Weinsheimer.	Primary Rocks. Gratz.	Mountain Limestone. Gratz.	Mica Slate.	Mean.
Potash . . .	17.32	25.24	34.13	24.93	26.41	25.60
Soda . .	28.50	2.74	8.03	7.31	8.79	11.07
Lime . . .	29.75	40.75	32.67	37.59	33.47	34.85
Magnesia . .	9.78	7.47	4.66	7.12	9.16	7.64
Oxide of iron .	4.12	1.52	0.16	0.24	0.19	1.25
Phosphoric acid*	5.20	18.87	16.35	19.55	16.87	15.37
Sulphuric acid .	1.96	2.88	2.16	2.37	2.44	2.36
Chlorine . .	1.82	0.53	0.50	0.35	0.25	0.68
Silica . . .	1.55		1.45	0.62	2.48	1.22
Total .	100.	100.	100.11	100.08	100.06	100.04
Percentage of ashes in dry twigs .	2.885	2.689	2.525	2.25	2.325	2.525

These analyses show that potash, soda, lime, magnesia, and phosphoric acid enter largely into the composition of the vine, and that grapes will succeed best on soils rich in those materials. The other ingredients are such as are found in nearly all soils, and may be left out of our investigations.

It is a well-established principle of vegetable science, that *lime* may supply the place of *soda* and *potash*, in part at least, in some plants. The following analyses of vines from two localities show this to be true of the vine also:

	I.	II.
Alkalies . . .	45.82 . . .	27.98
Lime . . .	29.75 . . .	40.75

If, therefore, soda and potash be deficient in soils their place may be partially supplied by lime, should it exist in sufficient quantities.

Climate. — The success of the grape on the islands and the shores of the Mediterranean shows their adaptation to a climate in which the winters are short and mild, and the summers are temperate and equable. In the Ionian Islands, where the grape attains great perfection, it is never exposed to pinching cold or burning heat, or to any very sudden changes from one to the other. But the great profusion and excellence of the grapes in India, at Candahar and Cabul, "the sunny home of the grape," indicate an ability to reach perfection in spite of sudden changes from extreme cold to burning heat. "In no part of the world," says Lindley, "are the grapes more delicious than in Candahar and Cabul"; and yet the traveller speaks of the *bitter cold wind*, and *blazing fires at night*, and the *burning sun* by day, in March; and the sun's heat at 140° in May, where the grapes ripen as early as June.

We may conclude, then, that the grape will, under favorable circumstances, reach the greatest perfection, though exposed to sudden changes, and extremes of heat and cold.

Having ascertained the conditions of soil and climate best adapted to the successful culture of the vine, it has been my aim, during the progress of the geological survey of Missouri, to determine how far these conditions are fulfilled in Missouri; to what extent and with what success the vine may be cultivated in our State; and the advantages to be derived from its cultivation. In order to secure the most accurate data for our conclusions, our investigations have been directed to the following subjects: —

1. The characters and habits of all our native vines, and the soils on which they succeed best, have been carefully noted.

2. Five persons* have been employed to make meteorological observations. One at Springfield in the south-west, one at Cape Girardeau

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* It gives me great pleasure to bear testimony to the disinterested labors of those who have so faithfully observed and recorded the meteorological phenomena at the stations above named. Our State will be under many obligations to the Rev. G. P. Comings, of St. Paul's College, Palmyra; Rev. James Knoud, of St. Vincent's College, Cape Girardeau; J. A. Stephens, Esq., Springfield; E. B. Neely, A. M., of the St. Joseph High School; and Miss M. B. Hill, at Columbia, who have made the observations at their several localities.

in the south-east, one at Palmyra in the north-east, one at St. Joseph in the north-west, and one at Columbia in the centre, in the valley of the Missouri River. These observers have been supplied with the very best instruments, and they have made and recorded their observations according to the plan adopted by the Smithsonian Institution.

3. The experience of our most successful vine-growers has been collected, and the results carefully compared with the conclusions derived from our examinations of the climate, soils, and wild vines of the State.

4. The soils of the State have been carefully observed, and the varieties collected and submitted to a most skilful chemist for full and accurate analyses.

Native Grapes. — The growth and fruit of our native vines give us most important indications of the adaptation of our soil and climate to the cultivation of the grape. The following species have been observed; the growth, habits, and fruit of each variety have been carefully examined.

1. VITIS LABRUSCA, *Linn.* *Fox Grape* of the Northern States.

This vine is abundant in all parts of the State. It attains to a very large size* in our rich alluvial bottoms and on our best upland soils; but the vines of a smaller size, which are found on the poorest soils in the State, produce much the best grapes. Thóse which grow upon the dry ridges, on the declivities of the bluffs (especially those of the magnesian limestone), and on the talus of debris at their bases, exhibit a healthy, firm growth, and produce an abundance of fine fruit. The grapes found in these localities are larger, and the pulp is more juicy and palatable.

Many well-known and excellent varieties of grapes now in cultivation were derived from this species. The *Isabella*, *Catawba*, *Schuylkill*, and *Bland's* are the most esteemed.

2. VITIS ÆSTIVALIS, *Michx.* *Summer Grape.*

This, like the preceding, is found in all parts of the State, and is doubtless the largest of all our vines. It is one of the most striking objects in our magnificent forests. While the stem, like a huge cable, hangs suspended from the limbs of the largest trees, the branches clothed in rich foliage, and often loaded with fruit, hang in graceful festoons over the highest boughs. But the vines growing on the thin soils of our limestone ridges and bluffs, and on the loose debris at their bases, where they are more exposed to the air and the sun, produce a greater abundance of the best fruit.

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* This vine often attains a diameter of ten inches, ascends the loftiest trees, and spreads its branches over their highest boughs.

3. VITIS CORDIFOLIA, *Michx.* *Winter* or *Frost Grape.*

This vine is widely diffused through the State; but it is not so large as the Fox or the Summer Grape. Its fruit is small and acerb.

4. VITIS RIPARIA, *Michx.* *River Grape.*

This grape is partial to the alluvial soil along the margins of our streams. It grows to a large size.

5. VITIS VULPINA, *Linn.* *Muscadine* of the West, and *Fox Grape,* according to Elliott, in the South-eastern States.

It is most abundant in the southern part of the State. It grows very large and produces abundantly. Its fruit is very much esteemed. The cultivated *Scuppernong Grape* is a variety from this species.

6. VITIS BIPINNATA, *Michx.*

This plant was observed in Cape Girardeau and Pemiscot counties.

7. VITIS INDIVISA, *Willd.*

This vine abounds in the central and western counties.

From this list it will be seen that Missouri possesses all the native grapes of our country save one, the *Vitis Caribæa?* (D. C.) of California. The vines are so abundant and so large as to form an important and conspicuous part of every copse and thicket throughout the entire State. They are everywhere present, lending grace and beauty to every landscape, and indicating with prophetic certainty that the day is not far distant when the purple vineyards will cover our hills, the song of the vine-dresser fill the land with joy, and the generous juice of the grape will improve our moral, intellectual, and physical powers.

Experience of our Vine-dressers.[*] — Several vine-dressers in our State have been engaged in the cultivation of the grape during the last twelve or fourteen years. Their success has been fully equal to their expectations; and they are full of high hopes of the most useful and profitable results, even of entire and *permanent success*. Their experience in cultivating the vine has led them to the same conclusion that we have deduced from our scientific examinations of the soil, climate, and native vines; namely, *that the vine can be cultivated with entire success, in favorable localities, in all parts of the State.*

It should be borne in mind that these results have been derived mostly from vineyards in the valleys of the Missouri and Mississippi Rivers, which are not, by far, the most favorable localities in the State; for the "mildew" and the "rot," the most formidable obstacles they have had to contend with, may be partially or entirely obviated in localities where the atmosphere and soil are not so densely charged with moisture. "The *rot*," says one of our most successful vine-dressers, Mr. Haas, "attacks the berries when the soil is in a wet condition, in July and August." "It is most severe on the low and wet parts of the vineyard." Mr. Husmann says, "the principal cause, all are agreed, is an excess of moisture about the roots, and damp, moist weather." Now the larger part of our vineyards are located upon a *stiff, cold, clayey subsoil*, which of necessity retains the excess of moisture and produces the injurious results.[†] This evil may be obviated by thorough draining and preparation of the soil; or, what is better, by selecting some of the millions of acres in the southern part of the State, where the soil is *warmer* and *lighter* and richer in the ingredients most favorable to the vine, and where the subsoil is so porous as to permit a free passage to the excess of moisture.

The *mildew* appears in June; and all agree that it is caused by *foggy, damp,* and *hot weather* accompanied by *mists*, which is much more prevalent in the valleys of our large rivers than on the table-lands of the south.

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[*] I am indebted to Mr. Wm. Haas, of Boonville, Mr. Geo. Husmann, of Hermann, Mr. Frederic Munch, of Marthasville, and Mr. Joseph Stuby, of Hamburg, for valuable information respecting the cultivation of grapes in our State.

[†] See soil No. 12, page 12.

The characters of the two regions under comparison show most conclusively that the excess of moisture in the valleys must be considerable and permanent. These valleys are covered with numerous and extensive lakes and sloughs, and forests of rank growth and vast extent, besides the broad rivers which flow through them; while the table-lands are almost destitute of lakes and ponds, and but partially covered by a very sparse and much less vigorous growth of timber. And, besides, they occupy an elevation of several hundred feet above the valleys.

No fears, therefore, need be entertained that these obstacles will prevent the entire success of vine-culture in Missouri, should our atmosphere even continue as moist as at present. But we may expect much improvement in this respect, as it is fully established by past experience, that the settlement of a country and the opening of a soil to cultivation lessen the amount of rain and moisture in the atmosphere.

Notwithstanding the many difficulties our vine-dressers have had to contend with, and notwithstanding some of their vineyards are not, to say the least, in the most favorable localities in the State, their success has been very flattering.

The vineyards of Boonville have yielded the present season about 6,000 gallons, worth $12,000. Five acres gave a clear profit of $2,000, or $400 per acre. Mr. Haas made 1,550 gallons from three acres.

The vintage of Hermann was about 100,000 gallons, from less than 200 acres. At one dollar per gallon, which is less than the value, it will give a profit of at least $400 per acre, or of $80,000 on the 200 acres in cultivation. One small vineyard at Hamburg, Mr. Joseph Stuby's, yielded over 1,000 gallons per acre.

The entire cost of vineyards, preparing the soil, setting and training the vines till they come into bearing, varies from $200 to $300 per acre; annual cost of cultivation after, fifty dollars to sixty dollars per acre; ten per cent. on first cost, $20 to $30 per acre; total expense for each year, $70 to $90 per acre. So that an income of $100 per annum for each acre is sufficient to pay the interest on the first cost and the expense of cultivation.

Judging from the statistics before me, I would suppose all our vineyards have yielded an average of at least 250 gallons per acre since 1849, which, at an average price per gallon of $1.60, would give an annual income of $400, and a yearly profit of $300 per acre. So that the vinedresser, even in the poorest seasons, can scarcely fail of a handsome profit;

while in good years his gains will far surpass those derived from any other department of husbandry. But the profits of our most successful cultivators have been much greater. M. Pœschel, of Hermann, is said to have made over 400 gallons per acre for the last ten years, and an annual profit of more than $500 for each acre.

Such are the favorable results legitimately derived from the experience of our vine-dressers, in their early efforts in a new country, with a soil and climate unknown to the cultivators of the grape. All must admit that they are most satisfactory. Even if our climate does not become more dry, if no more improvements are made in the modes of culture, and if no more favorable localities are obtained, grape culture must increase very rapidly, and become an important element in our agricultural and commercial interests.

Climate. — It will be impossible to give, in the few pages allotted me in this communication, the results of our meteorological observations. It must suffice to state in general terms, that the extremes of heat and cold are not so great as in some of the best grape-growing regions; and that the atmosphere in the southern part of the State is sufficiently dry. The results, in short, present but one very objectionable feature. There are occasional changes of temperature so great and sudden as to prove somewhat injurious to the grape at certain stages of its growth. But it should be observed, that these changes are not so marked in the high table-lands of the south and west as in the north and in the valleys of the Missouri and the Mississippi, where our vineyards are located; and, even where most objectionable, they are not so great as in India, and other grape-growing districts of the old world.

That portion of Southern Missouri extending from Newton county in the south-west to Ste. Genevieve in the south-east, usually represented as the eastern extremity of the Ozark mountains, is in fact a table-land varying from 1,000 to 1,500 feet above the ocean. In the west it is sufficiently undulating to be well drained, while in the east it sometimes rises into ridges and knobs of moderate elevation. From this table-land the country descends by moderate slopes in every direction. On the northern slope are the head-waters of the Sac, Pomme de Terre, Niangua, and Gasconade, flowing into the Missouri; on the east, the Meramec and the Big, flowing into the Mississippi; on the south, the waters of the St. Francis, the Current, and the White with its tributaries, descending towards Arkansas; and Spring River and Shoal Creek on the western slope.

The valleys of the numerous streams which flow from this table-land are at first but little depressed below the general level; but the further they descend the deeper and wider they become, until they expand into broad alluvial bottoms, bounded by bluffs more or less precipitous. The fountains are numerous, bold, and pure; the streams clear and rapid.

The surface of these table-lands is undulating, with no mountains or arid plains to disturb the equable and agreeable temperature which usually prevails at that elevation under the 37th parallel of north latitude. There are no swamps or overflowed lands, from which vapors and noxious exhalations can arise to render the air damp and unhealthy. As these facts plainly indicate, the summers are long, temperate, dry, and salubrious,* and the winters short and mild. The country possesses the clear, brilliant skies of Italy, and the dry, bracing air of the western prairies.

Soil. — Nearly all the soils of Missouri possess all the ingredients necessary to the complete development of the vine; but some of them are too heavy, wet, and cold, unless improved by artificial means. This is true to some extent of those on the bluffs of the Mississippi and Missouri, where nearly all the vineyards of our State are located. These soils are based upon the Bluff formation, where it contains more clay and less lime than in the western counties, which possess our best soils.

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* According to the census report of 1850, this is one of the most healthy regions in the country.

Analyses of Soil from the Bluffs of Boone county, by Dr. Litton.

	No. 12 A.	No. 12 B.	No. 12 C.
Water expelled by drying at 150° C.	0.4105	0.6558	0.8030
Organic matter and water not expelled at 150° C.	3.0957	2.6049	3.8901
Silica, etc., insoluble in hydrochloric acid	90.1420	90.8063	85.0571
Soluble silica	0.1384	0.1475	0.2187
Alumina	3.0654	2.9346	4.7672
Peroxide of iron	2.0553	2.0590	3.8814
Oxide of manganese	a trace	a trace	a trace
Lime	0.2086	0.1242	0.4722
Magnesia	0.3423	0.2088	0.6581
Potash	0.3368	0.2121	0.3895
Soda	0.1828	0.2925	0.1220
Phosphoric acid	0.0560	0.0346	0.0556
Sulphuric acid	0.0035	0.0508	0.0099
Chlorine	0.0000	0.0000	0.0276
Total	100.0373	100.1311	100.3524

No. 12 A was collected from 2 to 6 inches below the surface; No. 12 B, from 10 to 12; and No. 12 C, from 18 to 20 below the surface, on a high ridge.

This soil is very similar to those upon which the vineyards of Boonville, Hermann, and Hamburg, are located; and it produced an abundance of large and excellent grapes, on small vines of the *Vitis labrusca.* The superior native grapes growing upon this soil, and the success of the vineyards above-named, prove its adaptation to the vine. Its greatest defect is a capacity to hold and retain an excess of water; which must be remedied by trenching and a proper admixture of vegetable matter, sand, pebbles, and broken limestone. This labor, however, may be avoided by selecting some of the millions of acres in Southern and Central Missouri, the soils of which are already prepared, as if by design, to invite the vine-dresser to possess and cultivate them.

Analysis of a Magnesian Limestone Soil from the Southern bluffs of Callaway county, by Dr. Litton. Soil No. 14.

Water expelled by heating to 150° C.	1.1700
Organic matter and water not driven off at 150° C.	9.6299
Silica, etc., insoluble in hydrochloric acid	54.2600
Soluble silica	0.1639
Alumina	10.8588
Peroxide of iron	2.5186
Manganese	a trace
Lime	8.0720

Magnesia	1.6609
Potassa	1.6378
Soda	0.3442
Carbonic acid	10.1111
Sulphuric acid	0.0605
Phosphoric acid	0.0950
Chlorine	0.0053
Total	100.5880

This soil is all that could be desired for the culture of the grape; it contains an abundance of all the mineral substances which enter into the composition of the vine, as shown above by its analysis. While it is *wàrm*, *light*, and *dry*, it contains large quantities of magnesia and vegetable matter or humus, giving it great capacity for absorbing and retaining a sufficient quantity of moisture, even in the droughts of summer. This is a fair representation of the soils on the Magnesian Limestone ridges and slopes throughout Central and Southern Missouri. These slopes and ridges occupy millions of acres now deemed worthless, which are, in fact, by far the most valuable lands in the State for the cultivation of the grape; especially is this true of those located upon the southern highlands, away from the vapors and sudden changes of our large rivers and their broad valleys.

The Magnesian Limestone Series occupies a large portion of Southern Missouri, and is made up of magnesian limestones, sandstones, and porous chert, which are usually overlaid with thin beds of reddish-brown marly clays. The sand, lime, magnesia, and alumina, derived from the decomposition of these rocks, together with the abundance of vegetable matter and the alkalies derived from the fires which annually overrun this country, combine to form a soil* *light*, *dry*, *warm*, and rich in *potash*, *soda*, *lime*, *magnesia*, and all the other mineral ingredients needed to render it fertile, and suitable in an eminent degree for the culture of the vine. In many places this soil is underlaid with a sufficient quantity of pebbles, and fragments of porous chert, to constitute a most thorough system of drainage; while in others the fragments of chert are disseminated through the soil in such quantities as to injure it somewhat for ordinary cultivation, but giving precisely the preparation so highly recommended by Virgil and later authors, and the best cultivators of the grape. It is true that the native vines do not grow so large and sappy on this as on the deep, damp soils of the State; but they are nevertheless strong and healthy, and produce finer clusters of larger and better grapes. This improvement was particularly observed in the *Muscadine*, the *Northern Fox*, and the *Summer Grapes*.

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* See preceding analysis No. 14.

This variety of soil also extends over a large portion of the counties on both sides of the Osage, and over the southern part of Boone, Callaway, Montgomery, and Warren, on the north side of the Missouri, occupying in all an area of some 15,000,000 acres. Of these, at least 5,000,000 acres might be selected in the most desirable localities and devoted to vineyards, without encroaching upon the lands most desirable for other departments of agriculture. And so far as we can judge from the characteristics of soil and climate, and the indications of the native vines, these 5,000,000 acres in the highlands of Southern Missouri present rare inducements to the vine-dresser — such a combination of favorable circumstances as will not fail to attract the attention of those who would engage in this most pleasant and profitable department of husbandry. And so important will be the results, that every effort should be put forth to hasten the time when these 5,000,000* acres shall be covered with flourishing vineyards, giving profitable employment to 2,000,000 people, yielding more than 1,000,000,000 gallons of wine, and an annual profit, at the lowest estimate, of $500,000,000. And what is still more important, the pure nourishing juice of the grape would take the place of the vile, maddening compounds used under the names of wine and brandy; drunkenness would give place to sobriety; and our people, nourished by the grape and its pure wines, would become as robust and hardy as they are now daring and indomitable.

Natural Terraces.—The bluffs of the numerous streams in Southern Missouri, and in the valley of the Osage, usually slope back into knobs and ridges, which are frequently surrounded by numerous natural ter-

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* France has about 5,000,000 acres in vineyards. They yield about 925,000,000 gallons of wine, besides the 95,000,000 gallons distilled into brandy, and give profitable employment to 2,000,000 of people, mostly women and children.

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PLATES OF MAGNESIAN LIMESTONE ON THE NIANGUA 3 MILES N W OF HUNTERS SPRING SHOWING THE NATURAL TERRACES SO COMMON IN THE BLUFFS OF THAT STREAM races so regular and uniform that they appear like the work of human hands, as seen in Plate VIII. These terraces are produced by the decomposition of the strata of magnesian limestones which form the bluffs. Their height varies from one to six feet, and the width of the top from two to twelve, according to the angle of the slope and the height of the terrace. Their surfaces are nearly level, and usually covered with a light, warm, and rich soil, containing fragments of chert and the decomposing limestone, all wonderfully prepared by nature for the planting of vineyards. These terraces generally surround high, open ridges and knobs, exposed to the free circulation of the dry atmosphere of the region under consideration. We have observed but one objection to their use for vineyards. In some places the soil does not appear sufficiently deep to secure the vine against the effects of droughts. But, as an offset to the want of depth, it always contains large proportions of carbonate of magnesia and humus, which give a great capacity for absorbing and retaining moisture; as these substances possess this capacity to a greater degree than any of the other ingredients of our soils. And besides, the thinnest soils on these terraces sustain a vigorous growth of prairie grasses, flowers, shrubs, and vines, which produce the finest quality of grapes in great profusion.

Caves. — There are numerous spacious caves in all parts of this interesting country. The temperature of those measured ranges between 50° and 60° F. Many of them would make most excellent wine cellars, as their temperature is sufficiently low and uniform to prevent that acidity to which the wines of all temperate latitudes are predisposed. It should also be borne in mind that this is the richest mineral region in the Mississippi Valley. It abounds in mines of lead, zinc, copper, cobalt, and mountains of iron and quarries of marble; and, besides, its agricultural resources are sufficient to sustain a population of many millions.

These facts respecting the *native vines*, the *climate*, the *experience of our vine-growers*, and the *soil*, clearly prove the capacity of Missouri to become the great wine-growing region of our continent. They should encourage those noble spirits who have so faithfully devoted their labor and their money to promote this important department of husbandry in our midst; for the time is not far distant when the "*poor flint* ridges" and *terraced slopes* of Southern Missouri will be as valuable for vineyards as some of them are now for their rich mineral deposits. The vine-clad hills of the beautiful Niangua will vie in wealth with the leaden veins of Potosi and Granby; the purple vintage of Arcadia will compete in golden profits with the glowing furnaces of the Iron Mountain.

TITLES

OF

COMMUNICATIONS.*

A. MATHEMATICS AND PHYSICS.

1. RESEARCHES IN ANALYTIC MORPHOLOGY. PART FIRST OF A NEW THEORY OF CURVES. By BENJAMIN PEIRCE.

2. NOTE ON THE ATLANTIC COTIDAL LINES; DERIVED FROM THE EQUILIBRIUM THEORY. By BENJAMIN PEIRCE.

3. THE TIDES OF SATURN'S RINGS. Part I. By BENJAMIN PEIRCE.

4. COMPARISON OF THE COMMITTEE METRE OF THE AMERICAN PHILOSOPHICAL SOCIETY WITH OTHER STANDARDS. By A. D. BACHE and J. E. HILGARD.

5. ON THE ESTABLISHMENT OF A MATHEMATICAL JOURNAL. By J. D. RUNKLE.

6. A PAPER IN ADVOCACY OF THE AMERICAN ASSOCIATION, IN ITS ENDEAVOR TO SET ON FOOT A GREAT SYSTEMATIC CHAIN OF SIMULTANEOUS METEOROLOGICAL OBSERVATIONS THROUGHOUT THE WHOLE OF THE AMERICAN CONTINENT. By R. LACHLAN.

7. NOTE ON CURVATURE. By WILLIAM WATSON.

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* The following papers were also read: of some, no copy has been received for publication; of others, it was voted that the title only should be printed. No notice, even by title, is taken of articles *not approved*.

8. NOTE ON CERTAIN IRIDESCENT PHENOMENA UPON LAKE MICHIGAN. By GEORGE W. BURNAP.

9. CLIMATE OF NORTH AMERICA. By J. B. HURLBURT.

10. ON THE CAUSE OF THE ROTATION AND ORBITAL MOTION OF EXTENSIVE REVOLVING STORMS. By E. B. HUNT.

11. NOTE ON SOME DAGUERREOTYPE IMPRESSIONS OF THE SUN'S IMAGE. By STEPHEN ALEXANDER.

12. ON THE HARMONIES OF THE SOLAR SYSTEM. By STEPHEN ALEXANDER.

13. ON A CRITERION APPLICABLE TO THE QUESTION OF THE COMMON ORIGIN OF THE ASTEROIDS, AND, POSSIBLY, ALSO OF CERTAIN COMETS. By STEPHEN ALEXANDER.

14. REPORT ON ATMOSPHERIC ELECTRICITY. PART I. By JOSEPH HENRY, Secretary of the Smithsonian Institution.

15. OBSERVATIONS MADE AT THE FALLS OF NIAGARA, SEPTEMBER 1ST, 1857. By JOSEPH HENRY.

16. ON THE METEOROLOGY OF MOSUL. By W. F. WILLIAMS.

17. SOME EXPERIMENTS ON SONOROUS FLAME, WITH REMARKS ON THE PRIMARY SOURCE OF THE VIBRATIONS. By WILLIAM B. ROGERS.

18. ON RING-FORMATIONS IN GASES AND LIQUIDS. By WILLIAM B. ROGERS.

19. ON DISTRIBUTION: THE PHYLLOTAXIS DEDUCED FROM THE MATHEMATICAL DEFINITION OF DISTRIBUTION. By CHAUNCEY WRIGHT.

20. ON THE GEOGRAPHICAL DISTRIBUTION OF THE PLACES FROM WHICH THE METEOROLOGICAL OBSERVATIONS ARE REPORTED TO THE SMITHSONIAN INSTITUTION. By JAMES H. COFFIN (in behalf of the Secretary of the Institution).

21. On a Supposed Protracted Perturbation in the Atmospheric Pressure, in the Region of the White Mountains, by an Extensive Conflagration of Forests. By Arnold Guyot.

22. An Observation of the Sonorous Vibrations of Coal Gas Flames. By Alfred M. Mayer.

23. On the Confirmation, from Providence data, of a newly determined Law of Mortality for Early Childhood. By E. B. Elliott.

24. Concerning the number of Telegraphic Conductors that may connect with a single Battery, at an Extreme Station, without Sensible Interference. By E. B. Elliott.

25. On Recent Efforts to Deepen Maffit's Channel across the Bar of Charleston Harbor. By P. N. Lynch.

26. On the Telegraph and the Telegraphic Lines of the World. By Lawrence Turnbull.

27. A Note in Relation to Further Researches on the alleged Influence of Solar Light on the Process of Combustion. By John Leconte.

B. NATURAL HISTORY.

28. On the Insensible Gradation of Words in Comparative Philology, illustrated by Five Charts. By J. P. Lesley.

29. Signs of Significance — like Symbols for a Language — in Fanciful Rock-work — the Stone Spear and Arrow Heads of the Catawbas. By J. H. Gibbon.

30. Distinct Tones and Accents of Voice — with Special Signs — explain Sensible Emotions from Mute Animals to each other. By J. H. Gibbon.

31. CERTAIN IDENTITIES BETWEEN DEAD LANGUAGES AND EXISTING ALPHABETS. By J. H. GIBBON.

32. A METHOD SUGGESTED FOR TEACHING NOTES OF MUSIC TO NEGRO BOYS, NOT INSTRUCTED TO READ. By J. H. GIBBON.

33. MARINE PHOSPHORESCENCE. By H. G. WARNER.

34. ANTIQUITIES AT GRAVE CREEK — INSCRIBED STONE — EXPLORATIONS AT THE WEST. By WILLS DE HASS.

35. OBSERVATIONS ON THE GEOLOGICAL STRUCTURE OF KEY WEST, AND ON A SPECIES OF FICTITIOUS FOSSILS ABOUNDING THERE (WITH SPECIMENS). By E. B. HUNT.

36. ON THE SOLUBILITY OF THE PHOSPHATES USED IN AGRICULTURE, AND THEIR ASSIMILATION BY PLANTS. By A. SNOWDEN PIGGOT, M. D.

37. UPON THE THICKNESS OF SOME METAMORPHIC STRATA IN VERMONT AND MASSACHUSETTS. By CHARLES H. HITCHCOCK.

38. ANALYSIS, CLASSIFICATION, AND REPRESENTATION OF THE SOUNDS OF THE ENGLISH SPOKEN ALPHABET. By W. D. WHITNEY.

39. ON THE CHARACTER, NATURAL RELATIONS, AND RELATIVE RANK OF THE PRIMARY DIVISIONS OF THE VEGETABLE KINGDOM, FOUNDED ON DIFFERENCES OF STRUCTURE; AND ESPECIALLY ON THE TRUE RANK OF THE CLASS OF GYMNOSPERMS. By ARNOLD GUYOT.

40. ON THE TRUE ELEVATION OF THE CULMINATING POINTS OF THE APPALACHIAN SYSTEM IN THE BLACK, THE WHITE, AND THE GREEN MOUNTAINS. By ARNOLD GUYOT.

41. SYNOPSIS OF THE ICHNOLOGY OF NEW ENGLAND. By EDWARD HITCHCOCK.

42. PERMIAN ROCKS OF SOUTHERN ILLINOIS. By A. H. WORTHEN.

43. OBSERVATIONS UPON THE GENUS GRAPTOLITHUS AND ALLIED GENERA. By JAMES HALL.

44. OBSERVATIONS UPON THE GENUS MEGANTERIS AND OTHER PALÆOZOIC BRACHIOPODÆ. By JAMES HALL.

45. NOTICE OF AN ARABIC WORK ON PHYSICAL SCIENCE OF THE TWELFTH CENTURY. By W. D. WHITNEY.

46. FORMATION AND AGE OF BURLINGTON HEIGHTS AT HAMILTON, CANADA WEST. By J. B. HURLBURT.

47. DESCRIPTION OF THE COAL BEDS NEAR FORT BELKNAP, IN TEXAS, WITH THE SUBJACENT AND SUPERINCUMBENT STRATA, AND THE DISCOVERY OF FUSULINA LIMESTONE THERE, IN A COMMUNICATION FROM DR. FRANCIS MOORE TO EDWARD HITCHCOCK.

48. ON THE GENERAL FEATURES IN THE CONFIGURATION OF THE BAYS, SOUNDS, CAPES, ISLANDS, &C. OF THE EASTERN COAST OF THE UNITED STATES, AND ON THEIR CLASSIFICATION. By J. G. KOHL.

49. ON THE PRODUCTION OF LOCAL ANÆSTHESIA, BY A NOVEL APPLICATION OF CURRENT ELECTRICITY. By C. P. WILLIAMS.

50. THE GEOLOGICAL MAP OF PENNSYLVANIA. By H. D. ROGERS.

EXECUTIVE PROCEEDINGS

OF THE

BALTIMORE MEETING, 1858.

HISTORY OF THE MEETING.

The Twelfth Meeting of the American Association for the Advancement of Science was held at Baltimore, Maryland, commencing on Wednesday, April 28, and continuing to Tuesday night, May 4.

The number of names registered in the book of members in attendance on this meeting is one hundred and ninety. Eighty-five new members were chosen, of whom all but twenty-one have already accepted their appointment by paying the assessment, and fifty-two of them signing the constitution. Thirty-four others joined the Association by virtue of Rule 2 or 3, all of them signing the constitution and paying the assessment. Three others paid, without a formal election, or signing the constitution; and one signed the constitution without a formal election, and without paying. One hundred and nine papers were presented, of which most were read; but only a part have been printed. Some were thought unworthy of publication, and, in other cases, copies have not been furnished by the authors.

The sessions of the Association were held in the rooms of the Maryland Institute.

The meeting was called to order by Professor Alexis Caswell, Vice-President of the Montreal meeting, who requested Rev. A. C. Coxe to open the services with prayer, when he addressed the Throne of Grace in the following words:—

"O God of our Fathers, and Lord of Mercy, who hast made all things with Thy word, and ordained man through Thy wisdom that he should have dominion over the creatures which Thou hast made, give us wisdom that sitteth by Thy throne, and reject us not from among Thy children; for though a man be never so perfect among the children of men, yet if Thy wisdom be not with him, he shall be nothing. And wisdom was with Thee when Thou madest the world; O send her out of Thy holy heavens, and from the throne of Thy glory, that being present she may labor with us, and that we may know what is acceptable unto Thee. For we confess that the thoughts of sinful men are miserable, and that our devices are but uncertain, seeing that the corruptible body presseth down the soul that museth on many things. And hardly do we guess aright at things that are upon earth, and with labor do we find the things that are before us; but the things that are in heaven, who can search out, or Thy counsel, who can know except Thou give wisdom, and send the Holy Spirit from above, that so the ways of them that live on earth may be reformed, and men may be taught the things that are well-pleasing unto Thee. Grant us, therefore, to think as we should, and to conceive as is meet, for in Thy hands are both we and our words. Give us also certain knowledge of the things that are; to know how the world was made, and the operation of the elements; the beginning, ending, and midst of the times; the alterations of the turnings of the sun, and the change of seasons; the circuits of years and the positions of stars; the natures of living creatures, the powers of the winds, and the reasonings of men; the diversities of plants and their virtues, and all things that are either secret or manifest, and above all to know Thee, the only living and true God, and Jesus Christ whom thou hast sent. Finally, for his merits and mediation, by whom we receive all things, we bless Thy Holy Name; and through His glorious intercession we beseech thee to grant unto us, and to our country, and to all mankind, whatever Thou seest to be necessary and expedient: saying in the perfect words which He himself hath taught us: Our Father, who art in heaven! Hallowed be thy name. Thy kingdom come; thy will be done, on earth as it is in heaven. Give us this day our daily bread, and forgive us our trespasses, as we forgive those who trespass against us. And lead us not into temptation, but deliver us from all evil. For thine is the kingdom, the power, and the glory, forever, and ever. Amen."

On motion of Prof. A. D. Bache, the Vice-President of the Montreal meeting, Professor Alexis Caswell, was requested to act as President of this meeting.

Mayor Swann was then introduced to the meeting, and welcomed the Association to the city in the following address: —

"GENTLEMEN OF THE CONVENTION: — I meet you here to-day, not in the relation in which your kindness has placed me — as chairman of your local committee — but as chief magistrate of the city of Baltimore, to thank you in the name of our people for the compliment you have paid them by your presence on this occasion. When I look at the character of this Association — its varied and distinguished membership, its past contributions to science, its enlarged preparations for usefulness in the future — it seems to me proper and becoming that I should recognize, with whatever official influence I may command, the dignity and the importance of the labors in which you are embarked here.

"You stand upon the soil of one of the old States of this confederacy; a State whose past history is not without its incidents of proud and stirring reminiscence. It was in Maryland that the Father of his country laid down his commission for those more congenial and peaceful pursuits in which you are engaged, and it was here, in the city of Baltimore, that the last surviving signer of the Declaration of Independence terminated, in the unobtrusive walks of an enlarged benevolence, a career devoted to liberty, and invoked, with his expiring breath, the blessings of Almighty God upon those cherished institutions which he aided so largely in establishing.

"The city of Baltimore occupies what may be called the geographical centre of the eastern confines of our Union. A system of improvements, more stupendous than the works which signalized the bondage of the ancient Egyptians, proclaims its triumphs north and south of the point where you are now standing — stretching away to the waters of the St. Lawrence on the one side, and the more distant shores of the Gulf of Mexico on the other. Looking westward, a spirit of restless enterprise, by an effort hitherto unparalleled in the progress of human labor, has penetrated the valley of the Mississippi, by a work of intercommunication more than one thousand miles in extent; and the day is not distant, nay, I may say the day is now at hand, when you will be called to inaugurate the still grander achievement of a union between the great oceans that wash our eastern and western borders.

"From this favorite point of observation you look out upon a field worthy your noblest efforts. Upon a people whom God has chosen to illustrate the great problem of self-government; upon institutions which have called into being and stimulated the most startling discoveries in science and philosophy; upon a territory boundless in extent, comprising every variety of climate, soil, and production, — in short, upon whatever is attractive and worthy of pursuit within the extended range of mental and physical inquiry.

"The annual meetings of this Association must continue to increase in usefulness and practical benefit to the whole Union. I trust it may go on with each succeeding year to expand wider and wider the circle of its influence. Coming from remote States, urged on and sustained by a common impulse, coengaged in the same great cause of human progress and development, your labors must soon be felt beyond the more immediate sphere in which you move. In the midst of those fierce political conflicts which have sometimes separated State from State, and section from section, you have held in check, by your example of patient labor and friendly coöperation, the threatening asperities of party rancor, and have pointed us to the only reliable source of virtuous and lasting renown.

"Gentlemen, in the name of the city of Baltimore, I again bid you welcome to the city of Baltimore. May your labors be crowned with success, and each returning anniversary of this noble institution add more and more to that national reputation which you have so richly earned, and which has contributed so largely to the credit of our people at home and abroad."

Professor Caswell responded as follows: —

"MR. MAYOR: — It has unexpectedly become my duty to respond to the warm and eloquently expressed welcome which you have addressed to the Convention. I heartily wish it was in my power to clothe that response in the fitting terms which the occasion suggests. I need only say that it needed not the testimony you have given to convince us that the city of Baltimore is deeply interested in all that concerns the arts and sciences, and prepared to cordially welcome here their devotees. The proof of your intelligent appreciation and liberal encouragement of practical scientific and literary pursuits is to be found in your Historical Society, your Maryland Institute, your noble system of public schools, in the prospective beneficence of your Peabody and M'Donogh Institutes, and in that other liberal endowment, which delicacy almost forbids me to mention, the Swann Professorship of Applied Chemistry, from which we are justified in expecting the most useful results. In these organizations you have the means, and I am assured will not fail in the will, to carry on your kindred projects for the promotion of science. The hospitality of your fine city is so renowned that it calls for no commendation from me. Baltimore always gives a welcome to honest men, well-meaning men, of whatever shade of political or religious opinion. We rejoice in your prosperity, and shall rejoice in and enjoy your abundant and liberal hospitality.

"The body of scientific men before you are working men — hard, laborious, working men, developing from the laboratory of thought and from deep research the results that are to advance the condition of all. The science they pursue is not an abstraction, interesting only to a few speculative, gifted minds, but their achievements are to be spread before living, practical men, and their science would fail of its highest object if it did not advance the social, moral, and intellectual progress of the whole people, aiding their industry and promoting their well-being whilst also furnishing themes of lofty intellectual thought. Their inquiries are not confined to the starry heavens nor to the deep strata of earth, but extend to all things, and seek to show the beneficent adaptation of the means to the end which pervades the whole universe, and makes it an outspoken attribute of Deity. Their office is to aid in bringing the great mechanism of the earth to the contemplation of the people, and to encourage their intellectual advancement whilst quickening their religious instincts, — for science has its religious teachings. Its God is the God of revelation, the God whose choicest blessings have been invoked upon their proceedings, and without whose Divine aid their doings will come to nought.

"On the part of the Association I again accept gratefully your proffered hospitality."

In the absence of the President, Vice-President, and General Secretary, Professor Alexis Caswell was appointed to preside at the Baltimore meeting, and Professor William M. Gillespie was chosen General Secretary.

At 1½ P. M., on Monday, May 3, Dr. I. I. Hayes delivered, in general session, a paper ".On the Practicability of Reaching the North Pole."

At 4½ P. M. of the same day, the Association listened to the "Report of the Committee on the Registration of Births, Marriages, and Deaths."

No lengthened abstract of the proceedings, scientific and executive, of the Baltimore Meeting of the Association, is necessary in this place, as they are contained in full in the papers and resolutions printed in this volume.

The Association voted to hold their next meeting at Springfield, Massachusetts, on Wednesday, the 3d of August, 1859.

The officers elected for the next meeting are, Professor STEPHEN ALEXANDER, of Princeton, N. J., President; Professor EDWARD HITCHCOCK, of Amherst, Mass., Vice-President; Professor WILLIAM CHAUVENET, of Annapolis, Md., General Secretary; and Dr. A. L. ELWYN, of Philadelphia, Treasurer. The Permanent Secretary, Professor JOSEPH LOVERING, of Cambridge, was elected for a third term, commencing with the Springfield Meeting.

During the session of the Association, the members and their ladies, besides receiving many private hospitalities, were elegantly entertained on Wednesday evening, April 28, by Hon. Thomas Swann, Mayor of Baltimore; on Thursday evening, April 29, by the Maryland Historical Society; on Friday evening, April 30, by Mrs. Lincoln Phelps; and on Monday, May 3, at the house of W. J. Albert, Esq. On Saturday afternoon, May 1, an excursion was made to Fort McHenry, by the request of the Secretary of War, and while there the members and their ladies were received with generous hospitality by Brevet Major William H. French, Commanding at the Fort. The members' tickets also admitted them to the Library and Gallery of Paintings of the Maryland Historical Society; to the Rooms of the Mercantile Library Association; to the Maryland Institute Library; to the Church Home and Infirmary, Broadway; to the Greenmount Cemetery; to the public schools of Baltimore; to the Washington Monument; to the House of Refuge; to the Medical Department of the University of Maryland; and to the Infirmary of the University of Maryland.

RESOLUTIONS ADOPTED.

Resolved, That the Acting President be requested to prepare an address to be read at the next annual meeting.

Resolved, That a Special Committee of seven be appointed by the Chair to inquire and report at this session upon the expediency of having a Committee of the Association to coöperate with Dr. Hayes in reference to an Expedition to the North Polar Sea.

Resolved, That a Special Committee of five, to be named by the Chair on Tuesday morning, be appointed to report upon the "Report of the Committee on the Registration of Births, Marriages, and Deaths."

Resolved, That the Special Committee to whom was referred the Report on Registration be empowered to publish five hundred copies of the report, if they shall think it expedient.

Resolved, That the System of Earthworks and other Ancient Remains, spread over the valley of the West and the South, recommends to the members of the Association and to the friends of science generally the subject of American Ethnology, and requires a Special Committee to investigate these curious antiquities for the purpose of reporting to this Association.

Resolved, That a Committee of seven (afterwards increased to eight) be appointed to report at the next meeting on the best method of conducting a State Geological Survey.

Resolved, That in accordance with the recommendation of Major

Lachlan, a Committee be appointed to memorialize the several States of the Union to establish systems of Meteorological Observations in the cases in which they have not already done so.

RESOLUTIONS OF THE STANDING COMMITTEE.

Resolved, That the General and Permanent Secretaries be instructed to repair to the locality chosen for the ensuing meeting, a sufficient time before the date of the meeting, and there make general arrangements with the Local Committee for the reception of the Association and the convenience of the members, and that the expenses incident thereto be paid out of the funds of the Association.

Resolved, That an appropriation, not exceeding one hundred dollars, be placed at the disposal of the Permanent Secretary, in order to carry out his recommendation, namely, that the rooms in which the Sections meet be connected by electro-telegraphic lines in order to signalize, in each room, the business or paper which is under consideration in the other rooms.

Resolved, That a complete set of the volumes of Proceedings of the Association be presented to the Baltimore Historical Society, and the Maryland Institute for the Promotion of the Mechanic Arts.

Resolved, That the Standing Committee recommend to the Association the adoption of the following additions and amendments to the Constitution: —*

Before *Rule 1st* insert the following rules, to be numbered in order: —

Rule 1st. Members of the Association shall be divided into two classes, who shall be styled respectively Members and Associate Members. Members shall consist exclusively of persons devoting themselves to scientific pursuits. Associate Members of those who take an active interest in the advancement of Science.

Rule 2d. Members only shall be eligible to the offices of the Association, to the Standing Committee, or to Committees upon purely scientific subjects, and they alone shall be entitled to vote upon the election of officers or the appointment of the Committees above specified.

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* To be acted upon at the next meeting.

Rules 1*st* and 2*d* of the present Constitution shall be stricken out.

Rule 3*d* of the Constitution shall be amended by the addition of the words, "In every case in which a person shall be nominated as a member, such nomination shall be accompanied by a written statement of the services which such person has rendered to science, and which entitle him to election."

The Standing Committee further recommend to the Association that all persons now considered as members shall remain members under the revised Constitution; but that, after the present meeting, new members shall be classified by the Standing Committee when their names are presented for election.

The Standing Committee further recommend, that wherever the word Members occurs after *Rule* 3*d* in the present Constitution, the words "and Associate Members" shall be inserted, excepting only in those cases in which such insertion shall be at variance with the amendments adopted above.

Also, that ladies may be admitted as Associate Members.

VOTES OF THANKS.

Resolved, That the thanks of the Association be returned to the LOCAL COMMITTEE, and particularly to its CHAIRMAN, as also to the SUB-COMMITTEES and their OFFICERS for their convenient arrangements to accommodate the Association, and for their unwearied exertions to promote the personal comfort of its members.

Resolved, That the thanks of the Association be tendered to the MARYLAND INSTITUTE FOR THE PROMOTION OF THE MECHANIC ARTS, for the accommodation afforded in the use of its spacious rooms.

Resolved, That the thanks of the Association be presented to the MARYLAND HISTORICAL SOCIETY, for the elegant civilities extended to its members.

Resolved, That to the generous hospitalities of the CITIZENS OF BALTIMORE we owe much of the pleasure of the meeting of the Association, and that we return our warmest thanks for the kindness which has been extended to us.

Resolved, That the thanks of the Association be offered to BREVET Major W. H. FRENCH, U. S. A., commanding officer at Fort McHenry, for the handsome manner in which he executed the instructions of the Secretary of War, and for his own cordial and courteous reception of the members.

Resolved, That the thanks of the Association be given to the several public INSTITUTES OF THE CITY OF BALTIMORE which have favored the members of the Association with invitations to visit them during the session.

Resolved, That the thanks of the Association be presented to Professor ALEXIS CASWELL, the President, for the courtesy, dignity, efficiency, and kindness with which he has presided.

Resolved, That the thanks of the Association be tendered to Professor W. M. GILLESPIE, the General Secretary, for the ability, precision, and promptness with which he has discharged the duties of that office.

Resolved, That the thanks of the Association be given to the PERMANENT SECRETARY for the attention, accuracy, and system which have contributed so much to the success of our Association.

REPORT OF THE PERMANENT SECRETARY.

The Constitution of the Association requires that the Permanent Secretary shall make an annual report to the Standing Committee (at its first Meeting) of the business of which he has had charge since the last Meeting of the Association.

The official duties of the Secretary, which have occupied him since the last Meeting at Montreal, may be classified as follows: —

1st, Correspondence for the Association; 2d, the issue of various circulars and notices; 3d, the collection of assessments; 4th, the preparation of the Montreal volume for the press, its revision, publication, and distribution, and the distribution of the proceedings generally; 5th, the care of the property of the Association, namely, its copies of printed Proceedings and its records. The number of living members of the Association at this date is 948; thirty-seven having been struck from the previous list, each of whom was in arrears for nine dollars, or in all $333, and 237 new members having been added at Montreal; making a net increase of two hundred over and above the number in the old catalogue of names printed in the tenth volume of Proceedings.

From the commencement of the Montreal Meeting to this date (the first day of the Baltimore Meeting), one thousand nine hundred and five dollars and eighty-four cents ($\$1{,}905\frac{84}{100}$) have been collected by the Secretary; mostly in payment of assessments. The arrears now due from members amount to ($1,530) fifteen hundred and thirty dollars.

The expenditures made for the Association during the same period amount to $\$2{,}050\frac{47}{100}$, the details of which are printed in full in the Montreal volume. They may be distributed as follows: —

1. For printing, binding, and illustrating, &c., the Montreal volume,	$1,257.75
2. Expenses in connection with the Montreal meeting,	125.37
3. Salary of the Secretary,	500.00
4. Shelves, &c., for volumes of Proceedings,	100.00
5. Postage, stationery, &c.,	67.35
	$2,050.47

The Association now possess six thousand volumes of printed Proceedings. These have been taken from the boxes in which they were formerly kept, and placed upon dry shelves, according to the recommendation of the Secretary in his last report; a special appropriation having been made for that object.

The Secretary has also collected and arranged the original records for the first ten meetings of the Association, and caused them to be bound together in a durable form for future reference.

JOSEPH LOVERING,
Permanent Secretary.

BALTIMORE, April 28, 1858.

CASH ACCOUNT OF THE

Dr. AMERICAN ASSOCIATION in

Clerk at Montreal	$35.00
Assistant at Montreal	10.00
Loiselle's services at Montreal	5.37
Buck's Express	17.00
Hollingsworth, for paper	222.30
Salary of Permanent Secretary	500.00
Express from St. Louis	2.38
Express	2.25
Buck's Express	5.00
Kilbourn & Mallory, for wood-cuts	32.00
Marsh, for stationery	6.50
Hollingsworth, for paper	166.73
Expenses in attending meeting	75.00
Paper for covers	6.85
Metcalf & Co., for printing	41.00
Buck's Express	8.12
Allen & Farnham, for printing	504.00
Higgins & Bradley, for binding	95.12
G. F. Lewis, for printing plates	30.00
J. Bien, for printing plates	97.50
Major & Knopp, for printing plate	22.50
Postage and discount	50.85
Index to Montreal volume	5.00
Stationery	10.00
Appropriation for arrangement of property	100.00
	$2,050.47
Balance to next account	353.59
	$2,404.06

PERMANENT SECRETARY.

Account with Joseph Lovering.	Cr.
Balance from last account	$111.35
Error in charge (19) of last account . . .	1.98
Drafts from the Treasurer	342.00
Cash from G. P. Putnam & Co.	42.89
Assessments, &c. (from 951 to 1558) . . .	1,905.84
	$2,404.06

STOCK ACCOUNT OF THE PERMANENT SECRETARY.

Volumes Distributed or Sold.

Volumes	I.	II.	III.	IV.	V.	VI.	VII.	VIII.	IX.	X.	XI.
DELIVERED TO											
Boston City Library											*
Manchester City Library *	*	*	*	*	*	*	*	*	*	*	*
Nantucket Athenæum †											*
Munroe & Co. ‡											10
John Bartlett ‡										3	3
Dudley Observatory											*
Albany State Library											*
Phil. Acad. Nat. Science †											*
Phil. Philos. Soc. †											*
Providence Athenæum †											*
Brown University											*
Yale College											*
Harvard College											*
American Academy †											*
Boston Nat. Hist. Society											*
Boston Athenæum											*
Montreal Nat. Hist. Society †											*
Super't Education, Montreal †											*
Smithsonian Institution †										*	*
Lyceum of Nat. Hist. N. Y. †										*	*
Mr. Dalrymple *	*	*	*	*	*	*	*	*	*	*	
Mr. Hollingsworth *			2		*	2	2	2	2	6	6
Taylor & Maury *											*
Mr. Whitney *	*	*	*	*	*	*	*	*	*	*	
Sold											14
To Members	1	1	2	2	2	1	9	5	9	45	523
Total	4	4	7	5	6	6	14	10	14	59	575

* Sold.

† By order of the Association.

‡ In exchange for rarer volumes.

List of European Institutions to which Copies of Volumes X. and XI. of the Proceedings of the American Association were distributed by the Permanent Secretary in 1858.

Stockholm, — Kongliga Svenska Vetenskaps Akademien.
Copenhagen, — Kongel. danske Vidensk. Selskab.
Moscow, — Société Impériale des Naturalistes.
St. Petersburg, — Académie Impériale des Sciences.
" " Kais. Russ. Mineralogische Gesellschaft.
Amsterdam, — Académie Royale des Sciences.
Haarlem, — Hollandsche Maatschappij der Wettenschappen.
Berlin, — K. P. Akademie der Wissenschaften.
Breslau, — K. L. C. Akademie der Naturforschen.
Franckfurt, — Senckenbergische Naturforschende Gesellschaft.
Göttingen, — Königl. Gesellschaft der Wissenschaften.
Munich, — K. B. Akademie der Wissenschaften.
Prag, — K. Böhm, Gesellschaft der Wissenschaften.
Stuttgart, — Verein für Vaterländische Naturkunde.
Vienna, — K. Akademie der Wissenschaften.
" K. K. Geographischen Gesellschaft.
" Geologischen Reichsanstalt.
Bern, — Allgemeine Schweizerische Gesellschaft.
Basel, — Naturforschende Gesellschaft.*
Geneve, — Société de Physique et d'Histoire Naturelle.
Neuchatel, — Société des Sciences Naturelles.
Bruxelles, — Académie Royale des Sciences, &c.
Liège, — Société Royale des Sciences.
Paris, — Institut de France.
Dijon, — Académie des Sciences, &c.
Turin, — Accademia Reale delle Scienzie.
Madrid, — Real Academia de Ciencias.
Cambridge, — Cambridge Philosophical Society.
Dublin, — Royal Irish Academy.
Edinburgh, — Royal Society.

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* Volume XI. only.
† Volumes VI. to XI. inclusive.

London, — Board of Admiralty.
" East India Company.
" Museum of Practical Geology.
" Royal Society.

BALANCE OF STOCK.

Volumes	I.	II.	III.	IV.	V.	VI.	VII.	VIII.	IX.	X.	XI.
Balance, March, 1858,	61	83	262	241	449	308	553	871	979	1003	
Received from Binders,											1527
Received from Munroe,		10									
Received from others,*.	2	6	1	1							
Total, ...	63	99	263	242	449	308	553	871	979	1003	1527
Delivered as on page 302, ...	4	4	7	5	6	6	14	10	14	59	575
Balance, ...	59	95	256	237	443	302	539	861	965	944	952
Delivered as on page 303, ...						1	1	1	1	33	34
Balance March, 1859,	59	95	256	237	443	301	538	860	964	911	918

REPORT OF THE AUDITORS.

We certify that we have examined the preceding account of the Permanent Secretary, comparing the entries with the proper vouchers and accounts, and find that it is properly set forth and correct in the several items, and that the balance due the Association ($353.56) has been carried to the next year's account.

(Signed) P. N. Lynch, D. D.,
John L. LeConte, M. D.

Baltimore, Md., May 1, 1858.

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* In exchange for volumes less rare.

REPORTS.

The Committee to whom was referred the subject of Dr. I. I. Hayes's proposed expedition to the Arctic Seas report, that

1st. The question of the open Polar Sea, its limits and character, is the most interesting of those remaining to be completely solved in Arctic geography.

2d. The statements of Dr. Hayes, surgeon to Doctor Kane's second Grinnell expedition, make it probable, that, with moderate means and appliances, this problem may be completely solved.

3. The indirect results readily obtained by such an expedition in regard to the magnetism, tides, currents, meteorology, geology, and natural history of the Arctic regions, and the peculiar phenomena of glaciers and icebergs, and the ethnology, are of themselves of such importance as to demand further research.

4th. Dr. Hayes is desirous of devoting himself to this line of exploration, in the difficulties, hardships, and dangers of which he has, when serving with the lamented Kane, had full experience.

5. Therefore, this special committee recommends to the Association the passage of the following resolution: —

Resolved, That a committee of fifteen members of the American Association be appointed to coöperate with Dr. Hayes in his efforts to organize another expedition for Arctic research.

Signed, Edward Hitchcock, *Chairman.*

CORRESPONDENCE.

Cambridge, April 19, 1858.

My Dear Sir, — I have the pleasure of acknowledging the receipt of your note of March 22d, informing me that the American Association for the Advancement of Science, had, at their late meeting held in Montreal, in August, 1857, elected me President for the ensuing year.

I desire through you to express to the Association my sincere thanks for the great and unexpected honor which they have thus conferred upon me, and at the same time my regret, that, from private considerations, as well as from those connected with my relations to the college, it will not be practicable for me to be present at the meeting in Baltimore.

As I shall not, therefore, be able to attend to the duties which would officially devolve upon me before the Association, I beg, through you, to request of them to accept my resignation of the office with which they have been pleased to honor me.

Sincerely yours,

JEFFRIES WYMAN.

Prof. JOSEPH LOVERING, *Permanent Secretary of Am. Assoc. for Adv. of Science.*

CHARLESTON, April 1, 1858.

MY DEAR SIR, — Your letter, in which you inform me that I was elected Vice-President of the Association at its last meeting in Montreal, was received several days since. A note from our friend Wyman, received about three weeks ago, had in a measure made me acquainted with the fact, as he said it would be out of his power, for certain reasons, to preside at the Baltimore meeting, and this was the first intimation that I ever received of the honor done me by the Association. For you will remember that we were abroad all last summer for the benefit of Mrs. Holbrook's health, and that we only returned to Charleston at Christmas, and of course this long absence put me entirely out of the way of receiving the news of the acts of the Association. On the receipt of friend Wyman's letter, I wrote him that it would be out of my power to be present at the Baltimore meeting, for many reasons, which I hope may be acceptable to him and to you; at the same time I requested him to make known, to the proper officer of the Association, his resignation as well as my own, as otherwise it might cause some inconvenience at Baltimore, or it might be said that we were negligent, or unmindful of the high honor done us by the Association.

Yours truly,

S. E. HOLBROOK.

Prof. JOSEPH LOVERING, *Permanent Secretary, &c.*

U. S. NAVAL ACADEMY, Annapolis, Md., April 24, 1858.

SIR, — A severe cold, from which I am but slowly recovering, renders it doubtful whether I shall be able to attend the meeting of the American Association for the Advancement of Science, in Baltimore. But even if I shall be well enough to attend, as I hope, I shall certainly be unable to perform the duties of General Secretary. Will you do me the favor, therefore, to lay this communication before the Association, as my resignation of that office, at an early period of the meeting, in order that a substitute may be provided?

Very respectfully, your obedient servant,
WILLIAM CHAUVENET.

DR. LEWIS H. STEINER, *Local Secretary.*

ROYAL ASTRONOMICAL SOCIETY, London, March 12, 1858.

SIR, — The handsome invitation of the American Association for the Advancement of Science, to the members of the R. A. S. of London, has been duly read by me to the Council, and previously made known to such Fellows of the Society as have called at their rooms since it was received. The Council direct me to return their acknowledgments for the same, and to express their wish that the Association may have an auspicious and interesting meeting on the proposed and future occasions.

I am, Sir, your obedient servant,
R. C. CARRINGTON, *Secretary.*

THOMAS SWANN, Esquire.

AMERICAN GEOGRAPHICAL AND STATISTICAL SOCIETY,
University Building, Washington Square, New York, May 1, 1858.

The Committee appointed by resolution of the Society at their meeting on Thursday, April 22, 1858, to confer with other societies, for the purpose of extending to the American Association for the Advancement of Science an invitation to hold their next meeting at New York, has found that other societies are taking measures with the same view. The Committee has, therefore, resolved to request that members of this Society, at present in Baltimore, will respectfully urge on the Association that they elect to hold a meeting in this city at an early date.

J. C. ADAMSON, *Chairman.*

WAR DEPARTMENT,
Office Explorations and Surveys, Washington, April 29, 1858.

DEAR SIR, — I take the liberty to send you a map of the territory between the Mississippi River and the Pacific Ocean, which may be interesting to your meeting, should any of the subjects treated of have reference to this country.

Very respectfully your obedient servant,
G. K. WARREN, *Lieut. of Top. Eng.*

DR. CHRISTOPHER JOHNSTON, *Secretary of Section B. of American Association.*

ROME, March 24, 1857.

MOST ILLUSTRIOUS SIR, — I have to ask of your most distinguished Excellency, if agreeable to you, to present to the illustrious Scientific Association, the accompanying copy of my memoir on *The Undulatory Motion of the Sea*, and *on the Currents of the same*; as well as my *Letter on the Harbor of Pesaro.*

The principal questions considered in my Memoir are five:

First. The transporting of matter, attributable to the waves in a high sea in case of strong winds.

Second. The inappreciable motion of transportion of the waves, when the velocity of the wind is not greater than seven or eight metres in a second of time.

Third. The transporting of matter by waves, when they have not very free development in the lower part, and with moderate wind.

Fourth. The ascertainment of the measure of depth to which the action of waves is communicated.

Fifth. The prevalence of waves over littoral currents and tides by the transporting of obstructive matter.

To admit the transportation of matter by undulatory motion is not conformable to generally adopted hydrostatic principles. Whence my propositions, comprised in the first, third, and fifth questions, cannot be explained from the theory of the mechanics of fluids. But this praised theory has been, as is well known, contradicted many times in its different branches when submitted to experiment, and more, than in others, do I find it unfortunate in that of the undulatory motion of the sea.

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[*] Sent through Lieut. M. F. Maury, and translated by Prof. J. S. Hubbard.

The view of the hypotheses (or theories, if you choose), delineated by me in the *Introduction*, seems to me to demonstrate the truth of this my assertion.

Notwithstanding this, I am convinced that this theory is based upon truth, and hence that, whenever extraordinary causes do not come in to alter the elements of its stability in the phenomenon, this will happen according to the laws of the arranged theory. And this is why, in the second question, I do not admit appreciable motion of transportation, and combat the opposite opinion of de Tenan, of Stevenson, of Paoli, and of Caligny, — quoting many facts to show their error. (§ 15.)

Whenever a cause, like that of a very strong and continued wind, comes in to alter the preëstablished system in undulatory motion, I find myself agreeing with the above-mentioned gentlemen, or rather I go further than they. That the wind in these conditions may impress upon the waves a motion of transportation upon their surface, is a matter of no doubt to me, and my convictions are deduced (as of necessity in such an argument), not from small experiments made in a study, but from facts which result upon the whole breadth of the ocean; of such facts I have collected a great many in paragaph 12. Reason tells me that a liquid mass in equilibrium, acted upon by any force, intermittent or constant on one side, cannot be moved in the opposite direction by an intermittent or constant motion of translation. And if to any one this reasoning and all the long series of facts collected in the proposition do not suffice to convince of the raising power which a given force of wind exerts upon the liquid mass, I shall require of him that he give me satisfactory explanation of those extravagant transportings to which ships are subjected, in comparison with the effect of no currents, or those well known. (§ 13.) The facts exist, the necessity is perceived of an explanation of these hindered waves, but no one until now has occupied himself sufficiently with the subject. With the materials collected in paragraph 12, it has been easy for me to compose the succeeding paragraph 13; and the case of the vessel *Winchester*, and the other of the frigate *Venus*, contained in it, are to me a most clear elucidation of the cause of the mentioned enormous transportation. The number of cases might be greatly enlarged, but in a treatise which bears the title of *Hints*, I think enough has been given upon this question in the two above-named paragraphs, 12 and 13. Inasmuch as these my propositions which comprehend the first question, departing somewhat from the theoretical laws of undulatory motion, point out the potent causes which in certain cases act to alter them, I think that in these exceptional cases they may *at least* be received among those sufficiently probable, and thus deserving particular consideration.

I pass to the proposition which forms the subject of the third question. Paragraph 19 indicates the phenomena which should occur when the waves strike below upon the depths of the ocean, the 20th describes them, and the 21st demonstrates their existence in a series of very conclusive facts. The hypothesis, that, in waves created by the wind, there may happen near the shore that which does happen in the tide waves (§ 21, p. 23 to 46), seems to me to have such a weight of analogy and of probability as to compel me to admit one of two consequences, either that this hypothesis is the expression of that which really happens in nature, or that the truth, whatever it may be, should be found in one parallel to this, and so near as to admit of some modes of expression common to both. Whence, although my proposition could not have any other explanation, yet it could not be taken into calculation without giving, on the authority of Sir J. F. W. Herschel, a very limited idea of the value and importance of the hypothesis. But, following the history of the facts collected in pages 46 to 51, I think that the hypothesis brought forward by me reduces itself to certainty. Nevertheless, I conclude that it will do no harm to collect a greater number of documents *to render my own conviction universal.* (p. 51.) This proposition, then, if I mistake not, may be counted among those which on my part are sufficiently *proven.*

In the remainder of this paragraph, 21 (that is, from page 51 to nearly the whole of 56), are prepared the elements which must serve me in sustaining the propositions which form the fifth question. But at first entrance into this field, I stop to show the volume and velocity of the waves, the depth to which their action is communicated, and how great is their power. The shores of islands and continents have a physiognomy impressed on them by the liquid moving mass. Some, of most distinguished name, maintain that littoral currents or tides produce this effect. I, on the other hand, believe that it is the work of the waves. Paragraphs 22, 23, and 24, bringing in a long series of facts in point, make manifest the greatness of the waves, show their powerful steps, prove the immensity of their action, and at such a depth as is not hitherto acknowledged in the schools, so that it seems to me impossible to doubt that these are the very efficacious instruments with which nature cuts away or fills up the maritime shores.

With this first series of facts, and so far as relevant, I come to treat of the doctrine of Montanari, passing to the *second part* of my *memoir.* Considering this doctrine in the material relation, a small number of the facts already collected by me will suffice to show its erroneousness; but it has been sustained by a very strong moral force, that is by names most illustrious in the science of fluids; whence I have no fear, though compelled certainly to great circumspection. And hence I endeavor to introduce gradually the treatment in paragraphs 25 to 35, and at the outset I collect new facts against the defenders of this doctrine, to such an extent, that, in paragraph 37, it seems to me to be completely thrown down under the force of these facts, and under the power of reason, and I see rising solid above its ruins the new laws of sand-deposit, whose foundations were already laid by Castelli, Boscovich, and Fazio, but till now covered by the prevailing theory of Montanari. This proposition, if I may be permitted so to say, I think worthy to be placed among the axioms.

The new theory exposed by me, and established by so many arguments, fills me, I confess, with the highest confidence in its exactness; I have since seen the excellent *Considerations on the extending of the Shores and on the Sand-deposits of Harbors in the Adriatic, applied to the establishment of a Harbor in the Roads of Pelusium,* by the illustrious Professor Paleocapa. It is easy to see that this, contradicting the two principal propositions which serve as the base of all this edifice constructed by me, would place me under the necessity either of renouncing the authority of all the facts recorded in my memoir, or of adding thereto an *appendix,* which, based upon the facts mentioned, and others passed over for brevity, shall all the more strongly prove true this my conviction. After mature reflection, I have come to the second result, and it seems to me that a more accurate examination of the question has rather given, in greater relief, that which I had previously gone over in explanation; this has induced me to apply my theory, moreover, to the harbor of Pelusium, where by chance I find an agreement with the above-mentioned Professor Paleocapa.

My treatise *On the Harbor-canal of Pesaro* develops the project, proposed by me and approved by our noble Council of Arts, for giving to that city a new harbor.

The necessity for having the greatest possible depth at the mouth of a harbor-canal and in the banks which crown it, suggests the practice of arming the mouth of the stream with strong piles or rocks; and this practice has always been in full force in Italy and elsewhere. But this system has its defects, and two very grave ones; the first, of pressing too hard the sections of the mouth, and the other of producing very rapid advance of the coast, hence notably prolonging the lines by means of repeated extension of the moles.

The desire of eliminating from this system its principal defects suggested to me the expedient of stopping the usual greater extension of the mole on the side of the prevalent winds for about 150 metres, and furnishing with rocks the shore of the mouth on the same side with these winds; an expedient which I now propose for the new harbor of Pesaro, and which I think preferable to all others for any harbor-canal.

I take this occasion to have the honor of signing myself, with the highest esteem,

Most distinguished Sir,
Your most humble obedient servant,
ALESSANDRO CIALDI.

INVITATIONS.

MECHANICAL BAKERY COMPANY, Baltimore City, April 27, 1858.

SIR, — I have been instructed by the Board of Directors of this Company to request you to invite the president and members of the American Association for the Advancement of Science, about to assemble in this city, to visit and inspect the Mechanical Bakery, where there is in practical operation the Automatic Oven and Kneading Machine, invented by Mr. H. Berdan, of New York, which the Directors are confident will present objects of great interest to the intelligent and scientific body of that Association. Be pleased to inform me the day and hour that will best suit their convenience to visit us.

Very respectfully, your obedient servant,
WILLIAM NORRIS, *President Mechanical Bakery Co.*

HON. THOMAS SWANN, *Chairman of the Local Committee of the American Association for the Advancement of Science.*

FORT MCHENRY, Md., April 20, 1858.

DEAR SIR, — I have the honor to acknowledge your note of the 19th inst. It will give me great pleasure to give the Association an opportunity to see a light artillery drill on the first of May.

Your polite invitation to join in the excursion to Fort Carroll has been extended to the officers at this post, who will be most happy to accept it.

With great respect, your obedient servant,
WILLIAM W. FRENCH, Brevet-Major, U. S. A., Commanding.

Hon. THOMAS SWANN, *Chairman of the Local Committee, &c.*

OFFICE OF COMMISSIONERS OF PUBLIC SCHOOLS, Baltimore, April 21, 1858.

DEAR SIR, — I have the honor to inform you, that by a vote of the Board of Commissioners of Public Schools, an invitation is extended to the members of the approaching convention of the Association for the Advancement of Science to visit any of the Public Schools, at such times as may suit their convenience severally, during their stay in the city.

I remain, dear sir, very respectfully yours,
GEORGE N. EATON, *President of Board.*

Hon. THOMAS SWANN, *Chairman of the Local Committee, &c.*

BALTIMORE, April 23, 1858.

DEAR SIR, — Permit me through you to extend to the members of the American Association for the Advancement of Science at their approaching session an invitation to visit, at such time as may suit their convenience, "The Church Home and Infirmary" on Broadway, now thoroughly organized as a charity hospital.

Very respectfully,
F. DONALDSON, M. D.

Dr. LEWIS H. STEINER, *Local Secretary, &c.*

BALTIMORE, May 3, 1858.

You will please present an invitation to the members of the Association to attend Ahrend's Quintette Concert, at the New Assembly Rooms, corner of Hanover and Lombard streets, to-morrow evening, at eight o'clock.

The membership ticket will admit a gentleman and a lady, on being presented at the door of the Concert room.

With respect, I remain yours, &c.,
C. LENTBECHER.

To the Local Secretary of the American Association for the Advancement of Science.

EXCHANGE READING ROOMS, Baltimore, April 28, 1858.

DEAR SIR, — You will please inform the members of the American Association for the Advancement of Science, now in session in our city, that these rooms are free for them to visit at all hours, and their acceptance of the invitation in this general way will be very gratifying to

Yours, very truly,
GEORGE U. PORTER.

Dr. LEWIS H. STEINER, *Local Secretary, &c.*

BALTIMORE, April 23, 1858.

DEAR SIR, — The Board of Managers of the "House of Refuge" most cordially invite the members of the American Association for the Advancement of Science to an examination of their institution, on any day that may be most convenient, only requesting that one or two days' notice may be given to the Executive Committee of the Board, of the time of the intended visit.

Be so kind as to notify me of the acceptance of the invitation.

Respectfully,
JOHN I. GRAVES, *Secretary, in behalf of the Board.*

Dr. LEWIS H. STEINER, *Local Secretary, &c.*

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ERRATA.

Page 29, last line but one, for *three one hundredths* read *three one thousandths.*

Page 30, bottom, for $\frac{\Delta T d a}{a}$ read $-\frac{\Delta T d a}{a}$.

Page 81, twelfth and twentieth lines from top, for *Diagram* 1 read *Diagram* 3.

Page 84, second line from top, for *Diagram* 3 read *Diagram* 4.

Page 84, seventh line from top, for *Diagram* 4 read *Diagram* 2.

Page 84, thirteenth line from top, for *Diagrams* 4 *and* 5 read *Diagrams* 2 *and* 5.

Page 215, second line from top, for *Bakarellia* read *Bakevellia.*

Page 269, sixth line from bottom, for *patri* read *putri.*

Page 269, seventh line from bottom, for *defect* read *deficit.*

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